Child support and work incentives: Prospective effects of a larger disregard in the Income Support system

Yulia Kossykh, Ian Walker and Yu Zhu
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1 Project objectives

The primary objective of this project has been to estimate the effect of a £10 a week child support (CS) disregard (known as the Child Maintenance Premium), that was introduced into the Income Support (IS) system in 2003, on the labour market participation rate of parents-with-care (PWCs), more than 90 per cent of whom are lone mothers\(^1\). The secondary objective is to use the resulting estimated statistical model to investigate the likely effects of making the £10 a week disregard larger – a change recently proposed by Sir David Henshaw in his recent review of the existing CS arrangements\(^2\). The Government subsequently, in the recent Pre Budget Report 2007, announced a £20 disregard in IS by the end of 2008 with a full disregard in Housing Benefit (HB) and Council Tax Benefit (CTB) and then increasing to £40 a week from April 2010. This is a key feature in the redesign of the child maintenance system which aims to encourage and support parents to make voluntary maintenance arrangements where suitable. It aims to directly reduce the number of children living in poverty but, by increasing out-of-work incomes, it may have an impact on the employment outcomes of PWCs. The objective of this report is to attempt to assess this impact.

\[^1\] There are too few lone fathers for us to effectively model the effect of CS and IS on them separately and their much higher levels of labour market participation make it inappropriate to group them with lone mothers.

2 Background

The number of lone mother PWCs has grown dramatically over the last four decades. Successive governments have struggled with how to ensure appropriate living standards for lone parents. On average, lone mothers have lower than average education and lower than average work experience that results in their job opportunities being worse than average – in particular, the wages that they, on average, command in the labour market are likely to be lower than the overall average of all mothers. Moreover, having children and no co-resident partner with whom to share that burden of care results in lone mothers being more reliant on market-provided childcare than mothers whose partners are present in the household. Thus this group, more so than others, faces low wages and high costs of childcare which can sometimes limit their incentives to work.

Ironically, the IS system that is designed to ensure that lone parent households have minimum living standards can exacerbate this – increases in the real value of IS for such individuals (relative to in-work incomes) may generate greater welfare dependency, since such support is means tested so that IS payments are reduced by the full amount of earnings (above a small earnings disregard).

The rapidly rising number of such families, has increased the cost of providing IS for this group dramatically. The problem has been addressed in two main ways:

First, a variety of welfare-to-work programmes have been introduced, and existing ones have been expanded. In particular, following the evidence of their success in the USA under the Clinton presidency, the Family Credit programme (that had been introduced in 1988 and grew out of the Family Income Supplement which itself dated back to 1973) was reformed and expanded to become Working Families’ Tax Credit (WFTC) in 1999. This was later reformed and expanded again in 2002 to become Working Tax Credit (WTC) and Child Tax Credit (CTC). WTC makes working more financially attractive through providing cash support for parents with dependent children in low income households (and through subsidising the childcare costs). While the UK evidence is that the expansion of in-work transfers did increase the labour market participation of lone mothers, there are limits to which it is sensible to expand such a programme. In particular, while they do encourage non-participants to join the labour market there is some evidence
that they also reduce the incentives to work for existing workers in low income households resulting in a reduction in their hours of work – the more people who are affected by expanding the eligibility limit for WTC, the more important this adverse effect will become.

Secondly, CS arrangements have been formalised with the creation of the Child Support Agency (CSA) in 1993 and the introduction of the associated formula that determines liability. The original CS formula was complex and required a large amount of information to calculate liability, including the incomes of both PWCs and non-resident parents (NRPs). The aim has been to ensure that NRPs provide financial support for their children. However, CS was treated as income for the purposes of calculating IS entitlements, with the result that CS effectively became an in-work transfer programme. That is, PWCs only benefited from the CS payments of their NRPs if they were not in receipt of IS. Since IS is effectively an out-of-work welfare programme, this implied that CS promotes the incentive for lone mothers who are not working to join the labour market in order to benefit from the CS payments.

This interaction between IS and CS is likely to have had a doubly beneficial effect as far as Government expenditure was concerned: First, CS paid to PWCs who remained on IS reduced the Government’s IS spending pound for pound. Secondly, by promoting the incentive to work, CS may have both reduced IS payments and raised income tax and social security contributions (although this would be partially offset by greater spending on in-work welfare, such as WFTC, HB and CTB).

However, the original CS system was widely and rightly criticised for its complexity and for the lack of incentives for parents to cooperate with the CSA. Mothers on IS saw little point in taking any pains to ensure that their NRPs complied, since they would not benefit from the CS paid. Fathers, concerned about the welfare of their children, also saw no point in compliance if the PWC was on IS. The complexity of the CS system also made the job of the CSA very difficult. The result was widespread non-compliance.

Long delays in implementing reform to the CS system meant that it was not until 2003 that CS became much simpler (depending only on the NRP’s income from then on) and provided some incentive for compliance through the introduction of a disregard into the IS system for the first £10 a week of CS received for cases on the new (post-2003) scheme. However, the IS disregard for CS reduced the overlap between IS and CS and so potentially undermined the desirable work incentive property of the CS system. The reform also reduced the average level of NRP liability – but the hope was that the disregard and rising compliance would offset this effect on PWC net incomes of lower liabilities.

PWCs on IS now had a positive interest in CS being paid, albeit a small one. On the other hand, the introduction of the IS disregard somewhat blunted the work incentive effect because CS was no longer a pure in-work transfer – you could receive some CS even if you remained on IS.
However, there has been little research evidence that has focused on the effectiveness of the new system in terms of compliance, PWC household income levels and working behaviour. Only Paull et al. (2000) attempted to predict, albeit using only historical data, how this new system might affect PWC household incomes and their work incentives. They found that compliance levels would need to rise to quite high levels for the potential adverse work incentive effects of the disregard and the lower liabilities to be outweighed by greater CS compliance. These much higher levels of CS compliance were never achieved and a further review of the system took place which reported in summer 2006, recommending, among other things, that the disregard for CS in the IS system be raised further.

