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Working paper 23/02

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Do work search requirements work? Evidence from a UK reform targeting single parents*

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Abstract

Proponents of work search requirements for out-of-work welfare claimants argue they are effective in inducing individuals to work and delivering fiscal savings. In this paper, we provide a much more comprehensive assessment than has been available to date, exploiting a UK reform introducing full-time work search requirements for single out-of-work parents. Using the policy's staggered roll-out, we show that the reform reduced the number of single parents claiming welfare by a quarter, partly by discouraging eligible individuals from beginning a claim in the first place. However, only about half of the reduction in the number of claimants is explained by higher employment, and almost all of that is in part-time, low paid jobs – the median marginal job pays around the 13$^{\text{th}}$ percentile of the UK earnings distribution, so even those that get into work pay little in tax and receive significant (in-work) transfers. Most of the rest of the effect is accounted for by individuals substituting to – more generous – incapacity and disability benefits. As a result, the policy produces fiscal savings indistinguishable from zero. Furthermore, we find negative effects on the mental health of individuals who remained out-of-work, though positive effects for those pushed into work.

*Data from the Family Resources Survey were made available by the Department for Work and Pensions, which bears no responsibility for the interpretation of the data in this report (National Centre for Social Research et al. 2021). The UK Household Longitudinal Study is an initiative funded by the Economic and Social Research Council and various government departments, with scientific leadership by the Institute for Social and Economic Research, University of Essex, and survey delivery by NatCen Social Research and Kantar Public (University of Essex 2021a; University of Essex 2021b). The research data are distributed by the UK Data Service. The Labour Force Survey (LFS) data are Crown Copyright and reproduced with the permission of the Controller of HMSO and Queen's Printer for Scotland (Office for National Statistics and Northern Ireland Statistics and Research Agency 2021). The LFS data were made available through the UK Data Service. We are grateful to Richard Blundell, Monica Costa Dias, Robert Joyce, Claire Crawford, Alex Bryson, Jake Anders and seminar participants at IFS and the ASSA 2023 meetings for helpful comments and advice.

1 Introduction

Recent years have seen more OECD countries tightening out-of-work benefit eligibility criteria by requiring claimants to prove that they are seeking work (Venn 2012; Langenbucher 2015). One interpretation of such 'conditionality' reforms is that they aim to go some way to correct for the kind of fiscal externality that means-tested benefits inevitably create: when an out-of-work benefit claimant gets into work and stops claiming, it reduces government spending and increases tax revenue, but the claimant does not internalise this.[1] Making claimants prove that they are looking for work reduces the utility of being out-of-work on benefits and thereby goes some way to limiting the fiscal externality. Whether such a reform is overall socially valuable is partly - but certainly not entirely - contingent upon the efficacy of such reforms at getting claimants off benefits and into work. Accordingly, the employment and benefit caseload effects of this type of reform have been extensively studied in the economics literature (see Kluve et al. (2007), Card et al. (2018), and Vooren et al. (2018) for more recent reviews).

Despite this voluminous literature, the state of our knowledge about this sort of policy is limited in two important ways.

First, the focus of the existing research has been to a very large extent focused on employment and benefit receipt – but conditionality could affect a number of other key economic and non-economic outcomes critical for evaluating its welfare effects.

Second, and as discussed in more detail shortly, almost of all the existing research examines the impact of conditionality on the 'outflows' from benefits - for example, the extent to which such requirements reduce the time it takes for claimants to get into work. These effects might operate by increasing the intensity of claimants' job search and therefore raising the likelihood that they receive a job offer, and reducing their reservation wage by sanctioning them (reducing or ending their benefits) if they turn down an offer. But, importantly, such requirements also reduce the utility of claiming out-of-work benefits - either by making claimants do more intense search than they voluntarily would, or by increasing the hassle associated with a given level of search (travelling to the employment office, proving that they have applied for jobs, etc.). Reducing the utility of being on out-of-work benefits should increase the outflow from benefits, but also reduce *inflows* too: someone considering claiming benefits will be less inclined to do so if there are conditionality requirements attached. However, measurement of this 'deterrent' effect is almost entirely absent from the literature, because of the focus on outflows. By missing the deterrent effect on inflows, the total impact of conditionality is thus potentially understated by existing research.

