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Effects of Temporary In-Work Benefits for Welfare Recipients: Examination of the Australian Working Credit Programme*

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Abstract

We investigate the impact of 'Working Credit', a nationally-implemented programme which created increased incentives for welfare recipients to undertake temporary work. Highlighting the difficulties in identifying programme effects in the absence of a randomised controlled trial or a natural experiment, we produce estimates of impacts under alternative identifying assumptions and also undertake various robustness checks. Unconditional and regression-adjusted difference-in-difference estimates suggest that the introduction of the Working Credit programme increased employment rates, earnings and exits for those on income support, but matching methods and various robustness checks provide conflicting evidence on the impact on movements from welfare to work for unemployment benefit recipients. Moreover, estimated effects on earnings

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while on benefits are sensitive to identifying assumptions. Notwithstanding our inability to conclusively identify causal effects of the programme, we note that our findings are broadly consistent with the incentive effects of the programme, with recipients making use of the credits to increase earnings while on benefits, but not increasing movements off welfare.

Policy points

- The Australian Working Credit is akin to earned income tax credits that exist in other countries – the difference being that it is temporary rather than permanent.
- Using several evaluation strategies, we find that the Working Credit increased employment rates and earnings for individuals on income support.
- Back-of-the-envelope estimates suggest that, on a cost-per-job basis, the Working Credit compares favourably with existing labour market programmes.
- The difficulty of evaluating the impact of the Working Credit illustrates the importance of incorporating rigorous evaluation into new programmes.

I. Introduction

In 2003, Australia introduced Working Credit, a programme that aims to encourage working-age welfare recipients (particularly those with long spells on welfare) to take up paid work. Through this initiative, Working Credits are accumulated during periods in which working-age recipients have little or no earnings. These credits are used when they commence a job to allow the retention of welfare benefits to temporarily supplement their earnings. Accumulation of Working Credits is, in essence, proportional to current-spell duration on benefits (although credits can be used and then reacquired with additional time on payments). It is therefore worth little to those who have only just gone onto payments, and is in general largest for those who have been on payments the longest. This is consistent with a goal of targeting resources towards reducing long-term unemployment and welfare reliance.

The Working Credit programme nests within the broad range of 'activation' programmes that have been implemented internationally since the mid-1990s to promote movement of welfare recipients into employment. Activation policies are widespread and varied, but increasingly they tend to emphasise 'conditionality' – imposing requirements on welfare recipients to seek work or improve their readiness for work.[1] Examples include elements

[1]Eichhorst and Konle-Seidl, 2008.

of the UK New Deal, the German Hartz IV reforms, the Australian Mutual Obligation Initiative and various policies introduced in France, Denmark, the Netherlands and Switzerland in the last decade-and-a-half.[2] In contrast to these types of compulsion-oriented activation schemes, however, Working Credit provides financial incentives designed to make work more attractive to welfare recipients.

In this respect, by providing in-work benefits targeted at welfare recipients, Working Credit bears some resemblance to the broad-based earned income tax credit programmes employed in the US and the UK. Indeed, the stated policy objectives are similar to those of broader tax credit programmes, being to promote employment, reduce welfare reliance and increase incomes over the long term of welfare recipients. However, there are important differences between Working Credit and earned income tax credit programmes. Most importantly, Working Credit is designed as a temporary credit, for the period when individuals move from welfare into work. This significantly lowers its cost, but, equally, it raises questions about its effectiveness in promoting employment and exit from welfare. Evaluations of earned income tax credit policies have generally concluded that they boost participation rates, hours and earnings for those eligible to receive credits, but it cannot be assumed that these findings would translate to a temporary and more targeted programme such as Working Credit.[3]

The Working Credit initiative also has particular commonalities with the Canadian Self-Sufficiency Project (SSP), a trial programme that offered a temporary earnings supplement to selected single-parent families receiving welfare between November 1992 and March 1995. To collect the supplement, available for up to three years, a single parent had to work full-time and leave income assistance. The supplement roughly doubled the earnings of many low-wage workers. Michalopoulos et al. (2002) find substantial positive impacts of the programme on employment and earnings outcomes over the first five years after assignment to treatment, although Card and Hyslop (2005) find no long-term impacts on employment and welfare participation. In being a targeted and temporary in-work benefit, SSP has some similarities with Working Credit, although it was much more highly targeted on a narrow subset of welfare recipients, and was larger in value and longer in duration for beneficiaries, than is Working Credit.

[2]Lalive, van Ours and Zweimüller, 2005; Andersen and Svarer, 2007; OECD, 2007; Eichhorst, Grienberger-Zingerle and Konle-Seidl, 2008; Kvist, Pedersen and Koehler, 2008.

[3]Reviews of the US Earned Income Tax Credit literature include Hoffman and Seidman (2002), Meyer and Holtz-Eakin (2002), Hotz and Scholz (2003) and Eissa and Hoynes (2006). Studies of the UK earned income tax credit (variously known as the Family Income Supplement, the Family Credit, the Working Families' Tax Credit and the Working Tax Credit) include Gregg, Johnson and Reed (1999), Blundell et al. (2000), Paull, Walker and Zhu (2000), Brewer et al. (2003), Gregg and Harkness (2003), Francesconi and van der Klaauw (2007) and Leigh (2007).

Given the differences between Working Credit and the policies and programmes that have been subject to evaluation internationally, the existing findings are at best only suggestive of the impact of Working Credit. We therefore examine, in this study, the effectiveness of the Working Credit initiative in achieving increased economic participation and self-reliance among working-age welfare recipients. Our study uses administrative data, which has the advantage that our sample is very large, but the limitation that we can only observe employment and total earnings, not hours worked or hourly wage rates. The duration of our data further limits us to looking at relatively short-term outcomes (around one year), thereby precluding consideration of enduring effects on employment patterns.

Importantly, the constraints imposed by the manner of implementation of the programme mean that we are unable to conclusively identify causal effects of the programme on outcomes measured in the data. Specifically, the programme was implemented nationwide in September 2003 and applies to almost all working-age welfare recipients, meaning we do not have a natural experiment. Our empirical strategies therefore rely on the availability of data prior to implementation of the programme and on differences in incentive effects across different welfare recipients – in particular, incentive effects are greater for those with longer spell durations. These strategies require identifying assumptions that are unlikely to (completely) hold, leading to potential biases in estimates of uncertain direction and magnitude. However, by exploring sensitivity of results to alternative identifying assumptions and conducting various robustness checks, we are able to obtain strong indications of some of the effects of the programme.

We therefore present descriptive information on differences in recipient behaviour before and after introduction of the programme, and then use several different research designs to separate the effects of the Working Credit from the effects of prevailing economic conditions and the duration that a recipient is on welfare. Before–after comparisons, unconditional differences-in-differences and regression-adjusted differences-in-differences all suggest that the introduction of the Working Credit increased employment rates, earnings and exits for those on welfare. However, estimation of programme effects using matching methods indicates that the positive effect found on exits is spurious for unemployment benefit recipients. In short, while the programme appears to be successful in increasing employment of unemployment benefit recipients while on benefits, it does not appear to achieve its stated objective of moving more people off welfare. This is perhaps unsurprising, given the nature of the incentive effects created by the programme. Various robustness checks are undertaken which together support the conclusion that the programme increased earnings while on benefits, but had ambiguous or variable effects on exit rates.

The remainder of the paper is structured as follows. We outline the structure of the programme in Section II, followed by our empirical strategies in Section III. We discuss the data used and provide descriptive statistics on Working Credit balances in Section IV. Evaluation results are then presented, from unconditional difference-in-difference analysis in Section V and from regression-adjusted and matched analyses in Section VI. Section VII outlines robustness checks, including estimating with an alternative definition of the treatment group, examining effects of the programme on the behaviour of short-duration income support recipients, examining sensitivity to the exclusion of welfare churners, and investigating potential confounding effects of changes in macroeconomic conditions over this period. Section VIII concludes.

II. The Working Credit programme

Introduced on 20 September 2003, the Working Credit programme is open to most workforce-age income support (welfare) recipients. This includes all people below the official retirement age who are in receipt of unemployment benefits (Newstart Allowance or Youth Allowance (job seeker)), the Disability Support Pension (DSP), lone-parent benefits (Parenting Payment Single (PPS)) or partnered-parent benefits (Parenting Payment Partnered (PPP)). The programme allows people with accrued credits to earn additional income without reducing their benefit entitlement. Credits are accrued in fortnights in which earnings are less than \$48, up to the maximum Working Credit balance of 1,000. Credits are 'depleted' or 'used up' when earnings exceed the applicable income test 'free area' of the payment type. That is, subject to the Working Credit balance remaining between 0 and 1,000, if earnings E are less than \$48, $(48 - E)$ credits are accrued, while if earnings are greater than the free area F, $(E - F)$ credits are depleted. The free area is the amount of fortnightly earnings a recipient can have before benefit entitlement reduces (in the absence of the Working Credit programme). It is \$62 for allowances such as unemployment benefits and PPP, while for pensions such as DSP and PPS, it is \$122 for single people and \$216 for couples, with each dependent child further increasing the free area by \$24.60.[4] Working Credit balances are preserved for 12 months after exit from income support payments. Note that, on 20 September 2003, all welfare recipients, irrespective of spell duration, had a zero Working Credit balance and only began accumulating credits (at a maximum rate of 48 per fortnight) from that point in time.