A small amount of US research focus has been on the effect of the Temporary Assistance for Needy Families (TANF) ‘pass-thru’ for CS – TANF is the US equivalent to IS, and in the US they refer to a disregard as a ‘pass-thru’. While the focus of these papers is the Government’s welfare expenditure rather than on work incentives directly, the two are likely to be highly correlated – greater work incentives will result in lower numbers of individuals on TANF and this will reduce welfare expenditure. Miller et al. (2005)\(^3\) exploits a demonstration programme in Wisconsin, known as CDSE (part of the more general reform known as Wisconsin Works or W-2), which randomised the level of the disregard – some got a full disregard (all CS paid was disregarded) and some a lower level of disregard ($50 per month, or 41 per cent of what was paid, whichever is greater). They show that the full disregard increased the receipts of CS by PWCs not only because they got to keep all of the CS that was paid by NRPs, but also because NRPs paid more if it was disregarded. That is, the mechanical effect associated with the higher disregard was reinforced by a behavioural change. However, the behavioural effects, on average, were modest and barely statistically significant (although there were subgroups of the NRPs who did pay significantly more).

Wheaton and Sorensen (2005)\(^4\) exploit estimates of the effect of cross-state variations in TANF CS disregards (that states were free to implement under the Clinton reforms to the US welfare system in the mid-1990s) that were provided by Cassetty, Cancian, and Meyer (2001). Most states had a zero disregard but about 30 per cent of states had a $50 (per month) disregard and two states had even higher disregards. Their estimates imply that if all states had a $100 disregard then CS would rise by one per cent, again, a very modest effect. Wheaton and Sorensen use these estimates to calculate the consequences of a $100 disregard on welfare spending across a range of programmes including Aid to Families with Dependent Children (AFDC). However, they assume that there were no labour supply consequences of the additional CS.

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\(^3\) See http://www.mdrc.org/publications/397/full.pdf
\(^4\) See http://aspe.hhs.gov/hsp/05/cs-dist-TANF/report.pdf
Cook and Caspar (2006) exploit more recent data from Wisconsin’s experiment to investigate longer-term effects. After 2002, all cases received the full pass-thru (although in 2006 this was changed again, so that all cases then received just the $50 pass-thru or 41 percent of what was paid, whichever was greater). Cook and Caspar note that the early gains in CS for the treatment group did wane over time. However, these results may not translate outside the US because participants receive benefits for a very short period and thus, the disregard has only a short time to affect lone parents.

Using the most recent data from the Wisconsin experiment, Cancian, Meyer and Caspar (2008) find that when PWCs have all their maintenance fully disregarded, establishing paternity occurs more quickly, NRPs are more likely to pay child maintenance and PWCs receive more child maintenance. Thus, there is some evidence from elsewhere that a disregard has desirable effects on compliance.

One question that the Henshaw recommendation raises but does not attempt to answer, is: would the higher compliance that might be induced by a higher disregard offset the greater work disincentives that such a higher disregard might generate at existing compliance rates?

Simple economic thinking suggests that raising the disregard should raise compliance; but that, conditional on the existing level of compliance, it should reduce the incentives to work. However, simple economic theorising does not answer the question of how large these effects might be. Moreover, a simple static model of behaviour might not fully capture the complexity of decision-making that is induced by the reform. In particular, NRPs may feel that, a larger CS disregard would imply that paying CS would now yield a more reliable stream of income for the children than before; and PWCs may be more inclined to press the NRP for compliance, even if she were not on IS because she may anticipate spells of future IS receipt. Thus, the increase in compliance by NRPs may not be limited to those whose PWCs are on IS. That is, we might expect not only greater compliance but also compliance that was less strongly related to PWC IS status. Increasing compliance for those NRPs whose PWCs were not on IS might serve to offset, at least partially, any potential adverse labour supply consequences associated with the disregard itself.

3 Statistical methodology

The prospect for answering the question of how a CS disregard may affect the size of the stock of IS claimants is now better than it was in 2000 because we now have the benefit of the experience associated with the 2003 reform to draw on. Indeed, one important feature of the reform was that it only applied to new cases – existing CSA cases remained on the old rules with no IS disregard. Thus, the 2003 reform implied that there existed one group which enjoyed the disregard while the other did not. This helps considerably in isolating the effect of the disregard on NRP compliance and of the additional CS compliance of the NRPs on the labour market behaviour of their PWCs. Of course, the old rules implied, on average, higher CS liabilities than the new rules and one would have to control for this difference in liabilities in order to isolate the effect of the disregard component of the reform on PWC movements out of IS.

Thus, the research reported here models the effect of the £10 per week CS disregard on the IS system: on the probability of PWC exit from IS, conditional on the compliance of the NRP; and on NRP compliance itself. Specifically, we have estimated the determinants of the probability of exiting from IS (the IS ‘exit rate’) using data provided in the Department for Work and Pensions (DWP’s) administrative records of IS claims matched to the same individuals’ histories of CS from the CSA’s records of CS liabilities and payments by their NRPs. We have also estimated the ‘entry rate’ into IS using the FACS dataset constructed from a large sample of parents, with children present in the household, who have been followed over time from well before the reform to two years after the reform.

By exploiting the fact that some individuals became CSA cases under the old rules and some under the new and that these cases can coexist over time, we can infer what the effect of the rules change has been. Moreover, since the data allows us to observe the level of CS paid by the NRP, we can control for the fact that the new rules resulted in lower liabilities on average and control for this effect.

Further, we can exploit the fact that, post-reform, the CSA could now apply the same enforcement actions to old and new cases and that we would expect their greater powers to imply larger compliance for both new and old cases.
Thus, what remains, once one controls for the effects of the reform on NRP CS liabilities and for the effects of the changes in enforcement powers, is the effect of the disregard itself.

The major difficulty we face, however, is that post-reform, the new and old cases we observe in the data have, on average, different durations of IS receipt. Since the exit rate from IS may well depend on how long PWCs have already been on IS, means that we have to control for duration of IS receipt in our modelling of exits so as to be able to compare new rule PWCs with similar, old rule PWCs. Controlling for the effects of time already spent on IS on the exit rate from IS is complicated by the fact that IS duration is only observed for those who exit – we don’t know how long the spell would last for those who have not exited by the time our data records come to an end in 2007. That is, our data on spell lengths is right censored. Moreover, the records start in 2000 and we do not know how long those individuals who are in the records already in 2000 had already been on IS. That is, the data on spell lengths are left censored also. Thus, we need to use complex statistical methods that deal with the censoring in the spell lengths in the data. Fortunately, many individuals have completed IS spells in the data and this makes the statistical analysis more robust than would otherwise be the case.