In this context, we provide a much more comprehensive assessment of conditionality than has been available to

[1] Of course, the government could also seek to limit such fiscal externalities simply by cutting the financial value of out-of-work benefits. Various alternative rationales for conditionality-type policies have been proposed; see Besley and Coate (1992) and Moffitt (2006) and citations therein.

date. We do this in two key ways. First, we examine a much richer set of important outcomes than has typically been studied. These include the kind of jobs that the newly employed get into - in terms of hours, pay, and tax liabilities - as well as whether those to whom conditionality is applied substitute to alternative benefits. Bringing these results together allows us to estimate the full fiscal effects of the policy, and thereby assess its efficacy in correcting for the fiscal externality that out-of-work benefits create - something missing from existing research, despite the fact that it is arguably the key rationale for conditionality policies. Moreover, given that conditionality shuts down an option for workers (claiming a benefit without searching for work), it might plausibly have negative implications for their welfare, even if they get into work and especially if they are unsuccessful and have to carry the burden of additional work search requirements. We therefore also study the effects on the mental health and reported life satisfaction of those affected. Second, we can quantify the impact of conditionality on both inflows and outflows (to benefits or employment), and all our key estimates are of the 'total' effect, incorporating impacts on both flow directions. This allows us to calculate, for example, the effect of conditionality on the employment rate, which in steady state is determined by *both* the job separation and job finding rates - something not possible from almost all of the existing literature. These two core contributions - examining a much wider suite of outcomes than the existing literature, and estimating total effects including inflows - are necessary for a fuller appraisal of conditionality.

The critical components that allow us to make progress over the existing literature are twofold. First, the nature of the policy we study has the following features: (1) we can establish whether or not a given person would be subject to conditionality if they claimed out-of-work benefits, regardless of whether they are currently claiming; (2) the rules that govern who conditionality is applied to change over time and are based on plausibly exogenous characteristics;[2] and (3) at every point in time, some individuals are not subject to conditionality. Together, this allows us to identify the impact on all individuals, not just existing claimants. Much of the other literature uses randomised controlled trials among existing claimants, or variation in timing of when existing claimants are treated, which prevents the study of non-claimants and therefore inflows to benefits. Second, and relatedly, because whether a person would be subject to conditionality if she claimed is dependent upon observable characteristics (and not, for example, assignment in a randomised trial), we can use standard household surveys. These allow us to examine a much wider set of outcomes than is typically possible in the existing literature which generally uses administrative benefits data, and thus will rarely have information on things such as mental health or even tax liabilities.

The reform we study is the "Lone Parent Obligation" (LPO), introduced in the UK between 2008-2012. Whereas previously single parents could indefinitely claim out-of-work benefits without any job search requirements until their youngest child turned 16, the LPO (in a staggered fashion) brought that age threshold down to 5. Treated

[2] In this case, the single/couple status of the individual and the age of their youngest child. We are able to check for partnership and fertility responses to the policy (see Section 5).

single parents were required to prove that they were looking for work in order to receive the out-of-work benefit. If they did not, they could be 'sanctioned' - temporarily losing the out-of-work benefit (though not any others). We exploit the staggered nature of the policy rollout, together with modern difference-in-difference approaches, to estimate the impacts of the reform.

Our first finding is that the policy raised employment rates by 4.4ppts. While 4.4ppts represents a significant increase in employment, it was essentially entirely among part-time jobs. It was also in low earning jobs, with the median new job paying £8,000 per year (2021 prices) - around the 13^{th} percentile of the population-wide employee earnings distribution, and the 23^{rd} percentile among single parent employees. These findings have implications for the impact of the policy on the long-term career trajectories of lone parents. Blundell et al. (2016) find that part-time work for women results in only a small amount of human capital accumulation and future wage rises relative to not working at all, meaning that these jobs are unlikely to be a "stepping stone" to better paid work.

Our second finding is that the reform induced very little (if anything) in terms of fiscal savings, and thus is ineffective at correcting the fiscal externality created by out-of-work benefits. We can reject the government's expected savings at conventional significance levels. The limited savings are driven by two effects. First, because the jobs that the single parents got into were low paid, they paid little in tax and remained entitled to a significant amount of (in-work) benefits. Second, the policy induced substitution to other benefits. While claims of the main out-of-work benefit to which conditionality was applied fell by 8.9ppts, there was a 3.4ppt increase in the take-up of incapacity or disability cost benefits (benefits aimed at those who cannot work, or have higher living costs, because of ill health, and which do not have conditionality requirements attached). These benefits are more expensive than the main out-of-work benefit, offsetting some fiscal savings. The small fiscal effect has important normative implications. As discussed above, a plausible motivation for conditionality is to ameliorate fiscal externalities, by reducing the utility associated with claiming benefits and thereby mitigating the inevitable moral hazard they create. But if there is limited fiscal saving from the policy, then fiscal externalities are little affected - and the main effect is to remove a choice from the single parents (to claim out-of-work benefits without work search), making them worse off. This would appear to make the reform a worsening in welfare terms.