[4]Note, however, that the Working Credit programme distinguishes labour market earnings from other non-welfare income, with accruals depending on all non-welfare income but depletions depending only on earnings. We have ignored this distinction because it has little practical significance: few welfare recipients report non-welfare income other than earnings.

FIGURE 1

Working Credit balance for a hypothetical income support spell

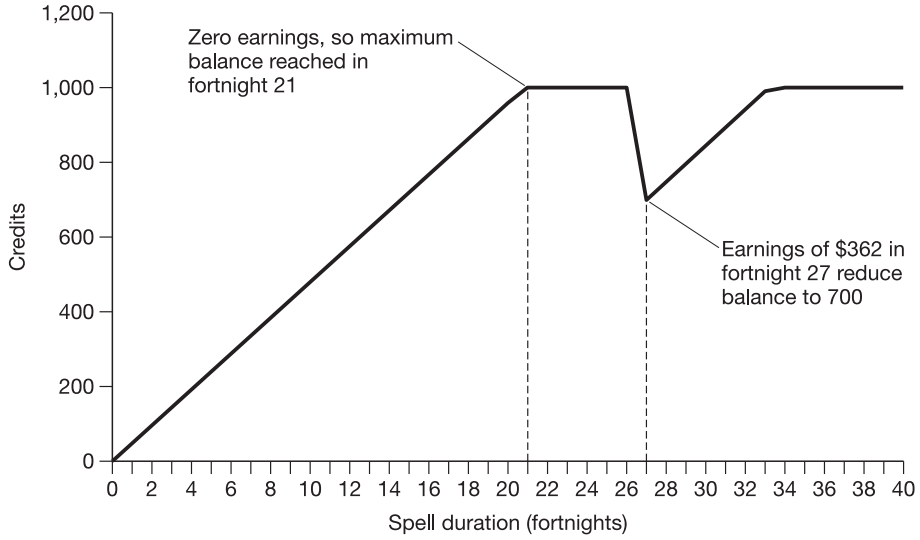


Figure 1 provides an illustration of the operation of the programme for a hypothetical income support spell (assumed to be right-censored at 40 fortnights). No earnings are reported in the first year of the spell, so that 48 credits are accrued in each fortnight until the 21st fortnight, when the maximum permissible balance of 1,000 is reached. In the 27th fortnight, the recipient reports earnings of $362, which reduces the Working Credit balance to 700 in that fortnight – that is, the recipient depletes 300 credits and does not experience a reduction in the benefit received in that fortnight. No further earnings are reported and the maximum Working Credit balance is reached again in the 34th fortnight.

Superficially, the incentive effects of the programme are clear. If it is assumed recipients consider only the current fortnight's income situation in deciding on employment participation, the presence of a positive Working Credit balance unambiguously increases incentives for employment. However, there is a dynamic dimension to incentive effects, which is perhaps best understood by interpreting Working Credit as a scheme to allow recipients to 'save' part (up to $48) of the income test free area each fortnight. For those with a Working Credit balance less than 952, the opportunity cost of the first $48 of earnings (abstracting from disutility of work) is no longer zero: it is 48 credits that allow higher income at some future date. This could in theory diminish the incentive to take up employment for individuals with Working Credit balances less than 1,000 – and could also in principle encourage cycling into and out of employment (all the while remaining on benefits) so as to maximise both usage of credits

and welfare benefits. However, given that only part of the income test free area can be saved, and that a zero interest rate applies to such savings, this incentive effect seems unlikely to be important in practice for most individuals. For example, compared with 'employment churning', total income would be higher simply by remaining in employment in every fortnight and using up all of the income test free area, which is at least $62. Moreover, such flexible employment opportunities for moving into and out of work would seem to be relatively rare, especially for low-skilled workers, who make up the bulk of unemployment beneficiaries.

III. Identification issues and empirical strategy

1. Approach

The study uses administrative data on income support recipients and takes a multifaceted approach in terms of both the types of effects investigated and the methods employed. Our examination of the incentive effects of the Working Credit programme suggests that its potential effects include:

(a) increasing the take-up and rate of employment among income support recipients while on income support;
(b) increasing the level of earnings among income support recipients while on income support;
(c) increasing the rate of exits from income support payments via increased employment of income support recipients (which can occur if part-time or temporary work acts as a stepping stone to more enduring and substantial employment);
(d) increasing the extent of employment 'churn'. Since Working Credit balances cease to accumulate when they reach 1,000 (which can occur after 42 weeks with zero earnings), the programme may create an incentive for income support recipients whose Working Credit balance is 1,000 to get a job, run down their Working Credit balance to zero and then leave employment, all the while remaining on income support. Such a strategy would allow an individual to maximise his/her gains from the Working Credit programme, potentially benefiting by more than $1,000 per year compared with no employment (although this is at least $300 less than could be received by simply earning the income test free amount each and every fortnight).

In this paper, we examine the first three potential effects. We also describe Working Credit balances and the depletion of Working Credits of eligible income support recipients, including examination of differences by payment type and other recipient characteristics. The fourth potential effect of the programme (d) is clearly of interest, but we do not investigate the

extent of employment (or welfare) churning, or other longer-term effects of the Working Credit programme, because the data available do not permit this. The data set available to us (discussed in Section IV) ends only 15 months after the programme became fully operational (in the sense that the maximum Working Credit balance could be reached). It is therefore not possible to investigate long-term effects, or even intermediate effects for a sufficiently large sample.[5]

Absent a natural experiment, the approach we adopt to examine the effects of Working Credits is based on two key features of the programme. First, the Working Credit programme did not involve replacement of an existing (similar) programme, so the period immediately prior to its introduction can be used to assist in inferring outcomes in the absence of the programme. Comparison of behaviour before and after the introduction of Working Credit can therefore provide information on the effects of the programme.

The second feature of the programme that we exploit is that potential Working Credit balances are increasing in spell duration (reaching the maximum balance of 1,000 only after 21 fortnights with no earnings), implying the potential benefits of Working Credits will in general be increasing in spell duration. Because short-term recipients receive only a small Working Credit and long-term recipients receive a large Working Credit, we can potentially use short-term recipients as a control group and long-term recipients as a treatment group.[6]

This dynamic feature of the programme is illustrated by Figure 2, which shows fortnightly 'participation' tax rates as a function of spell duration for two employment cases, as at July 2004. The graphs show the effective average tax rates on earnings for a recipient who takes on minimum-wage employment of 20 hours per week for the fortnight and on a recipient who takes up full-time employment (38 hours per week) at the minimum wage.[7] The tax rates are calculated as the sum of fortnightly income taxes and forgone welfare benefits as a percentage of gross fortnightly earnings plus the welfare benefit for a single adult unemployment benefit recipient without any earnings.[8] Figure 2 shows that participation tax rates are decreasing in

[5]Unfortunately, our requests to the government employment department for access to additional administrative data to examine longer-term effects were denied.

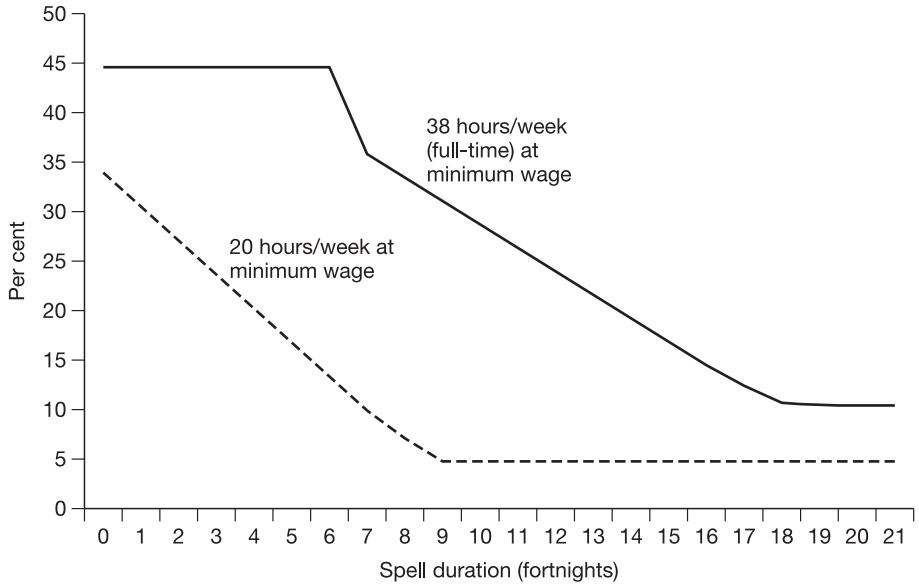
[6]The near-universality of the programme, applying to almost all income support recipients, means we do not have available a suitable control group of people ineligible for the programme. The main ineligible group is full-time students receiving benefits, who are clearly not an appropriate control group.