A second important difficulty in the analysis is that we want to estimate the effect of CS payments that would be enjoyed by PWCs if only they were not on IS and the effect of CS payments that would be enjoyed by PWCs if they were on IS. We only observe one of these two levels of CS for an individual PWC at a point in time. There is some difficulty in estimating the counterfactual levels of CS receipt by PWCs in both these cases. We know how much CS PWCs not on IS would have received if they had been on IS prior to the reform (i.e. zero because there was no disregard) but we do not know how much they would have received after the reform. What we do know is that it could not have been more than £10 – the size of the disregard – and it might be reasonable to suppose that NRPs who were paying less than £10 while the PWC is not on IS, might continue to pay this amount if the PWC moved onto IS. More difficult is the case of PWCs on IS. What might they receive if they were not on IS? We do observe the amount of CS that the NRP pays when the PWC is on IS but it may not be reasonable to assume that if the PWC moved off IS, the amount that the NRP would pay would remain the same. We overcome this problem by estimating a statistical model of CS and using the predictions implied by those estimates.

A further difficulty is what to do about the CS arrangements for separated couples who make their own arrangements for CS without the help of the CSA’s enforcement powers and without the straightjacket of the CSA’s rules that determine liabilities? Of course, there would be no such cases amongst the IS recipients – they are effectively compulsory CSA cases. Thus, this problem does not affect our modelling of the IS exits\(^6\). But these non-CSA cases are likely to be

\(^6\) Note that by the end of 2008, PWCs claiming benefit will no longer be treated as applying for child maintenance via the CSA.
numerous amongst those PWCs not on IS and so will affect our estimation of the determinants of the IS entry rate. We might assume that the CSA rules act as a focal point for bargaining between separating couples. This seems reasonable, although there is currently no evidence to support the idea.

A final difficulty that we wish to finesse is that: even if we establish a statistical relationship between CS and IS recipiency we cannot be sure that the relationship implies that it is the variation in the CS that NRPs pay that causes the variation in the IS exit probability across individual PWCs. It could be that the correlation is partly due to some unobserved factors that are associated with both the chances of exiting IS and with the level of CS. For example, proactive PWCs may be good at extracting CS from their NRPs and also good at extracting attractive job offers from employers. Proactivity is not something that is observed in our data and our inability to control for it will imply that our estimate of the effect of CS on IS exits is biased because it also picks up an element of proactivity as well as CS. Thus, we need to see if we can isolate some variation in CS that is not associated with unobserved factors that affect the propensity to exit from IS. Then we could relate just that variation in CS alone to the probability of exiting IS.

The same set of considerations applies to estimating the IS entry probability. However, in this case, we use data from the Families and Children Study (FACS) panel. Since this is a small sample and since it is available only up to 2005, we have rather small datasets to work with – the sample of individuals who experience the new cases rules is particularly small. This will invariably compromise the precision of the statistical modelling of entry to IS.

Estimates of the effects of the reform on IS entry and exit allows us, in principle, to predict the implications of the reform for the number of PWCs who can be expected to be on IS. If the CS change induces the exit rate to rise, say, and the entry rate to remain the same we would expect the stock of PWCs on IS to continue to fall until the entry rate equals the exit rate. Thus, the ‘equilibrium’ number of PWCs on IS is defined as that level where the flow of PWCs into IS just matches the flow of PWCs out of IS. The number flowing into IS is the entry rate, say \( n \), times the number not on IS, say \( N \). The number flowing out of IS is the exit rate, say \( s \), times the number on IS, say \( S \). Thus, equilibrium is defined by \( nN = sS \). So that the equilibrium proportion of PWCs on IS is given by \( S/(S+N) \) which, by rearranging the definition of equilibrium, gives \( n/(s+n) \).

Note that \( n=n(X_n) \) and \( s=s(X_s) \) – that is both the flow out of IS and the flow into IS depend on a set of variables – \( X_n \) and \( X_s \). For example, they will be affected by variations in the observable characteristics of individuals, the levels of CS that they expect to receive – both while on IS and while not on IS and by other factors that are affected by the reform. Thus, we can use estimates of \( n(X_n) \) and \( s(X_s) \) to predict what would happen to the steady-state number (or proportion) of IS recipients amongst the population of PWCs as the values of the \( X_s \) change. In particular, we could use our estimate of the effect of the £10 disregard (one of the \( X_s \)) to forecast the effect of replacing the £10 disregard with a higher one.
Thus, our methodology can be summarised as follows:

1. Estimate a statistical model of the amount of CS that NRPs pay using the CSA records. Include in that model at least some factors that affect CS but do not directly affect the probability of exiting IS. Also allow for the level to differ between pre- and post-reform – to capture any enforcement effect and the difference between old and new rules in levels of entitlement.

2. Estimate a statistical model of the probability of exiting IS using the IS records. Using the estimates from step 1, include in the model of IS exit that variation in CS that is associated with those factors that do not directly affect the IS exit rate but only affect the exits through its effects on CS. Also allow for the reform to affect IS exits directly, apart from its effect via CS – for example, under the old rules CS entitlement depended partly on PWC earnings and hence their labour force participation, but under the new rules CS was independent of PWC earnings. Also allow for changes in the macroeconomic environment pre- and post-reform – for example, there may have been changes in the wage distribution, perhaps through minimum wage changes and in other welfare programmes that might affect the incentive to leave IS. And finally, allow for the possibility that the CS disregard had a direct effect on IS exits under the new rules.

3. Similarly, model entry into IS using data for non-recipients of IS in the FACS data. All of the same considerations apply.