Our third finding is that, despite conditionality removing a (frequently exercised) choice from claimants, there appears to be little change in claimants' life satisfaction, wellbeing or mental health outcomes on average. We provide evidence that this average impact reflects two offsetting effects. On the one hand, those that get into work because of the policy see an improvement in these outcomes of a similar size to those that got into work before conditionality was introduced. But this is offset by a worsening in life satisfaction, wellbeing and mental health by those who remain out of work.

Our fourth finding is that the 'deterrent' effect of conditionality on benefit claiming - disincentivising people from starting a claim in the first place (e.g. following job loss) is significant. Incorporating the negative impact of conditionality on inflows to out-of-work benefits increases our estimate of the total effect on the benefit caseload by 40%, partly accounted for by mothers choosing to claim incapacity benefits instead of the main out-of-work benefit which the reform attaches conditionality to. This suggests that the focus of the existing literature on outflows alone means much of the effect of conditionality is being missed. Despite this large effect, we find no impact of conditionality on job separation. It seems, therefore, that conditionality does not make it any less likely that workers stop working, but does affect their claiming decisions thereafter.

This research connects to the wider active labour market policy (ALMP) literature. Conditionality represents one tool in the ALMP toolbox, with other major ones being training programmes, public sector jobs, and subsidised private sector jobs. As discussed above, the extant literature on conditionality is heavily focused on 'outflows', or more broadly how existing claimants are affected. Table 6 in the Appendix surveys 38 papers which study conditionality policies or similar: those policies that do not primarily aim to enhance a given claimant's employability or place them directly into a job, but incentivise them to search more intensely.[3] 36 of these 38 papers look only at the impact on already unemployed claimants, and so are unable to measure the deterrent effect on inflows. Of the two that look at the whole population, one (Hérault et al. 2020) is underpowered to distinguish between the impacts of inflows and outflows. The other (McVicar 2010) studies only a short pause in job search monitoring brought about by local employment office refurbishments. This is unlikely to represent the long run equilibrium impact of monitoring on flows - for example, potential inflows to benefits are unlikely to know about the temporary pause in their local area, and even if they do, its short lived nature diminishes the value in responding to it. The overwhelming focus on outflows likely owes partly to the identification strategies that the literature employs: 21 of the 38 papers use either randomised controlled trials or a timing-of-events method (which, following Abbring and Van Den Berg (2003), use variation in when existing claimants are treated by conditionality). Such methods cannot be straightforwardly applied to examining inflows.

The paper is structured as follows. Section 2 provides an overview of the reform and the broader institutional context. Section 3 describes the data and empirical approach that we use. Section 4 steps through our results on labour market, income, fiscal, and wellbeing outcomes. Section 5 examines threats to identification and sensitivity checks. Section 6 discusses implications for policymakers and concludes.

[3] To find papers that study these policies, we adopted two approaches. First, we looked at the papers that examine those programmes categorised as 'job search assistance' in Card et al. (2018) and those categorised as 'services and sanctions' in Kluve et al. (2007). Second, we searched for relevant terms on Google Scholar. We discarded papers that were training programmes, public sector employment programmes, or private sector wage subsidies. We also discarded programmes where participation was voluntary.

2 Institutional setting

At the time of the policy, the UK had two means-tested out-of-work benefits for those without disabilities: Income Support (IS) and Jobseekers' Allowance (JSA). The two benefits provide identical financial support to those who are out of work, at £60.50 per week for singles over 25 in 2008. Both of these benefits are (identically) means-tested and claimants can receive them indefinitely (they can only receive one or the other at any one time, however). Neither are contribution or employment history related. The two benefits differ only in eligibility: IS is largely only available to single parents, while all out-of-work individuals are eligible for JSA (subject to the means-test) but claimants must prove that they are looking for work.[4]