[7]Note that these tax rates apply only for that fortnight's earnings, since participation tax rates will rise once Working Credits are depleted.

[8]The minimum wage was $467.40 for a 38-hour week in July 2004 and the single adult unemployment benefit rate inclusive of maximum rent assistance was $242.30 per fortnight. Income tax is calculated as the minimum amount compulsorily withheld from the employee's pay by the employer (see http://www.ato.gov.au/content/downloads/n1005-05-2004.pdf), which would be applicable in most cases.

FIGURE 2

Fortnightly 'participation' tax rates as a function of duration of current spell (assuming no earnings in the spell)



spell duration (assuming no earnings), reaching the lowest point of 4.7 per cent at fortnight 9 for the part-time job and reaching the lowest point of 10.4 per cent at fortnight 19 for the full-time job.

Taking both the above two features into account, programme effects may be estimated using difference-in-difference estimators, comparing the difference in the outcome measures in the period before the programme was introduced (the 'before' period) and the period after the programme was introduced (the 'after' period) for long-term recipients with the same difference for short-term recipients, i.e.

$$\text{Policy effect on outcome } Y = \{Y(\text{after,long-term}) - Y(\text{before,long-term})\} - \{Y(\text{after,short-term}) - Y(\text{before,short-term})\}.$$

This approach controls for all other changes over time between the before and after periods that could affect outcomes, on the assumption that these changes affect short-term and long-term recipients in the same way.

We define 'short-term' as 'spell durations of six fortnights or less' and adopt two definitions of 'long-term': 'spell durations of 21–26 fortnights' and 'spell durations of 14–20 fortnights'. The 21–26-fortnight interval is one in which many individuals are likely to have the maximum Working Credit balance. People in the 14–20-fortnight spell duration category could not have reached the maximum Working Credit balance, but would certainly tend to

have higher Working Credit balances than those in the short-term group. Compared with the 21–26-fortnight definition of long-term, this definition has the advantage of comparing individuals who are more similar. That is, people in the 1–6-fortnight spell duration range (the control group) are likely to be more similar to a treatment group defined as those in the 14–20-fortnight spell duration range than to a treatment group defined as those in the 21–26-fortnight range.

In addition to producing 'before–after' comparisons and 'unconditional' difference-in-difference estimates, we also employ difference-in-difference estimators that attempt to control for observed differences between the 'treatment' and 'control' groups. These comprise both regression models and matched difference-in-difference estimators. Regression models are estimated on the outcome of interest on the full sample (in the before and after periods) and include a 'post Working Credit introduction' indicator variable interacted with a 'long-term recipient' indicator variable. The matched difference-in-difference estimator compares individuals matched on characteristics available in the data we use, including age, location, local region and income support receipt history. It thereby controls for differences in the composition of recipients across the four comparison groups in terms of observed characteristics without imposing the common support assumption required by the regression models.

Note that our approach involves comparing one set of people in the 'after' period with *different* people in the 'before' period – that is, in general, we do not examine the same person before and after the introduction of the programme. Our analysis focuses on all individuals in receipt of unemployment benefits and women in receipt of the two main parenting payments, PPS and PPP. We focus on these income support categories because they are the groups of recipients that – *ex ante* – one would expect to be most affected by the Working Credit. This is particularly true of unemployment benefit recipients, who not only have a greater attachment to the labour market than recipients of other allowances and pensions have,[9] but were also notified more regularly about their Working Credit balances than recipients of other payments.[10]

Importantly, the 2001 to 2005 period that we examine was entirely free of any other policy changes in relation to unemployment benefit recipients. In

[9]As evidence of the higher degree of labour market attachment among unemployment benefit recipients, we find that 8–9 per cent of unemployment benefit recipients deplete Working Credit balances in any given fortnight (shown in Table 3), whereas for other payment types only 4–7 per cent of recipients deplete balances.

[10]The claim form that must be lodged fortnightly by unemployment benefit recipients is pre-printed with the recipient's Working Credit balance. This would tend to raise awareness of the programme amongst this group of recipients, and for that reason they might be expected to be more responsive to the policy than other income support recipients, who do not need to lodge fortnightly claim forms. Indeed, some recipients, such as most Disability Support Pension recipients, received no information about Working Credit balances.

the case of PPS, however, coincident policy changes make it more difficult to discern the precise impact of the introduction of Working Credit. Most notably, the government changed the PPS income test on 20 September 2003 from an annual to a fortnightly income test. Associated with this change were more onerous income reporting requirements for many of these recipients. These changes have the potential to impact not only labour supply, but also earnings (as reported in the administrative data). A further policy change at the time of introduction of Working Credit was the extension to PPS of a rule known as the 'six-fortnight nil rate rule'. This rule has been in place for allowances (including unemployment benefits and PPP) for many years, but was only introduced for other payments on 20 September 2003. The rule provides that a person can go off income support for up to six fortnights and come back onto payments without going through the re-application process. This also may have affected labour supply. For example, recipients of these payments may have been more likely to exit payments for employment given the knowledge that they could easily return within six fortnights if their new job did not work out.

In addition to our analysis for the full working-age population, we also focus on male and female unemployment benefit recipients in the prime 25–44-years age range. These are recipients with a particularly strong attachment to the labour market and are therefore particularly likely to have obtained employment in the event of exit from payments. Note also that prime-aged women are a demographic group generally found to have high labour supply elasticities, and are therefore potentially more responsive to incentives created by Working Credits.

In defining the 'before' and 'after' periods appropriate to our analysis, we need to take into account two factors. First, it is useful (although not crucial) to define windows that span the full year, so as to take account of seasonal factors. More importantly, it is necessary to define an 'after' window that includes a period in which eligible income support recipients have had the chance to build up a maximum balance. Although the Working Credit programme came into effect on 20 September 2003, all individuals began with zero balances on that date. It was only 42 weeks later – in the first fortnight of July 2004 – that income support recipients could potentially have accumulated the maximum Working Credit balance of 1,000.

Our analysis correspondingly takes into account the lower potential for programme impacts in the period up to July 2004. For the unconditional and matched difference-in-difference analyses, this is achieved by excluding the 'build-up' period. Thus, we define the 'before' period to be July 2002 to June 2003 and the 'after' period to be July 2004 to June 2005. For the regression models, we take account of the build-up period by including explanatory variables capturing programme effects that distinguish the period from September 2003 to July 2004 from the post-July-2004 period.

2. Identification problems

Crucially, the approach described above requires the assumptions that the programme has negligible impacts on short-term recipients and – more importantly – that the compositions of the short-term and long-term recipient groups are not themselves affected by the programme. However, the mechanical properties of the programme do in fact act to alter the composition of the treatment and control groups. Specifically, some recipients who move into employment will remain on benefits longer under the Working Credit programme than they otherwise would due to depletion of credits that leaves them eligible for at least part-payment. This effect is likely to be reasonably small because, over this period, fortnightly earnings needed to exceed $600 (and in many cases exceed much more than $600) before a recipient became completely ineligible for benefits. That is, a recipient was eligible for at least part-payment if earnings were less than $600. This means that spell durations are only ever extended if fortnightly earnings are at least $600. Moreover, it is not possible for this mechanical property of the Working Credit programme to increase spell durations by more than two fortnights. For example, earning the minimum amount to disqualify oneself from unemployment benefits in the absence of Working Credit ($600), 1,000 credits would be exhausted within two fortnights. At higher earnings, credits would be exhausted more quickly, potentially within one fortnight.

Increases in spell duration due to the mechanical properties of the programme can occur at all durations, although the potential effects are greater for longer-duration recipients, who will tend to have higher Working Credit balances. The composition of the treatment and control groups may also be altered due to *behavioural* effects of the programme. For example, if the programme causes more people to exit payments before the 21st fortnight, estimates based on the 21–26-fortnight treatment group may be underestimates of positive impacts of the programme. Essentially, the duration of spells is endogenous to the introduction of the policy, for both these 'mechanical' and behavioural reasons. The identification strategy, which defines treatment and control groups based on spell durations, will therefore potentially lead to biased estimates. The net effect on programme impact estimates is, however, ambiguous, because some of the effects are positive and some are negative.

Given the data available and the universal nature of implementation, there is no clear remedy for this identification problem. We are therefore not able to produce conclusive evidence of the effects of the programme on employment participation. However, by examining the actual changes in behaviour of each spell duration group, by investigating sensitivity of difference-in-difference estimates to alternative definitions of the treatment

group and alternative methods for controlling for other factors, and by undertaking robustness checks (in Section VII), we are able to obtain a strong sense of at least some of the likely effects of the programme.

Several other issues for identification of programme effects also arise given our data and empirical approach, all of which we attempt to address. First, as noted above, the behaviour of short-duration recipients may be affected by the programme: there is an incentive to save credits that did not exist prior to the introduction of Working Credit, tending to reduce earnings and exit; but also, even after only a few weeks on benefits, Working Credits can be used and so could increase employment incentives for short-duration recipients. Effects in either direction are likely to be minimal, but we address these potential behavioural effects on short-duration recipients in two ways. First, using the 14–20-fortnight group as the treatment group addresses the concern that saving incentives diminish employment incentives, since these recipients have the same saving incentive as short-duration recipients – that is, both groups of recipients are able to 'save' 48 credits each fortnight. Second, a robustness check in Section VII addresses this concern by explicitly examining impacts of the programme on short-duration recipients.