4. Use the estimated models of IS entry and exit to predict the average probabilities of IS exit and entry.

5. Define the steady-state percentage of IS recipients using the estimated flow rates predicted by our modelling exercise evaluated with and without the disregard. The difference then tells us the effect of the disregard on the stock of PWCs on IS.
4 Data and estimates of the Income Support exit rate

Our data for modelling the determinants of IS exit is based on data from the DWP administrative records of post April 2000 IS/Jobseeker’s Allowance (JSA) claimants which we matched to CSA records of all these cases whose NRPs had received a CS calculation\(^7\). All PWCs on IS are, by definition, compulsory CSA cases. The IS/JSA data provided 155,595 observations of 111,564 distinct PWCs (as indicated by their encrypted National Insurance numbers (NINOs)) which had at least one spell of IS/JSA receipt (up to 13 spells were recorded in the data). We dropped all JSA cases (just over ten per cent) and all lone PWCs who were male (a further eight per cent)\(^8\). We found that the data prior to April 2000 was incomplete in important respects so we retained only data since then. We then matched the data with the CSA records and found 14,817 lone mother PWCs (some repartnered) who had matching IS and CSA information\(^9\). We then dropped a sizeable number of cases which pre-dated the reform but which were assessed under the ‘new’ post-March 2003 CS rules or which were cases where assessment had been delayed for some reason\(^10\).

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\(^7\) Including those NRPs whose CS liability is zero (22 per cent of all matched cases).

\(^8\) Male PWCs have much higher labour market participation rate and much lower transition rates out of IS. They face much lower NRP CS liabilities (about £48 per month for male PWCs compared to £90 for female) and there is much lower compliance by female NRPs (48 per cent compared to 74 per cent for male).

\(^9\) Data could only be matched for those PWCs whose NRPs have had an assessment and this is an important limitation for our subsequent analysis.

\(^10\) These cases arise because they were in the pipeline when the reform occurred. They would have been more difficult cases than average which accounts for them still being in the pipeline.
This left just over 11,000 cases and Table 4.1 shows the summary statistics for this sample. The new rules cases have shorter spells of IS (because they entered IS later than the old cases); NRPs in these cases have, on average, lower CS liabilities, but the amount of the lower CS paid that was received by PWCs was higher (because of the disregard). In addition, it should be noted that old rules cases have been separated for longer and so are more likely to be repartnered and are more likely to have an older youngest child.

Figures 4.1 and 4.2 show the distribution of IS spell lengths but do not account for their censored nature. Figure 4.1 corresponds to cases determined by the old rules and Figure 4.2 shows the cases that were assessed under the new rules. They suggest a clear shortening of spell lengths.

**Table 4.1  Summary statistics**

<table>
<thead>
<tr>
<th>CS rules</th>
<th>Old</th>
<th>New</th>
</tr>
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<tbody>
<tr>
<td>IS start date</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations (distinct mother PWCs)</td>
<td>3,658</td>
<td>7,439</td>
</tr>
<tr>
<td>Mean completed IS spell (months)</td>
<td>24.5</td>
<td>14.1</td>
</tr>
<tr>
<td>Number of completed spells</td>
<td>1.114</td>
<td>0.591</td>
</tr>
<tr>
<td>Mean CS charges (£/wk, January 2007 prices)</td>
<td>26.47</td>
<td>20.13</td>
</tr>
<tr>
<td>Mean CS receipt (£/wk, January 2007 prices)</td>
<td>15.60</td>
<td>16.56</td>
</tr>
<tr>
<td>Mean NRP net income (£/wk, January 2007 prices)</td>
<td>136.33</td>
<td>134.21</td>
</tr>
<tr>
<td>IS start – CS application gap (days)</td>
<td>81.25</td>
<td>176.65</td>
</tr>
<tr>
<td>CS application to calculation gap (days)</td>
<td>241.40</td>
<td>157.04</td>
</tr>
<tr>
<td>PWC (re)partnered (%)</td>
<td>5.8</td>
<td>1.7</td>
</tr>
<tr>
<td>Number of qualifying children</td>
<td>1.79</td>
<td>1.45</td>
</tr>
<tr>
<td>Age of youngest qualifying child</td>
<td>9.51</td>
<td>4.34</td>
</tr>
<tr>
<td>Shared care (%)</td>
<td>13.1</td>
<td>20.6</td>
</tr>
</tbody>
</table>
Figure 4.1  Distribution of spell lengths (months): old cases

Figure 4.2  Distribution of spell lengths (months): new cases
Figure 4.3 shows plots of the survival rates (of how long IS spells lasted) for the two sets of cases. These are, so-called Kaplan-Meier plots which do allow for the censoring in the spells (we dropped all cases which started before April 2000, and we allow for right censored spells – they were ongoing when our data period ended in 2007 so we do not know when they will end). The darker line shows the proportion of individuals who remain an IS as the spell length varies among old cases and the lighter line shows new cases. This shows that, at any duration, old cases are more likely to ‘survive’ (i.e. remain on IS) than new (the dotted line is below the solid one) – so that, after the reform, for whatever reason, exit from IS was faster. The median duration can be seen from Figure 4.3 by looking at the durations corresponding to a survival rate of 0.5 – old rules have a median duration of approximately 19 months and new cases have a median duration of 12 months. The raw data suggests that the exit rate was 4.1 per cent per month for old rules cases and 7.1 per cent for new rules cases. Indeed, even when we narrow the window of the data to immediately (i.e. less than 12 months) before and after the reform, we still find a significant fall in the exit rate\(^1\). The mean IS spell length is 24.5 months for old cases compared to 14.1 months post-reform for new cases following the introduction of the disregard\(^2\). Figures 4.4 and 4.5 show the distribution of NRP payments in the data for old (Figure 4.4) and new (Figure 4.5) cases, and Figures 4.6 and 4.7 show the corresponding distributions of CS liabilities. It is clear that the new rules result in not only much fewer zero liabilities but also much fewer zero payments (but more paying the £5 required minimum). The spread of the distribution for both the NRP payments and CS liabilities is also more compressed under the new rules, reflecting, to a large extent, the underlying distribution of NRPs’ net earnings.

Our methodology requires that at least some of the variation in CS paid by NRPs reflects factors that do not directly affect PWC IS transitions. Here we rely on the idea that child gender mix affects CS paid but not IS status on PWCs – this is based on the presumption that fathers favour sons so are more likely to comply for sons and that parents like to treat their children equally so PWCs with all daughters will have lower compliance, on average, than similar PWCs with a mix of genders, which will be lower than similar PWCs with just sons\(^3\).

\(^1\) We also inspected the data to see if there was any anticipation of the rule change – did the exit rate rise in anticipation of the reform? We see evidence of an anticipatory increase, compared to the same months in previous years, only in March 2003 but the effect is small.