Until 2008, single parents remained eligible for IS so long as their youngest child was 15 or younger. Between 2008 and 2012 the government introduced the 'Lone Parent Obligations' (LPO), whereby that age limit was steadily reduced over four 'phases': from 15 to 11, then 9, then 6, then 4, with approximately a year gap between these changes. The immediate impact of this reform was to move those claiming IS onto JSA, and to require new out-of-work benefit claimants to claim JSA rather than IS. This did not change claimants' financial support (since IS and JSA have identical financial entitlements), but meant that they had job search requirements imposed if they were to keep claiming (and sanctions for failing to search could be applied). Existing claimants were eligible for a brief transition period (up to a year, depending on their child's birthday) where they could continue to claim IS even when otherwise identical new claimants would be ineligible. A full timeline of this roll-out is presented in Table 5 in the Appendix. In general we cannot determine in our data whether and how long a given single mother was eligible for a transition period, and so we define treatment time as the period when new claimants were no longer eligible for IS (and instead would have to claim JSA). Because we show the dynamic effect of the policy, this is implicit in our results, and we focus on the impact 6 to 15 quarters after treatment - at which point, the transition period will have ended for all claimants.[5]

We exploit the fact that the roll-out of the policy affected those with different aged children at different times. The only effect of the reform was to apply job-search conditionality to those who had not been subject to it before - and so it is the effect of this treatment that we study. The precise nature of the job-search conditionality requirements are described in more detail in Appendix A. In brief, claimants must have fortnightly meetings at the employment office with an employment officer, and show that they are taking steps towards finding a job - e.g., applying for jobs, preparing a CV, and attending interviews. The government state that finding work should

[4] Additional details on eligibility requirements are described in Appendix A.

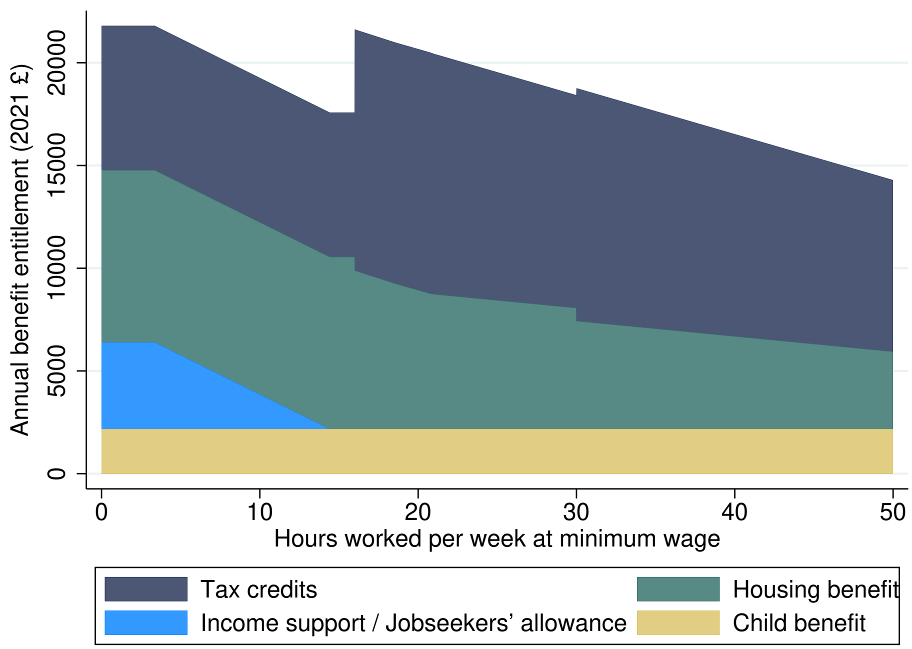
[5] The only other paper to study the impact of this policy on labour market outcomes is Avram et al. (2018), which uses administrative benefit claim data. They compare treated claimants to both claimants with younger children, and earlier cohorts of claimants. We are able to extend on this work in two key directions, consistent with our key contributions. First, we estimate impacts on a much wider range of outcomes, owing to our use of survey rather than administrative benefits data. Second, because Avram et al. (2018) look at outcomes only for the existing stock of claimants, they are limited to analysing outflows from benefit claiming. By comparison, we are able to analyse the effect on inflows and thus the whole population. Both of these extensions are critical for our objective of providing a fuller assessment of the effectiveness of conditionally.

be a 'full time job' for claimants (Child Poverty Action Group 2008). Importantly, being on JSA does not entitle the claimant to training programmes that they would not have been entitled to on IS. The employment officer may require the attendance of such programmes (which in some cases the claimant may not have been aware of beforehand), but if there is such an effect, it operates through the conditionality requirements placed upon the claimant by the employment officer. This means that we are able to isolate the effect of conditionality alone.