A second potential issue, closely related to the first, is that Working Credit could increase employment churning, which may in turn translate to increased *welfare* churning. This would lead to compositional change, most particularly to the control group. Increased welfare churning will tend to create upward bias in estimated programme impacts, since people who 'churn' because of the programme will be relatively unlikely to have earnings or to exit payments early in the spell, when their Working Credit balances are very low. To mitigate the potential for this bias, we treat periods off payments of up to six consecutive weeks as continuations of the same spell. Thus, no individuals in the 1–6-fortnights spell duration control group have been on payments within the six weeks prior to commencement of their current spell. This reduces the likelihood that individuals in the control group in the 'after' period are churners who only exited the previous payment spell because of the Working Credit programme. Furthermore, estimated effects on exit will not reflect simply an increase in very short-term exit followed by re-entry onto payments, since the exit must be sustained for over six weeks before it is classified as such. (In the event that more than six weeks is spent off payments, exit is deemed to occur at the time the individual was last observed on payments, not at the expiration of six weeks after that point in time.) In Section VII, as a further robustness check, we also examine results when all people on benefits at any time in the three months prior to commencement of the current spell are excluded from the analysis.

The final identification issue concerns the decline in the national unemployment rate over the period examined (2001 to 2005). This may have

affected short-duration and long-duration recipients differentially, causing difference-in-difference estimates to be biased. We investigate the potential direction and magnitude of this bias by conducting 'placebo' tests in the period prior to introduction of Working Credit, when unemployment was similarly declining. That is, we define artificial 'before' and 'after' periods in the period prior to September 2003, and then produce difference-in-difference estimates over these two periods. Significant estimates imply short-duration and long-duration recipients are differentially affected by declines in the unemployment rate.

IV. Data and sample selection

To investigate the impact of the Working Credit programme, we use de-identified payments administration data produced by the Australian government Department of Families, Housing, Communities and Indigenous Affairs. The data set comprises all fortnightly payment records over the period January 1995 to September 2005 of a 10 per cent random sample of individuals who received an income support payment at some stage in that period. A separate record is generated for an individual in every fortnight in the period in which an income support payment was received. Each payment record includes details on the individual's sex, date of birth, postcode of residence, whether partnered, partner income support status, number of dependent children, age of youngest dependent child, earned income, unearned income, payment type, payment entitlement and, depending on the payment type, potentially other information (such as 'activity type' for Newstart Allowance recipients).

The structure of the data allows us to identify detailed patterns of income support receipt and earnings while on income support, which is very useful for evaluating the effects of the Working Credit programme. However, there are some limitations of the administrative data, such as the absence of information during the time a recipient is off payments. The information on human capital and labour market activities is also very limited. For example, there is no information on working hours or wage rates, which is clearly important to assessments of programme effects on labour market activity. Furthermore, the data set is a series of fortnightly snapshots and does not contain retrospective updates. For example, if a recipient reports earnings for preceding fortnights, this will not appear in the data set at all. This can cause Working Credit balances to unaccountably drop. In general, earnings will be under-reported because of this. The issue arises more for non-unemployment-benefit payment types. These limitations of the data should be kept in mind when interpreting the results.

TABLE 1

Raw data counts (21 September 2001 – 30 September 2005)

	No. of recipients	*No. of observations*						
		Total	*By (incomplete) spell duration category (fortnights)*					
			1–6	7–13	14–20	21–26	27–39	40+
Male UB	147,271	4,821,452	789,796	596,491	414,017	276,935	458,631	2,285,582
Male UB 25–44	67,912	2,087,786	369,443	276,979	188,763	124,940	202,670	924,991
Female UB	91,278	2,321,512	388,373	286,886	196,226	131,329	215,668	1,103,030
Female UB 25–44	30,381	756,469	126,814	90,686	59,771	39,189	63,357	376,652
Female PPS	68,452	4,322,490	163,081	182,175	173,896	141,782	291,100	3,370,456
Female PPP	44,509	1,760,309	143,844	132,976	107,645	78,332	143,994	1,153,518
Total	318,418	13,225,763	1,485,094	1,198,528	891,784	628,378	1,109,393	7,912,586

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. An observation is a person-fortnight.

The sample comprises all payment records for eligible payments in the September 2001 to September 2005 period. As noted in Section III, distinct payment type categories are examined separately, on the basis that the greatly different circumstances of individuals receiving different payment types would suggest it is inappropriate to examine them as one group.

Table 1 presents, for each of the payment type categories we examine, counts of the number of recipients and the number of person-fortnight observations in the September 2001 to September 2005 sample period. We have around 13 million person-fortnight observations in this sample period, covering 318,418 individuals. Given we have a 10 per cent sample, this implies 3.2 million individuals were observed on an eligible income support payment between 21 September 2001 and 30 September 2005, generating a total population of 130 million fortnightly payment records. Of the 318,418 individuals in the sample, 238,549 individuals (147,271 men and 91,278 women) are observed on unemployment benefits (note that a person may be observed in more than one payment type category in the sample period). In addition, 68,452 females are observed on PPS and 44,509 females are observed on PPP.

The counts of person-fortnight observations are also disaggregated by spell duration category in Table 1. That is, each observation is assigned to a duration category as at the date of the observation. Of the 13 million observations in the data, nearly 8 million are at spell durations of 40 fortnights or more. Of the 7 million unemployment benefit payment records, about 3½ million are for 40 fortnights or more. Thus, even though the typical unemployment benefit spell is relatively short (approximately 11 fortnights), observations that belong to long spells will tend to dominate person-fortnight analyses that do not condition on spell duration. This simply reflects the fact that people who experience long spells each contribute many more fortnightly payment records than do people who experience short spells. It makes it clear that person-fortnight analyses that do not condition on spell duration need to be interpreted with caution.

Working Credit balances, accruals and depletions

Table 2 shows the mean Working Credit balances broken down by income support programme and sex. As can be seen, average balances rose steadily from the fourth quarter of 2003 (when the programme was introduced) to the third quarter of 2004. From this point onwards, mean balances have remained reasonably constant. The smallest balances are for women on unemployment benefits. When comparing across benefits, however, it is important to recognise that the size of the balance is a function of both accrual and depletion. Table 3 shows the proportion of recipients depleting

TABLE 2

Mean Working Credit balance, by quarter and payment type category

Year	*Quarter*	*Males – UB*	*Females – UB*	*Females – PPS*	*Females – PPP*
2003	4	141.15	142.18	123.52	145.75
2004	1	314.53	309.22	288.93	191.08
	2	465.95	433.86	485.43	422.01
	3	558.01	513.85	595.05	652.43
	4	576.96	526.93	598.29	688.61
2005	1	580.60	530.76	596.65	698.57
	2	585.72	533.14	597.55	707.86
	3	591.34	541.91	597.23	710.52
Overall		469.82	440.10	486.21	521.88

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered.

TABLE 3

Mean proportion depleting Working Credit balances per fortnight

Year	*Quarter*	*Males – UB*	*Females – UB*	*Females – PPS*	*Females – PPP*
2003	4	0.041	0.049	0.019	0.016
2004	1	0.081	0.089	0.052	0.145
	2	0.082	0.098	0.046	0.096
	3	0.079	0.091	0.030	0.074
	4	0.087	0.100	0.043	0.055
2005	1	0.079	0.094	0.042	0.051
	2	0.082	0.092	0.041	0.055
	3	0.081	0.089	0.039	0.053
Overall		0.076	0.088	0.039	0.067

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Equal weight assigned to each fortnight.

TABLE 4

Mean fortnightly depletion amount among those depleting Working Credit balances

Year	*Quarter*	*Males – UB*	*Females – UB*	*Females – PPS*	*Females – PPP*
2003	4	100.33	102.63	75.96	95.09
2004	1	178.90	168.43	189.27	256.10
	2	177.08	165.03	108.32	263.75
	3	192.82	182.46	165.11	238.94
	4	192.86	177.61	153.64	218.97
2005	1	192.83	182.29	161.14	205.93
	2	195.47	183.89	176.47	225.33
	3	201.40	186.10	163.05	225.75
Overall		183.17	172.11	153.36	235.70

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Equal weight assigned to each fortnight.

their Working Credit balance each fortnight (giving equal weight to each person-fortnight observation). The highest rate of depletion is for unemployment benefit recipients. On average, 8 per cent of unemployment benefit recipients (8 per cent of men and 9 per cent of women) deplete their Working Credit balances in a given fortnight. The next highest rate of depletion is for PPP, with a depletion rate of 7 per cent.

In Table 4, we estimate the mean fortnightly depletion amount among those depleting their Working Credit balances. This figure is a reflection of the hourly wage and the number of hours worked by income support recipients (our data do not allow us to separately identify these factors). We observe that the mean depletion amount across all income support programmes is 179. By way of comparison, the federal minimum wage in mid-2005 was $484.40 per week (or $968.80 per fortnight). Our figures therefore suggest that if the typical depleter is paid at the minimum wage, he/she is most likely working around 14 hours per fortnight, or one full day per week. Moreover, it is interesting to note that if an income support recipient took on a full-time minimum-wage job, he/she would deplete the maximum Working Credit balance (1,000) in slightly over a fortnight.