\(^2\) These are completed (i.e. censored) spell lengths. The predicted uncensored spell lengths can be found in Table 6.1 and are just slightly larger.

\(^3\) There have been a number of earlier studies that have documented fathers’ preference for sons and parents’ preferences for equal treatment of siblings.
Data and estimates of the Income Support exit rate

Figure 4.3  Plots of survival rates (months)

Figure 4.4  Distribution of NRP CS payments for old cases (£/week)
Figure 4.5  Distribution of NRP CS payments for new cases (£/week)

Figure 4.6  Distribution of NRP CS liabilities for old cases (£/week)
Thus, the first stage in our analysis is to model how CS payments by NRPs vary with their characteristics and the reform. This equation imposes the restrictions that: CS cannot be negative (although it could be zero and is in a proportion of cases); and CS paid by NRPs should not be more than liability (although many pay exactly their liability and a small proportion pay more). Below we report on the estimated effects of the variables that are most interesting for our analysis. Firstly the effects of the reform which are captured by a simple before and after variable, Reform, that reflects the fact that enforcement powers were tightened (which would increase CS through a compliance effect) and that there is a new formula (which implies lower CS liabilities on average). Of course, since it is simply a before and after indicator variable, it also reflects the effects of any time variation in compliance behaviour. Secondly, the reform introduced a disregard that we would expect to increase compliance which we capture through the variable Reform*min[CS, 10] which switches on only post-reform and then takes the value of 10 (£ per week) or the level of CS liability if that is less than ten. The idea here is that Reform captures all of the effects of the reform on CS – apart from that

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14 That is, we adopt a specification for this CS equation, known as a Tobit model, which accounts for these features of the data.
due to the introduction of the disregard. Finally, we also include two variables that capture the idea that gender mix of the children matters for compliance\(^\text{15}\).

The second stage of our analysis is to estimate the determinants of the probability of exiting IS for PWCs. Our modelling includes (amongst other determinants): the level of CS paid by NRPs (CS) which equals the amount received by PWCs if they are not on IS – so we can think of CS as a measure of CS when not on IS; the effect of the disregard (captured by Reform*\(\min[CS,10]\) as before) which is the amount of CS that would be received if the PWC were on IS (it is interacted with Reform because prior to the reform there was no disregard so CS while on IS was effectively zero; and the post-reform indicator (Reform) that here captures the effects of calendar time – and so picks up the effects of macroeconomic changes to the labour market and the effects of other policy changes that occurred at that time that would also affect IS flows independently of CS reform)\(^\text{16}\).

There are two major reasons why this issue cannot be addressed via straightforward multiple regression techniques: First, the dependent variable of interest in how long the PWC has been in receipt of IS (known, more generally in such modelling, as the ‘survival time’) is most likely not to be normally distributed\(^\text{17}\). Second, there is the problem of censoring, that is, some observations will be incomplete. The assumption made for the distribution of the unobservable determinants of survival in the modelling allows this problem to be overcome. Thus, our specification of the survival duration assumes that it depends on the duration of time spent in receipt of IS so far, as well as other factors that may or may not be time varying. A Weibull distribution is the most flexible parametric specification of survival duration models used in practice – it has coefficients that determine the scale, location and shape

\(^{15}\) These variables are: \(\text{AnyBoy}\) which takes the value 1 if there is at least one son; and \(\text{BoyRatio}\) which takes on the value 1 if there are sons only, \(\frac{1}{2}\) if there is one son and one daughter, 0 if there are all daughters, etc. Together, they capture the idea that sons may be favoured by NRPs but that if there is more than one child, NRPs prefer to treat them equally – equally well, if at least one is a boy; and equally badly if there are no boys. Note that gender mix of the children affects only compliance since it does not feature in the rules that determine liability.

\(^{16}\) We cannot analyse the effects of the HB and CTC disregards even though they were introduced earlier than the IS disregard. Housing tenure is not available in the administrative data and we have no idea of the ultimate earnings of individuals who exit IS on which to base their HB/CTC eligibility. Only 14 per cent of FACS lone mother PWCs are in receipt of HB and only 26 per cent are on CTB. It seems likely that these disregards will have at least some effect of the exit rate from IS but, since they would affect old and new cases alike, it seems unlikely that the possibility of such in-work disregards could bias our estimates of the IS disregard in any particular direction.

\(^{17}\) In practice, survival times are usually assumed to follow either an exponential distribution or a Weibull distribution.
of the density of survival times and allows the baseline hazard rate (the inverse of the survival rate) to shift according to the values of the explanatory variables – which may vary with calendar time or be constant across time.

The estimated coefficients (of just the main variables of interest) of both the first and second stages are presented in Table 4.2. Our estimation of the CS paid equation (which also included a range of other variables) implies the following\textsuperscript{18}: other things being equal, post-reform NRPs pay \pounds7.68 less per week (so the effect of the rule change on liabilities is large, negative and highly statistically significant); but the \pounds10 disregard introduction has a compliance effect that implies \pounds2.13 more CS paid post-reform; and the gender mix variables were statistically significant\textsuperscript{19}.

The effect of the reform on the hazard rate is threefold: first the new administration of CS (captured by the variable \textit{Reform}) speeded up exits considerably (in a Weibull model this arises if the coefficient in the hazard rate equation is significantly greater than one – here it is much greater than one) perhaps through ensuring higher compliance for all cases because of more effective compliance actions\textsuperscript{20}; secondly, the higher average CS payments associated with new cases speeds up exit from IS (because the compliance effect due to the disregard on CS payments

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\textsuperscript{18} Here we use a specification where the level of CS is replaced by its prediction from our first stage analysis but use actual CS in the second stage variable that captures the effect of the disregard – i.e. in the $\min[CS,10]*\text{Reform}$ variable. The results that replace CS in this variable by CS liability are very similar.

\textsuperscript{19} This specification is without the use of time varying covariates (TVCs). The specification with TVCs is difficult to estimate because of a lack of time series variation in CSA assessments. We also experimented by re-estimating just using the data observed immediately (i.e. one year), before and after, to minimise the possible contamination due to time varying factors that may be correlated with the other determinants of CS. The results were very similar – just less precise because they used less data. This is reassuring because it shows that our estimates do not depend on our parametric assumptions about the shape of the relationship between the exit rate from IS and its determinants.

\textsuperscript{20} Note that this variable also reflects any time varying factors that affected new and old cases alike, as well as the reform \textit{per se}. For examples Work Focused Interviews (WFIs) were introduced for some PWCs on IS from April 2001 and were extended to a wider range of cases over successive years and made more frequent. See Knight et al. (2006) for an evaluation of the WFI and New Deal policies. However, to the extent that changes such as WFI affected old and new CSA cases alike, our results of the effects of the disregard should be robust.
dominates the lower average liabilities); thirdly, the direct effect of the disregard is to lower the exit rate (coefficient < 1).

Table 4.2 Estimated parameters of CS and IS survival rate equations

<table>
<thead>
<tr>
<th>CS payment</th>
<th>IS ‘Hazard’ rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tobit equation</td>
<td>Weibull survival equation</td>
</tr>
<tr>
<td>Coeff</td>
<td>Std error</td>
</tr>
<tr>
<td>--------</td>
<td>-----------</td>
</tr>
<tr>
<td>CS</td>
<td>-</td>
</tr>
<tr>
<td>Reform</td>
<td>-7.684***</td>
</tr>
<tr>
<td>min[CS,10]*Reform</td>
<td>2.133**</td>
</tr>
</tbody>
</table>

Note: Specifications also include control variables for shared care, whether NRP and PWC live in the same local authority (a measure of proximity), relevant other children, age of NRP, age of the youngest qualifying child, gaps between IS application and CSA application, gaps between CSA application and assessment and the mean and standard deviation of NRP’s net incomes. *** (**,*) indicates that a coefficient is statistically significant at the one per cent (five per cent, ten per cent) level from the critical value of 0 for the CS equation and 1 for the hazard equation.

As a check on the reliability of the estimates we re-estimated using only the data from immediately before and after the reform – so that IS spell lengths were much more similar between the old and new cases. The results were similar in magnitude but less statistically significant.
Data and estimates of the Income Support entry rate

Our modelling of entry to IS uses the data from FACS waves 4 to 7\(^{22}\). Our sample consists of all female PWCs who are not on IS and we model entry onto IS from this population\(^{23}\). We include in this population individuals who are not CSA cases as well as those that are – the reform is likely to affect both groups. Most of them will have been working prior to exit onto IS and this will most commonly be associated with separation from a partner. There are 4,606 observations over the four waves of 1,767 distinct PWCs, 304 are new rules cases and 1,612 are old rules cases\(^{24}\). The entry to IS rates are 1.2 per cent per month for the old rules and 1.1 per cent for the new rules cases.

Table 5.1 shows the summary statistics for this sample. Here, PWCs are asked about how much they are ‘supposed’ to receive from the NRP and whether they usually receive all, some, or none of this. There is a large increase in the proportions with positive supposed liabilities and in the proportion who receive some or all of it. However, a major drawback of the FACS data is that the actual amount of CS received is not reported, we, therefore, have to ‘infer’ it by assuming that those PWCs who say that they receive all or some of their entitlement, actually receive all of their entitlement\(^{25}\).

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\(^{22}\) It is not possible to use FACS for modelling the probability of exit from IS because the data on duration is only available in 12-month intervals.

\(^{23}\) We treat all those who remain working but on IS as ‘failures’ – i.e. entrants onto IS.

\(^{24}\) There is some overlap between the two.

\(^{25}\) Assuming that those who report partial receipt, receive 50 per cent of their entitlement does not change our results significantly.
Table 5.1  Summary statistics for FACS entry to IS sample

<table>
<thead>
<tr>
<th></th>
<th>Old rule</th>
<th>New rule</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observations (person-wave)</td>
<td>4287</td>
<td>319</td>
</tr>
<tr>
<td>Mean completed work spell (months)</td>
<td>47.6</td>
<td>46.3</td>
</tr>
<tr>
<td>Contact with (contacted by) the CSA (%)</td>
<td>49.7</td>
<td>28.8</td>
</tr>
<tr>
<td>Percentage receiving IS</td>
<td>16.4</td>
<td>10.0</td>
</tr>
<tr>
<td>Mean weekly CS expected liability (January 2007 prices)</td>
<td>38.0</td>
<td>47.1</td>
</tr>
<tr>
<td>Mean weekly CS ‘receipt’ (January 2007 prices)</td>
<td>30.0</td>
<td>43.1</td>
</tr>
<tr>
<td>Percentage with positive CS liability</td>
<td>54.4</td>
<td>60.8</td>
</tr>
<tr>
<td>Percentage with positive CS ‘receipt’</td>
<td>39.2</td>
<td>51.7</td>
</tr>
</tbody>
</table>

Figure 5.1 shows the distribution of CS receipt for this sample. Figure 5.2 shows the Kaplan-Meier plots that show how the survival rate for being off IS varies with length of being off-IS for old (darker line) and new cases (lighter line). The two curves cut across each other at 24 months, after which point the survival rates under the new rules is higher than under the old. Note that the survival rates here are high and there is a long tail to the distribution of survivals.

This sample of PWCs off-IS in FACS is quite different from the stock of PWCs on IS in the administrative data. The average level of CS receipt is substantially higher for this sample and our estimated impacts of the reform reflect this. We find that the £10 disregard increases CS receipt by £8.44, while the new rule itself reduces CS by £32.16 a week because the new formula is less generous to PWCs. Note that these estimates are not directly comparable with those based on the IS administrative data because the nature of the two samples is very different. Indeed, the FACS data of lone mothers not on IS is heavily dominated by non-CSA cases, while the IS administrative data is effectively all CSA cases. The effect of gender mix of children remains significant.

This model is used to generate the exogenous variation in CS that determines the entry rate to IS and we find that variation in CS has a negligible and insignificant effect on entry onto IS; the new rule itself speeds up entry to IS but this is also not a statistically significant effect. The coefficient that picks up the effect of introducing the disregard suggests that the £10 disregard is found to have only a small marginally significant effect on increasing the entry rate onto IS (p=0.071)\textsuperscript{26} by approximately five per cent.