V. 'Unconditional' estimates

As discussed earlier, we examine three outcomes on which Working Credit may have an impact: whether recipients have earnings, the level of their earnings and the exit rate. The first outcome measures the rate of employment of income support recipients while on income support ('potential effect (a)'), the second outcome measures the level of earnings of income support recipients while on income support ('potential effect (b)') and the third outcome measures the rate of exits from income support payments ('potential effect (c)').

Table 5 presents means of these three outcomes for the 'before' and 'after' samples, for each of four spell duration categories, three of which are used in the difference-in-difference analysis. In this table, an observation is a 'person-fortnight' – that is, each fortnightly payment record is treated as its own observation. An individual will therefore contribute as many observations as fortnights that the individual was on an eligible income support payment in the sample period. The statistics presented in the table provide a picture of the changes in mean outcomes for each spell duration group. They show that both the proportion of recipients reporting earnings each fortnight and the mean value of reported earnings rose for most recipient groups and spell duration groups. Moreover, it appears that increases were generally greater for those with longer spell durations. For the rate of exit from payments, patterns are less clear. Declines are evident

TABLE 5

Mean values of outcomes before and after the introduction of Working Credit, by spell duration category

A. Proportion reporting earnings

	Spell durations of 1–6 fortnights		*Spell durations of 7–13 fortnights*		*Spell durations of 14–20 fortnights*		*Spell durations of 21–26 fortnights*	
	'Before' period	'After' period	'Before' period	'After' period	'Before' period	'After' period	'Before' period	'After' period
Males								
UB	0.137	0.142	0.172	0.193	0.185	0.207	0.183	0.205
UB 25–44	0.143	0.144	0.179	0.201	0.196	0.217	0.197	0.214
Females								
UB	0.199	0.209	0.245	0.260	0.250	0.262	0.242	0.262
UB 25–44	0.203	0.213	0.259	0.283	0.268	0.288	0.251	0.288
PPS	0.400	0.349	0.370	0.360	0.340	0.361	0.328	0.367
PPP	0.116	0.116	0.110	0.124	0.106	0.129	0.103	0.127

B. Mean reported real earnings (September 2005 prices)

	Spell durations of 1–6 fortnights		*Spell durations of 7–13 fortnights*		*Spell durations of 14–20 fortnights*		*Spell durations of 21–26 fortnights*	
	'Before' period	'After' period	'Before' period	'After' period	'Before' period	'After' period	'Before' period	'After' period
Males								
UB	78.66	81.94	103.22	123.99	102.34	117.84	97.31	111.07
UB 25–44	86.99	88.23	110.71	125.77	106.75	119.96	106.89	111.51
Females								
UB	86.96	97.96	115.26	139.48	117.14	128.07	100.87	121.00
UB 25–44	102.54	112.43	136.24	159.42	152.00	152.45	113.57	145.65
PPS	306.05	280.11	268.24	287.97	237.11	280.22	227.23	285.56
PPP	45.50	59.04	42.87	67.87	40.16	65.66	39.20	64.06

C. Proportion exiting income support receipt

	Spell durations of 1–6 fortnights		*Spell durations of 7–13 fortnights*		*Spell durations of 14–20 fortnights*		*Spell durations of 21–26 fortnights*	
	'Before' period	'After' period	'Before' period	'After' period	'Before' period	'After' period	'Before' period	'After' period
Males								
UB	0.067	0.060	0.058	0.065	0.048	0.052	0.037	0.040
UB 25–44	0.070	0.061	0.063	0.068	0.052	0.055	0.040	0.039
Females								
UB	0.061	0.054	0.056	0.059	0.044	0.044	0.036	0.035
UB 25–44	0.065	0.059	0.062	0.068	0.048	0.050	0.037	0.040
PPS	0.022	0.011	0.019	0.013	0.015	0.012	0.012	0.011
PPP	0.050	0.029	0.040	0.038	0.032	0.032	0.026	0.027

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. 'Before' period is July 2002 to June 2003. 'After' period is July 2004 to June 2005.

for all recipient groups with spell durations of between one and six fortnights, while slight increases are evident for most other recipient–duration groups. However, there is no strong indication that the change in exit rates is greater the higher the spell duration category.

One could assess the impact of the introduction of the Working Credit through the simple before–after comparison of outcomes, as presented in Table 5. However, while this would have the virtue of simplicity, its counterfactual would not be especially credible. In particular, we would like to separate the effects of the improving Australian economy over the period 2002–05 (i.e. changes in labour demand) from the impact of the Working Credit on labour supply. In January 2003 (the middle of the 'before' period), the national unemployment rate was 6.8 per cent. In January 2005 (the middle of the 'after' period), it had fallen to 5.6 per cent, which is very unlikely to be solely driven by Working Credit's introduction.

TABLE 6

Unconditional difference-in-difference estimates

Treatment group defined as people with spell durations of 21–26 fortnights

	A. Proportion reporting earnings		*B. Mean earnings (Sept. 2005 prices)*		*C. Proportion exiting income support receipt*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.018**	0.0048	10.49**	3.72	0.010**	0.0012
Male UB 25–44	0.016**	0.0071	3.38	5.77	0.008**	0.0015
Female UB	0.009	0.0077	9.12**	4.46	0.006**	0.0019
Female UB 25–44	0.027*	0.0151	22.19**	9.10	0.009**	0.0017
Female PPS	0.090**	0.0106	84.27**	10.05	0.010**	0.0012
Female PPP	0.023**	0.0086	11.33**	4.93	0.021**	0.0022

Treatment group defined as people with spell durations of 14–20 fortnights

	A. Proportion reporting earnings		*B. Mean earnings (Sept. 2005 prices)*		*C. Proportion exiting income support receipt*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.017**	0.0038	12.22**	3.24	0.010**	0.0013
Male UB 25–44	0.020**	0.0062	11.97**	4.89	0.012**	0.0018
Female UB	0.002	0.0074	–0.08	8.59	0.007**	0.0019
Female UB 25–44	0.010	0.0124	–9.45	24.96	0.008**	0.0026
Female PPS	0.072**	0.0110	69.05**	9.89	0.008**	0.0013
Female PPP	0.022**	0.0079	11.97**	4.59	0.021**	0.0020

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Control group is defined as people with spell durations of 1–6 fortnights. * and ** respectively indicate significance at 10 and 5 per cent levels.

As described in Section III, we therefore employ a difference-in-difference analysis, where we compare the changes in the outcomes from 2002–03 to 2004–05 for a group that we expect to be strongly affected by the Working Credit ('long-term' income support recipients) with those for a group that we expect to be affected much less by the Working Credit ('short-term' income support recipients). Table 6 shows the results from this analysis. As in Table 5, an observation is a 'person-fortnight'. However, to allow for dependencies between fortnightly observations for the one person, all observations for one person are treated as belonging to the same cluster for the purposes of statistical inference. As noted earlier, short spell durations (the control group) are defined to be durations of 1–6 fortnights, while long spell durations (the treatment group) are defined in two ways: spell durations of 21–26 fortnights and spell durations of 14–20 fortnights.

Looking first at the difference-in-difference estimates obtained using people with spell durations of 21–26 fortnights as the treatment group, and using the proportion reporting earnings as the outcome variable (panel A), we find that for several payment groups, the before/after change for the long-term unemployed was significantly larger than the before/after change for the short-term unemployed. This holds for men on unemployment benefits, and for women on PPP and PPS. The magnitude of these effects is quite large, ranging from a 2 percentage point increase in participation among men on unemployment benefits to a 9 percentage point increase in the participation of women on PPS. These results are consistent with those in panel B of Table 6, which uses mean reported earnings as the outcome measure and finds that the improvement among the treatment group (long-term unemployed) exceeded the improvement among the control group (short-term unemployed) by a significant amount for men on unemployment benefits and for women on unemployment benefits, PPS and PPP. The increase in fortnightly earnings ranged from $10 for men on unemployment benefits to $84 for women on PPS. Significantly, as Table 5 shows, in all cases other than PPS, the positive difference-in-difference estimates for earnings in both panels A and B arise from the increase for long-term recipients being greater than the increase for short-term recipients – that is, they do not arise from declines in earnings among short-term recipients.

Panel C of Table 6 examines effects on the exit rate – the proportion of those in the spell duration category exiting benefits each fortnight. The results suggest that the introduction of the Working Credit boosted exit rates of affected men on unemployment benefits and exit rates of affected women on unemployment benefits, PPS and PPP. The increase in the fortnightly exit rate is in the order of 1–2 percentage points. Nonetheless, it must be noted that, for all groups, exit rates declined among short-term recipients, and in fact declined for three of the six long-term recipient groups. The interpretation of the difference-in-difference estimate is that exit rates have

generally declined in the 'after' period compared with the 'before' period, perhaps because improving economic conditions have reduced the pool of recipients to those relatively more predisposed to entrenched reliance on income support, and thus the exit rate for long-term recipients would have declined, or declined more than it did, in the absence of Working Credit. However, the fact that exit rates actually declined for three groups of long-term recipients perhaps makes the difference-in-difference estimates somewhat less compelling than had exit rates actually increased.