However, because of the FACS data limitations (in particular, the lack of information on the actual CS receipts) and the resulting lack of precision in our estimate of the effect of the £10 disregard on the IS inflow rate, we choose to fix the IS entry rate.

\textsuperscript{26} Our results, using least squares, showed a small insignificant negative elasticity of the flow onto IS with respect to the CS disregard. However, if we use the Weibull specification (as we do with the outflow modelling) we find a small insignificant positive effect.
rate at its current level when simulating the effect of a higher disregard. That is, our simulations of the effect of the reform on the IS stock assume that there is no effect of the disregard on the entry rate onto IS.

Figure 5.1 Distribution of CS receipt
Figure 5.2  Entry to IS by off-IS spell duration

Data and estimates of the Income Support entry rate
6 Simulating the effects of the reform and the effects of extending the disregard

The equilibrium non-participation rate is determined by flows onto and off IS, i.e. by \( n(.) \) and \( s(.) \). Our analysis shows the effects of a £10 CS disregard on the IS system and the challenge is to use this to show the effect of widening this disregard. Our estimates of the effect of the disregard are found from comparing IS entry and exit rates of new and old cases for those with levels of entitlement (when on IS) that are less than £10 compared to those above this level. Unfortunately, our data has insufficient variation in CS below the £10 level of the existing disregard to allow us to identify any possible non-linearities in the effect of the disregard. Thus, we simply take our estimated effect of the £10 disregard on CS payments by NRPs and replace the \( \text{min}[CS,10]^\text{Reform} \) variable with \( \text{min}[CS,Z]^\text{Reform} \) and then vary \( Z \). We can then predict the effect of a disregard of any \( Z \) on IS entry, and exits compared to a £10 disregard – both directly and through the effect of the disregard on CS payments. Note that the effect of \( Z \) is inevitably non-linear because as \( Z \) rises, fewer and fewer cases will find that the disregard affects them since their CS would not be constrained by \( Z \). The levels of CS payments by NRPs, for each level of the disregard, predicted by the estimates are shown in Figure 6.1.

It is notable how the levels of CS vary little when the disregard size gets beyond £20 because the distribution of the liabilities gets quite thin above the £20 level.

Table 6.1 shows the predicted average effect of \( Z \), by predicting for each individual in the on-IS sample and then averaging over the samples of old and new cases\(^{27}\) separately and both together. We keep all other variables at their existing levels and so our simulation is the effect of \( Z \) on these samples – not the effect on the corresponding populations at some date in the future or even in 2007.

\(^{27}\) Of course, our analysis and simulations are limited to the cases for which matching IS and CSA records exist.
The first block of columns shows the effect on the predicted CS by NRPs for the sample on IS using the administrative data and we would expect this to increase as $Z$ rises because this improves the incentive to comply. However, this effect gets ‘capped’ by the constraint that payments should be no more than liability and the net effect is that average CS payments rise only modestly with the size of the disregard – from £10.20 to a peak of £18.40 per week for the old cases data and from £17.10 to just over £20 per week for new cases. Indeed, as the disregard expands from £10 to £20 we predict an increase in CS paid of approximately one-third but beyond £20 per week we find very little effect on the degree of CS compliance and level of CS that PWCs are likely to receive.

Figure 6.2 shows the effect on the predicted CS payments by NRPs as the disregard rises (from £10 to £50 as we move left to right in the figure) for both old and new cases. Figure 6.3 breaks this down by level of CS actually paid for all cases – it seems that the disregard has little effect on CS payments for those with low CS liabilities. Thus, for example, among those that paid £10-£20, we predict average payments of approximately £20 if they all face the £10 disregard and if they face the £20 disregard, the prediction is approximately £23, but raising the disregard further has essentially no effect on the predicted payment for this group. Of course, raising the disregard raises the amount of CS received by PWCs at any given compliance level.

Figure 6.3 also shows that even for the very small group that paid more than £50, raising the disregard beyond £30 has almost no effect on predicted payments. Thus, raising the disregard beyond £20 would actually have little effect on NRP payments. While, in principle, we would expect the higher disregard to mechanically have a beneficial effect on PWCs’ net incomes, it seems that much
of the non-compliance is for NRPs who have quite modest levels of liability, so that incentivising them to pay more through raising the disregard makes only a small difference to the PWC net incomes. The cumulative distribution of PWCs by NRP liability level is given in Figure 6.4. Almost 60 per cent have NRPs with very low levels of liabilities and less than 15 per cent have liabilities greater than £30.

The second block of columns in Table 6.1 shows the predicted effects of the disregard on the changes in the level of predicted CS received by PWCs abstracting from the IS system\textsuperscript{28}. Thus, overall, a £10 disregard induces £5.20 more CS receipt, £20 induces £9.70, etc.

The third block of columns in Table 6.1 shows the predicted effect of just the disregard on IS duration holding the level of CS payments constant\textsuperscript{29}, while the final block of columns shows the corresponding effect of both the disregard expansion and the higher level of CS induced by the disregard expansion. In the case of the old cases data, we first simulate what would happen if they faced the £10 disregard in the new rules and the new formula and then we simulate the effect of expanding the disregard, together with the liability levels we calculate under the new rules. That is, we convert old rules cases to new rules cases and then simulate the effect of varying the disregard from 0 up to £50 per week. The overall effects are the weighted average of the new and old cases where the weights are the proportions in the IS data – in practice, when the expansion of the disregard comes into effect, the population of IS cases is likely to have a higher weight than the new cases.

As we would expect, ignoring the effect of the reform on compliance results in a greater impact on predicted duration of IS. That is, the compliance effect and the labour supply effect, conditional on compliance, work in opposite directions\textsuperscript{30}.

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\textsuperscript{28} That is, these figures show the minimum of the disregard and the predicted CS paid by NRPs (which is the amounts indicated in the first block in Table 6.1), at each level of the disregard.

\textsuperscript{29} That is, it assumes that the reform has no effect on the CS paid by NRPs. It only captures the effect of CS receipts induced mechanically by the CS disregard.