The lower half of Table 6 shows that the difference-in-difference estimates obtained using people with spell durations of 14–20 fortnights as the treatment group are similar to those obtained using those with spell durations of 21–26 fortnights as the treatment group, albeit tending to be slightly smaller. This is somewhat reassuring, since it suggests that our estimates are not particularly sensitive to the definition of the treatment and control groups.

VI. Estimates controlling for compositional change to treatment and control groups

1. Regression-adjusted differences-in-differences

Although an unadjusted difference-in-difference approach has the benefit of clarity, it potentially suffers from the disadvantage that we do not control for other factors that might affect employment status. In this section, we therefore introduce a set of statistical controls for observable characteristics that are known to affect employment outcomes. If the extent to which the treatment group has 'better' or 'worse' characteristics than the control group changes over time, this may affect our estimate of the policy effect. However, if differences between the groups in observable characteristics are stable over time, the two estimates should be the same.

In this section, our models essentially estimate the Working Credit effect by including a 'post Working Credit introduction' dummy interacted with a 'long-term recipient' dummy in a regression of the outcome of interest. Note that there is an 'interim' period, 3 October 2003 to 25 June 2004, during which time the Working Credit programme was in place but no one could have reached the maximum possible Working Credit balance of 1,000. We therefore distinguish three phases: no Working Credit (up to 19 September 2003), transitional Working Credit (3 October 2003 to 25 June 2004) and full Working Credit (9 July 2004 to 30 September 2005).

In all specifications, the sample period is 21 September 2001 to 30 September 2005. We control for age, country of birth and indigenous status, partner status, dependent children, housing circumstances, region, whether subject to job-search requirements, income support history, quarter of year

and (incomplete) spell duration. Full details on these variables are reported in Table A1 in the online appendix.[11] The inclusion of a set of variables for history of income support receipt is likely to be particularly valuable to control for unobserved heterogeneity.

Formally, our difference-in-difference regressions take the form

$$Y_{it} = \beta_1 + \sum_{j=2}^{J} \beta_j Duration_{it}^{j} + \gamma_1 Transitional_{it} + \sum_{j=2}^{J} \gamma_j Duration_{it}^{j} Transitional_{it} + \delta_1 Full_{it} + \sum_{j=2}^{J} \delta_j Duration_{it}^{j} Full_{it} + \boldsymbol{\varphi}' \mathbf{Z}_{it} + \varepsilon_{it}, \quad (1)$$

where Y is the outcome variable of interest for individual i in fortnight t. The *Duration* variables are dummy indicators for spell duration categories (21–26 fortnights or 14–20 fortnights, the omitted group being 1–6 fortnights).[12] *Transitional* and *Full* denote the periods 3 October 2003 to 25 June 2004 and 9 July 2004 to 30 September 2005 respectively, $\mathbf{Z}$ is a vector of control variables and ε is an error term. The policy impact is captured by a coefficient on the interaction between the time indicator *Full* and an indicator for attaining the 'treatment group' spell duration. For example, if $Duration^4$ equals 1 for spell durations in the 21–26-fortnights range, δ_4 provides the programme impact estimate when the treatment group spell duration is defined to be 21–26 fortnights.[13]

Analogous to the analysis presented in Section V, the outcomes examined are 'probability of reporting earnings', 'amount of earnings' and 'probability of exit from payments'. Estimates are presented in Table 7. Panel A presents employment participation results, panel B presents earnings amount results and panel C presents exit probability results. As in Section V, standard errors are obtained assuming observations are clustered at the person level.

For employment participation, regressions are estimated using a probit model where the outcome is whether the respondent had positive earnings in the previous fortnight, with each estimate derived from a separate regression. In general, the estimates accord with those from previous specifications, with the Working Credit appearing to coincide with an increase in labour force participation by male unemployment benefit recipients, female PPS recipients and female PPP recipients. As in Section V, our results are quite similar whether we define the treatment group as those in the 14–20-fortnight duration interval or those in the 21–26-fortnight duration interval.

[11]Available at http://www.ifs.org.uk/docs/fssep12_wilkinsleigh_appendix.pdf.

[12]The estimated specifications actually include dummies that distinguish spell durations of 1–6 fortnights, 7–13 fortnights, 14–20 fortnights, 21–26 fortnights and 27 or more fortnights.

[13]Note that it is by specifying the 1–6-fortnights category as the omitted dummy that we are able to interpret the coefficient as the difference-in-difference estimate of the programme impact.

TABLE 7

Regression-adjusted difference-in-difference estimates of the effects of Working Credit

Treatment group defined as people with spell durations of 21–26 fortnights

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*		*C. Hazard rate*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.015**	0.0036	140.15**	46.29	1.186**	0.0669
Male UB 25–44	0.019**	0.0056	167.10*	89.30	1.092	0.0636
Female UB	0.001	0.0058	13.72	32.10	1.171*	0.0963
Female UB 25–44	0.015	0.0109	126.17	89.66	1.024	0.1075
Female PPS	0.070**	0.0082	177.74**	18.00	1.991**	0.3153
Female PPP	0.025**	0.0070	142.00**	45.63	1.717**	0.2428

Treatment group defined as people with spell durations of 14–20 fortnights

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*		*C. Hazard rate*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.018**	0.0030	159.80**	46.38	1.198**	0.0513
Male UB 25–44	0.024**	0.0046	215.18**	97.66	1.142**	0.0497
Female UB	0.004	0.0046	25.39	28.74	1.188**	0.0754
Female UB 25–44	0.013	0.0088	89.99	77.74	1.104	0.0846
Female PPS	0.054**	0.0069	140.83**	15.41	1.700**	0.2449
Female PPP	0.024**	0.0058	141.89**	40.83	1.588**	0.1815

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Control group is defined as people with spell durations of 1–6 fortnights. Estimates in panel A are mean marginal effects of Working Credit on the probability of reporting earnings in a fortnight while on income support, obtained from probit models of the probability that earnings are reported in the person-fortnight. Estimates in panel B are of the effects of Working Credit on the amount of earnings reported in a fortnight while on income support, obtained from tobit models of reported fortnightly earnings. Estimates in panel C are of the effects of Working Credit on hazard ratios, obtained from a proportional hazards model of exit from income support; an estimate greater than 1 denotes a positive impact on exit probability and an estimate less than 1 denotes a negative impact. * and ** respectively indicate significance at 10 and 5 per cent levels.

The magnitudes from this strategy are also quite similar to those obtained in Section V, suggesting that for male unemployment benefit recipients and female PPP recipients, the Working Credit raised employment rates by 1–2 percentage points.

In panel B of Table 7, we show results from a tobit regression of fortnightly earnings. The results suggest that the Working Credit boosted employment for male unemployment benefit recipients and female PPS and PPP recipients. For these groups, the specifications show an increase in fortnightly earnings, with the magnitude of the increase between $140 and $178. These estimates are considerably larger than the estimates obtained from unconditional differences-in-differences.

In panel C of Table 7, we examine the impact of the programme on spell duration via estimation of hazard models. To date, all estimation has treated the person-fortnight as the observation. We now treat the spell as the unit of analysis in order to investigate the impact of the Working Credit programme on spell durations. For this analysis, the sample comprises all payment records of people who *commenced* on an eligible payment in the period 21 September 2001 to 30 September 2005, and observations (spells) are assigned to payment type categories according to the *initial* payment type of the spell.

The estimates reported in Table 7 are from a complementary log-log model, $p(t) = 1 - \exp\{-\exp[\beta(t)]\}$. Coefficient estimates β are not directly informative about absolute magnitudes of effects; we therefore report $\exp(\beta)$, which gives the effect of the covariate on the relative hazard ratio.[14] All 12 of the estimates are positive and nine are significant. The coefficient estimates for PPS and PPP are very large, but this may simply reflect the fact that the hazard rate is lower for PPS and PPP than for unemployment benefits (as evident in Table 5), so that a given increase in the proportion exiting translates to a larger increase in the relative hazard ratio.

2. Matched differences-in-differences

In Table 8, we present results from matched difference-in-difference analysis. Nearest-neighbour propensity score matching is used, whereby the outcome experienced by each 'treatment group' member is compared with that of a matched 'control group' member who has similar observed characteristics.[15]

In this analysis, a treatment group member in the 'after' period (spell duration of 21–26 fortnights, after the introduction of Working Credit) is matched with a control group member in the 'after' period (spell duration of 1–6 fortnights, after Working Credit was introduced), with a treatment group member in the 'before' period (spell duration of 21–26 fortnights, before Working Credit was introduced) and with a control group member in the 'before' period (spell duration of 1–6 fortnights, before Working Credit was introduced). The difference-in-difference estimate is equal to the difference between the treatment and control group members' outcomes in the 'after' period minus the difference between the treatment and control group members in the 'before' period.

[14]Each spell generates an observation. We use Stephen Jenkins's *pgmhaz8* program in Stata. Reported results are for models without unobserved heterogeneity. Models with Gamma-distributed unobserved heterogeneity were estimated, but on smaller (randomly selected) samples in order to achieve model convergence. Despite the smaller sample sizes, qualitative results were not affected, and indeed point estimates were in most cases very similar to those reported.

[15]We use the Stata *psmatch2* program by Leuven and Sianesi (2003).

TABLE 8

Matched difference-in-difference estimates

Treatment group defined as people with spell durations of 21–26 fortnights

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*		*C. Probability of exit*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.018**	0.0070	6.73	6.07	0.006	0.0055
Male UB 25–44	0.022**	0.0088	–1.05	8.33	–0.006	0.0070
Female UB	0.004	0.0117	2.39	7.62	–0.013*	0.0077
Female UB 25–44	0.031*	0.0171	33.74**	11.41	–0.001	0.0111
Female PPS	0.107**	0.0124	98.76**	11.44	0.016**	0.0044
Female PPP	0.037**	0.0093	17.61**	5.40	0.014**	0.0071

Treatment group defined as people with spell durations of 14–20 fortnights

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*		*C. Probability of exit*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.020**	0.0060	8.82*	5.09	–0.006	0.0049
Male UB 25–44	0.026**	0.0075	7.04	7.27	–0.020**	0.0060
Female UB	0.012	0.0105	–70.99**	25.57	–0.014**	0.0069
Female UB 25–44	0.031**	0.0146	24.52**	9.54	–0.026**	0.0097
Female PPS	0.063**	0.0122	61.08**	11.24	0.007*	0.0044
Female PPP	0.031**	0.0087	11.69**	5.05	–0.001	0.0067

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Control group is defined as people with spell durations of 1–6 fortnights. * and ** respectively indicate significance at 10 and 5 per cent levels.

Matching is undertaken on age, income support history, family situation, housing situation, region, country of birth and indigenous status, and whether required to engage in job search. Similar to the regression approach in Section VI.1, the ability to match on income support history is particularly valuable because this will summarise a great deal of information about sample members relevant to their current earnings and exit behaviour.

Results for the probability of reporting earnings while on benefits are generally consistent with those from the simple difference-in-difference strategy. However, estimated effects on the level of earnings are only statistically significant and positive for female unemployment benefit recipients and female PPS and PPP recipients. The significant positive estimates are quite similar to those obtained from unconditional differences-in-differences, ranging from $12 to $99. For the probability of exit, estimated impacts are either insignificant or negative for unemployment benefit recipients, implying that the positive effects found in the unconditional difference-in-difference analysis are spurious. For female PPS

and PPP recipients, significant positive effects on probability of exit remain present, but only when the treatment group is those with spell durations of 21–26 fortnights.

VII. Addressing confounding factors

The analysis presented in Sections V and VI suggests that the Working Credit programme has positive effects on earnings while on benefits and ambiguous effects on exit from benefits. However, as discussed in Section III, there are several potentially confounding factors which preclude interpreting the estimates as causal effects. In this section, we shed some light on potential biases arising from these factors by presenting four robustness checks.

The first robustness check addresses the concern that the composition of the treatment group is affected by causing recipients who would have exited prior to the 21st fortnight to exit in or after the 21st fortnight, and recipients who would have exited between the 21st and 26th fortnights to exit after the 26th fortnight, simply due to the mechanical properties of the programme. Spurious positive effects on earnings while on benefits may be found as a result, and potential positive *or* negative effects on spell duration may be masked by this. To address this concern, we examine sensitivity of estimates to the (extreme) assumption that everyone in the treatment group effectively has their spell duration extended by two fortnights due to the mechanical properties of the programme. This is undertaken by defining the treatment group in the 'before' period to be people with spell durations between 19 and 24 fortnights and retaining the 21–26-fortnight definition in the 'after' period.

The second robustness check further addresses the concern that the programme affects the behaviour of short-duration recipients by comparing outcomes of short-duration recipients before and after the introduction of the programme. The third check removes from the analysis welfare churners – those who have commenced a welfare spell within three months of completing another welfare spell – to address the concern that the composition of the short-duration group is adversely affected by the programme. That is, if the programme causes individuals to exit and then subsequently re-enter welfare receipt, we will overestimate the positive effects (or underestimate the negative effects) of the programme. That said, this is unlikely to be a significant factor given the absence of significant effects of the programme on exit.

The final robustness check addresses concerns about effects of general improvement in labour market conditions differentially affecting the long-term and short-term unemployed. For this check, we draw on administrative data for the period prior to the introduction of Working Credit, when the

unemployment rate was similarly declining, examining the implications of declining unemployment itself for estimated effects on employment and exit for long-duration recipients relative to short-duration recipients.

For all of the robustness checks, we present only regression-adjusted and matched difference-in-difference estimates, on the basis that these are the more credible estimates.

1. Changing the definition of the treatment group in the 'before' period only

Table 9 presents estimates that allow for the composition of the treatment group to be affected – in particular assuming that Working Credit increases spell durations of the treatment group by two fortnights. Most of the estimates of the effects on earnings while on benefits are very similar to

TABLE 9

Estimated effects defining the treatment group in the 'before' period as 'people with spell durations of 19–24 fortnights'

Regression-adjusted differences-in-differences

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*		*C. Hazard rate*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.014**	0.0035	128.58**	44.33	1.017	0.0446
Male UB 25–44	0.019**	0.0055	171.42*	90.11	0.897**	0.0422
Female UB	–0.001	0.0057	0.48	31.15	0.949*	0.0675
Female UB 25–44	0.012	0.0107	99.39	84.08	0.893**	0.0699
Female PPS	0.070**	0.0081	176.68**	17.80	1.460*	0.2812
Female PPP	0.026**	0.0069	147.19**	45.92	1.252	0.1706

Matched differences-in-differences

	D. Probability of reporting earnings		*E. Amount of earnings (Sept. 2005 prices)*		*F. Probability of exit*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.012*	0.0070	3.47	6.41	0.004	0.0055
Male UB 25–44	0.020**	0.0088	1.70	8.34	–0.021**	0.0077
Female UB	0.004	0.0117	1.22	7.63	0.002	0.0110
Female UB 25–44	0.063**	0.0170	33.22**	11.50	–0.008	0.0112
Female PPS	0.100**	0.0124	97.03**	11.44	0.014**	0.0040
Female PPP	0.041**	0.0093	18.82**	5.40	0.009	0.0071

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Control group is defined as people with spell durations of 1–6 fortnights. Treatment group is defined as people with spell durations of 21–26 fortnights in the 'after' period (July 2004 to June 2005) and 19–24 fortnights in the 'before' period (July 2002 to June 2003). Estimates in panel A are mean marginal effects obtained from probit models. Estimates in panel B are obtained from tobit models of reported fortnightly earnings. Estimates in panel C are obtained from a proportional hazards model of exit from income support. * and ** respectively indicate significance at 10 and 5 per cent levels.

those presented in Tables 7 and 8, the only exception being the matched difference-in-difference estimate of the effect on the probability of reporting earnings among prime-age female unemployment benefit recipients, which increases from 0.03 to 0.06 and becomes strongly significant. For exit rates, however, many of the estimates are affected, particularly for the regression models of the hazard ratios, estimates of which all become smaller. Clearly, estimated impacts on spell durations are sensitive to redefining the treatment group in the 'before' period.

2. Effects of Working Credit on short-duration income support recipients

A further potential concern with our identification strategy is that the behaviour of our control group – short-duration income support recipients – may have been affected by the Working Credit programme. As we have noted, there are two main ways this could happen, each working in the opposite direction to the other. On the one hand, there is a newly-created incentive to defer earnings in order to accumulate credits, which may reduce earnings and exit of short-duration recipients. On the other hand, a significant number of credits can accumulate in the first few fortnights of the spell, producing some increase in the incentive for earnings from quite early in the spell.

Simple before-and-after comparisons of the estimates for short-duration recipients presented in Table 5 suggest slight increases in employment and earnings, and slight decreases in exit rates, but of course these comparisons do not control for time effects. A more rigorous analysis attempting to ascertain whether the behaviour of the control group was affected is presented in Table 10. The table compares the behaviour of short-duration recipients immediately after the introduction of Working Credit (20 September 2003 to 19 December 2003) with the behaviour of short-duration recipients immediately prior to its introduction (20 June 2003 to 19 September 2003). To account for behavioural changes due to seasonal factors, the difference in behaviour is compared with the difference in behaviour over the same time periods one year earlier – giving us a difference-in-difference analysis.