\textsuperscript{30} The simulations in Tables 6.1 and 6.2 are obtained from the estimation sample that takes into account that some of the NRPs have a zero liability.
Table 6.1  Simulated effects of expanding the disregard on IS exit rate: conditional on CS and unconditional total effects

<table>
<thead>
<tr>
<th>Disregard size (£/week)</th>
<th>Predicted CS payment by NRPs (£/week)</th>
<th>Predicted value of CS to PWCs net of IS (£/week)</th>
<th>Predicted duration of IS (months)</th>
<th>Predicted duration of IS (months)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Old</td>
<td>New</td>
<td>Overall</td>
<td>Old</td>
</tr>
<tr>
<td>0</td>
<td>3.7</td>
<td>7.5</td>
<td>6.1</td>
<td>0.0</td>
</tr>
<tr>
<td>10</td>
<td>10.2</td>
<td>17.1</td>
<td>14.5</td>
<td>4.5</td>
</tr>
<tr>
<td>20</td>
<td>15.9</td>
<td>19.8</td>
<td>18.3</td>
<td>8.4</td>
</tr>
<tr>
<td>30</td>
<td>17.9</td>
<td>20.1</td>
<td>19.3</td>
<td>11.5</td>
</tr>
<tr>
<td>40</td>
<td>18.3</td>
<td>20.2</td>
<td>19.5</td>
<td>13.8</td>
</tr>
<tr>
<td>50</td>
<td>18.4</td>
<td>20.2</td>
<td>19.5</td>
<td>15.4</td>
</tr>
</tbody>
</table>

Note: The simulations are based on a baseline that assumes that WFIs have happened and that post-2001 administrative changes are in effect. That is, they capture just the effect of the disregard and changes in levels of entitlement.
Simulating the effects of the reform and the effects of extending the disregard

Figure 6.2 Predicted average CS payments by disregard size: old and new cases (£/week)

Figure 6.3 Predicted average CS payments by disregard size: all cases by CS (£/week)
Although expanding the disregard beyond £20 has a relatively modest impact on the amount that NRPs pay (an increase from £18.3 to £19.5 on average), the amount that PWCs receive increases quite substantially – from £9.7 to £17.1 per week (see Table 6.1). This is likely to have an adverse effect on lone parents’ incentives to work, increasing their average IS spells from 20.3 months to 21.7 months.

The final step in our analysis is to take the predicted exit rates (the entry rate effects are badly determined and so we fix this rate at the observed level) and calculate the implications, at each level of $Z$, for the steady-state proportion of PWCs on IS. These calculations are presented in Table 6.2.

Table 6.2 Simulated effects of expanding the disregard on percentage of PWCs with a CS calculation and on IS: overall

<table>
<thead>
<tr>
<th>Disregard (£/week)</th>
<th>0</th>
<th>10</th>
<th>20</th>
<th>30</th>
<th>40</th>
<th>50</th>
</tr>
</thead>
<tbody>
<tr>
<td>Predicted change in the percentage of PWCs on IS</td>
<td>-4.2</td>
<td>0</td>
<td>3.3</td>
<td>6.1</td>
<td>8.2</td>
<td>9.3</td>
</tr>
<tr>
<td>Predicted the percentage of PWCs on IS (total effect)</td>
<td>-4.2</td>
<td>0</td>
<td>3.3</td>
<td>5.7</td>
<td>7.8</td>
<td>9.0</td>
</tr>
</tbody>
</table>

On a practical level we cannot observe NRP earnings in FACS data and hence, not compute CS liabilities under the new rules. Moreover, the instability of the estimate of the effect of the disregard on the IS inflow rate is further exaggerated in our predictions because we are effectively making predictions far from the sample means.
Conclusions and further research

Our conclusion is that the direct effect of the disregard on the exit rate from IS is relatively large and the indirect effect working through the level of compliance and payments has only a modest offsetting effect, especially for a disregard level beyond £20 per week. We find that expanding the disregard from £20 to £50 increases the amount that NRPs pay from £18.3 to £19.5 on average. At the same time the amount that PWCs receive increases quite substantially – from £9.7 to £17.1 per week. It is this substantial increase that gives rise to an adverse effect on lone parents’ incentives to work, increasing their average IS spells from 20.3 months to 21.7 months.

Overall, we estimate that the direct effects of expanding the disregard, ignoring the CS compliance effect, are fairly large – suggesting that a £50 disregard (relative to a £10 disregard) would result in a 9.3 per cent increase in the number of PWCs with a maintenance calculation on IS. If the compliance effect of the disregard is taken into account, we still predict a nine per cent rise in the number of PWCs with a maintenance calculation on IS when increasing the disregard from £10 to £50.

While this predicted increase appears to be quite substantial, there are other policies that would counteract the adverse effect of a higher IS disregard. For example, the announced increase of a full disregard in HB and CTB will act to increase in-work incomes and so should increase the exit rate from IS. Similarly, the Government is proposing to introduce a requirement that PWCs with youngest child aged seven or older (which is approximately 60 per cent of CSA cases) should be moved from IS to JSA and should be supported to look for work. Because this group would then be compelled to look for work, this ought to reduce the IS rate and the proportion of PWCs who are not in work.

Unfortunately, the estimated effect of the disregard on the IS entry using the FACS data is very poorly determined and, in the light of this imprecision, we fix the IS entry rate.
A number of further research possibilities are suggested by our analysis. We have confined our attention to the effects on participation in IS and this could be extended to consider the effects on Government spending and revenue, following Wheaton and Sorensen (2005), by incorporating our behavioural responses into calculations of welfare/tax-credit expenditure and tax revenue to compute the fiscal consequences of the disregard. Similarly, it would be possible to extend the analysis to compute the effects of the reforms on the distribution of net incomes (if one were prepared to make some assumptions about the hours of work distribution of new working PWCs) – in principle, for both the PWC households and the NRP households.

Secondly, one might look at further behavioural modelling to capture the effects of CS on partnership dissolution which we might expect to rise with the disregard (see Walker and Zhu (2006)) and fertility (especially teenage child-bearing and early school leaving) which might conceivably be encouraged by the disregard) about which we have no evidence that is informative.

An important element of the prospective reform is to allow ex-partners to negotiate their own CS arrangements without recourse to the CSA. We might expect that the CS formula acts as a focal point for negotiations for those separated parents who are currently not CSA cases and it may be possible to analyse how their arrangements work and extrapolate to forecast the effects of the further increase in the disregard. However, it is unclear whether this could be done with existing data.
References