None of the regression-adjusted difference-in-difference estimates is statistically significant, but several of the matched estimates are significant and positive for earnings while on benefits, and all of the estimates for probability of exit are significant and positive. Thus, there are some indications that employment participation and exit from payments increased for short-duration recipients following the introduction of Working Credit. This implies a tendency for our estimates to understate positive effects of the

TABLE 10

Difference-in-difference estimates of the effects of Working Credit on short-duration recipients

Regression-adjusted differences-in-differences

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*	
	Estimate	Standard error	Estimate	Standard error
Male UB	–0.006	0.0211	6.56	30.592
Male UB 25–44	0.018	0.0300	28.14	35.589
Female UB	0.019	0.0287	23.75	21.997
Female UB 25–44	0.058	0.0480	64.11	42.088
Female PPS	–0.022	0.0435	3.48	34.383
Female PPP	–0.025	0.0570	–1.12	42.901

Matched differences-in-differences

	C. Probability of reporting earnings		*D. Amount of earnings (Sept. 2005 prices)*		*E. Probability of exit*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.001	0.0062	3.11	21.682	0.030**	0.0030
Male UB 25–44	0.000	0.0071	26.65**	5.479	0.030**	0.0037
Female UB	0.001	0.0109	–2.78	14.786	0.025**	0.0044
Female UB 25–44	0.009	0.0125	25.50**	7.961	0.030**	0.0052
Female PPS	0.017**	0.0069	15.91**	5.240	0.006**	0.0014
Female PPP	0.051**	0.0049	16.02**	2.282	0.014**	0.0025

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Short-duration recipients comprise people with spell durations of 1–6 fortnights. Estimates in panel A are mean marginal effects obtained from probit models. Estimates in panel B are obtained from tobit models of reported fortnightly earnings. * and ** respectively indicate significance at 10 and 5 per cent levels.

programme. Certainly, there is no evidence that Working Credit has negatively impacted on the behaviour of short-duration recipients, which would have caused upwardly-biased estimates of programme impacts.

3. Excluding individuals on income support in the three months preceding the start of the current income support spell

As discussed, Working Credit may increase employment churn – cycling into and out of employment – but this seems unlikely to promote welfare churning – cycling into and out of welfare receipt. This is especially true for our definition of a welfare spell, which requires moving off benefits for at least six weeks before the spell is deemed to have ended. However, concerns may persist that findings of positive effects of Working Credit reflect compositional change to the short-duration group produced by increased welfare churning – specifically, re-entry onto welfare of those who exited

temporarily because of the programme. We therefore present, in Table 11, estimates of programme impacts excluding those who have been on benefits within the three months preceding commencement of the current spell.

Comparing with the estimates presented in Tables 7 and 8, results are broadly quite similar. While some differences in estimates are evident, most of the significant positive estimates in Tables 7 and 8 are similarly significant and positive in Table 11. Thus, as expected, results do not appear to be driven by compositional change to the short-duration group deriving from churning.

TABLE 11

Estimated effects excluding people on benefits in the three months preceding the commencement of the current spell

Regression-adjusted differences-in-differences

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*		*C. Hazard rate*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.017**	0.0040	159.06**	53.737	1.117**	0.0346
Male UB 25–44	0.023**	0.0063	210.47*	108.236	1.088*	0.0493
Female UB	–0.004	0.0063	–9.97	35.210	1.025	0.0467
Female UB 25–44	0.006	0.0118	71.03	88.622	1.027	0.0811
Female PPS	0.049**	0.0084	124.18**	18.798	1.721**	0.1495
Female PPP	0.028**	0.0077	163.24**	51.900	1.984**	0.1494

Matched differences-in-differences

	D. Probability of reporting earnings		*E. Amount of earnings (Sept. 2005 prices)*		*F. Probability of exit*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	0.020**	0.0082	4.62	6.916	–0.005	0.0066
Male UB 25–44	0.016	0.0100	10.23	10.059	–0.008	0.0078
Female UB	–0.004	0.0132	5.28	8.703	–0.019**	0.0088
Female UB 25–44	–0.036*	0.0191	–20.97	14.067	–0.002	0.0132
Female PPS	0.082**	0.0133	72.89**	12.134	0.004	0.0044
Female PPP	0.039**	0.0100	16.75**	6.080	0.022**	0.0079

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Control group is defined as people with spell durations of 1–6 fortnights. Treatment group is defined as people with spell durations of 21–26 fortnights. Estimates in panel A are mean marginal effects obtained from probit models. Estimates in panel B are obtained from tobit models of reported fortnightly earnings. Estimates in panel C are obtained from a proportional hazards model of exit from income support. * and ** respectively indicate significance at 10 and 5 per cent levels.

4. Pre-programme 'placebo' tests

To examine the potential biases created by the declining unemployment rate over the 2001 to 2005 period, in Table 12 we present results from 'placebo' tests over the pre-Working-Credit period, when labour market conditions were likewise improving. A suitable period, also close in time to the introduction of Working Credit, is the two years immediately preceding the introduction of Working Credit, from October 2001 to September 2003.[16] The synthetic 'before' period is defined to be October 2001 to September 2002 and the synthetic 'after' period is October 2002 to September 2003.

Most estimates are not statistically significant, and all but one of those that are significant are negative, suggesting improving labour market conditions have in general not led to greater improvements in employment

TABLE 12

Pre-programme 'placebo' tests of identification strategy

Regression-adjusted differences-in-differences

	A. Probability of reporting earnings		*B. Amount of earnings (Sept. 2005 prices)*	
	Estimate	Standard error	Estimate	Standard error
Male UB	0.003	0.0064	–24.26	69.920
Male UB 25–44	0.008	0.0102	101.69	132.006
Female UB	–0.025**	0.0108	–140.66**	69.903
Female UB 25–44	–0.008	0.0196	–43.01	172.820
Female PPS	0.000	0.0147	–13.07	33.742
Female PPP	–0.015	0.0098	–119.32	81.271

Matched differences-in-differences

	C. Probability of reporting earnings		*D. Amount of earnings (Sept. 2005 prices)*		*E. Probability of exit*	
	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error
Male UB	–0.013**	0.0067	–14.09**	5.508	–0.028**	0.0054
Male UB 25–44	0.013*	0.0078	93.89	242.696	–0.009	0.0063
Female UB	–0.001	0.0116	22.59	208.602	–0.015**	0.0079
Female UB 25–44	–0.003	0.0156	1.72	10.662	0.001	0.0104
Female PPS	0.014	0.0124	5.31	10.972	0.001	0.0047
Female PPP	–0.019**	0.0088	–3.28	4.264	–0.032**	0.0073

Notes: UB – unemployment benefits; PPS – Parenting Payment Single; PPP – Parenting Payment Partnered. Control group is defined as people with spell durations of 1–6 fortnights. Treatment group is defined as people with spell durations of 21–26 fortnights. 'Before' period is October 2001 to September 2002. 'After' period is October 2002 to September 2003. Estimates in panel A are mean marginal effects obtained from probit models. Estimates in panel B are obtained from tobit models of reported fortnightly earnings. * and ** respectively indicate significance at 10 and 5 per cent levels.

[16]A longer window is preferable, but in the year to October 2001 the unemployment rate was rising. Because of the shortness of this window, models of spell duration were not estimated for the placebo tests.

and exit outcomes for long-duration recipients than for short-duration recipients.

VIII. Conclusion

The Australian Working Credit programme was introduced in September 2003 with a goal of increasing employment participation of income support recipients. Absent an experiment, natural or otherwise, we present a variety of estimators and undertake a variety of robustness checks aimed at assessing the extent to which this goal was realised. The analysis provides strong indications that the programme has positive effects on the incidence and level of employment participation while on benefits, and ambiguous – or possibly zero – effects on exits from the welfare system. These findings are consistent with the incentive effects of the programme, and can be interpreted as affecting behaviour on the intensive margin and not the extensive margin (i.e. moving recipients off benefits altogether).

However, we are reluctant to ascribe a causal interpretation to the 'effects' we find. The manner of implementation and the nature of data available make it difficult to credibly evaluate the causal effects of the Australian Working Credit programme. The identification strategy of comparing differences in outcomes for long-duration recipients with differences in outcomes for short-duration recipients has strong appeal from the perspective that the incentive effects of Working Credit are much stronger for long-duration recipients. However, it is inherently problematic that the characteristic used to define treatment and control groups – spell duration – is likely to be affected itself by the programme, creating an identification problem for which there is no completely satisfactory remedy.

The difficulties in evaluating programme impacts are, of course, not unique to the Australian Working Credit initiative. As this paper highlights, policymakers should always exercise extreme caution in estimating programme impacts based on observational data. Indeed, for many programmes, credible evaluation will require carefully-designed randomised controlled trials and, even for such trials, caution is required, since findings may not be generalisable to national or large-scale implementation. Quite simply, evaluation of labour market and social policies is almost always difficult.

Finally, while no clear effects on welfare spell durations or movements off welfare are evident from the analysis presented here, it must be emphasised that the costs of the programme are not large. For example, the annual cost of the Working Credit programme per male unemployment benefit recipient, based on third-quarter 2005 figures for the share depleting Working Credit balances (Table 3) and the mean depletion amount among depleters (Table 4), is approximately \$424 (0.081×26×\$201.40). Increasing

engagement with the labour market, as the programme appears to do, increases incomes and therefore surely increases well-being of recipients in the short term. Moreover, while it is conceivable that the programme produces adverse long-term effects, it seems more likely that, by increasing connections with the labour market, the programme also improves welfare recipients' longer-term labour market prospects. This is, of course, not something we are able to test with the available data.

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