As well as IS or JSA, lone parents are usually entitled to other cash benefits. These include child benefit, housing benefit, and tax credits (the last of which is comprised of child tax credit and working tax credit, though those eligible to both generally claim them together). Receipt of IS or JSA guarantees eligibility to tax credits and, if the claimant is a renter, housing benefit. With the exception of child benefit, all of these benefits are means-tested; and despite the name, tax credits are unrelated to the tax system and can be received by those who pay no tax. Moreover, none of these benefits have conditionality attached to them. Figure 1 shows benefit entitlements for an example lone parent. There are three important things to note about this. First, for many claimants, IS or JSA makes up only a minority of their entitlement - on average, just 30%. They could chose to claim other benefits out of work, and not be subject to conditionality, even after the LPO. Second, single parents are strongly incentivised to work part-time; above 16 hours per week they often faced very high effective marginal tax rates (up to 96%). Third, if they do choose to work part-time, they would often be entitled to a similar level of support in-work as they were out-of-work - a consequence of the fact that tax credit eligibility increases markedly at 16 hours per week. This will be important for the fiscal effects of the policy.

Because the only important distinction between IS and JSA is the application of conditionality, we largely refer to the two together as "IS/JSA", and describe the LPO as introducing conditionality to IS/JSA.

Figure 1: Benefit entitlements for example single parent (two children, renting)



Notes: Benefit entitlements under the 2010-11 tax and benefit system shown, expressed in 2021 prices (using the CPI to deflate). The single parent is assumed to be a social renter with a weekly rent of £130; working at the October 2010 minimum wage of £5.93 per hour; and not have any other source of income. Income support and Jobseekers' Allowance are mutually exclusive, but provide identical financial entitlements, hence a claimant of either could be represented by this figure. Child benefit is a universal payment to families with children. housing benefit is paid to low income renters to support housing costs. The 'tax credits' series is comprised of child tax credit (paid to low income families with children) and working tax credit (paid to low income families if they work at least 16 hours per week). Those that are eligible to both generally claim them simultaneously, and so we combine them together into one series here. It is because working tax credit eligibility is conditioned on working 16 hours per week that the tax credit series spikes at that point. A fifth benefit, council tax benefit, is paid to low income families to cover a local property tax; it is small in size and so for simplicity we exclude it from this figure.

There are two benefits for those in ill health. Neither map closely onto the US's Social Security Disability Insurance (SSDI). Incapacity benefit is a means-tested benefit paid to those who cannot work for health reasons.[6] It is an alternative to IS or JSA (claimants can receive only one of the three). It is paid at a higher rate than IS or JSA, with the level of payment depending on disability, and with no or very few conditionality requirements attached. Separate to incapacity benefit is a disability cost benefit.[7] This is a non-means-tested, and non-contributory benefit

[6] There are two main incapacity benefits in place during the period we study - one simply called 'incapacity benefit', and another called employment and support allowance (ESA), which slowly replaced the former from 2008. We refer to receipt of either as 'incapacity benefit'.

[7] By 'disability cost benefit' we mean Disability Living Allowance and Personal Independence Payment; the latter began to replace the former from 2013.

which aims to compensate disabled people for higher living costs (such as wheelchairs or personal carers) - however it is simply a cash benefit and does not require the claimant to actually purchase any such disability-related items. Incapacity benefit and disability cost benefit can be claimed simultaneously.

3 Data and empirical approach

The analysis at the parental level in this paper is based on three large household surveys. Much of the literature on conditionality and other active labour market policies has used administrative benefit data to assess their impact. Using surveys comes with several trade-offs. First, representative survey data captures those that do not participate in the benefit programme, but whose decisions - including whether or not to stop working and *begin* participating - may be affected by the threat of conditionality (the 'deterrent' effect). This allows us to examine the impact of the policy on the whole population and therefore population-wide averages (e.g. employment rates), rather than just outflows of existing claimants (into, e.g. employment), which administrative data is often limited to and, as discussed above, the existing literature is almost entirely focused upon. Second, we can observe a much wider range of outcome variables than is typically available in administrative data, including hours and earnings of those that get into work, type of jobs, tax payments, claims to other benefit programmes, net fiscal contribution, reported wellbeing, and fertility responses. Both of these advantages allow for a more comprehensive evaluation of conditionality than is typically available using administrative benefit data. Third, benefit claimant data is affected by fraud and error in claims and the reporting of one's actual circumstances (e.g. employment). This is not a trivial issue: over the period we study and for the benefits we examine, fraud and error is estimated to vary between 3% and 10% of total entitlement depending on the benefit and year in question (DWP 2015; HMRC 2021). On the other hand, surveys have measurement error too, and in particular benefits are known to be underrecorded. Sample sizes of those on the benefit are also smaller. In addition, household response may be non-random and related to the treatment studied. The literature's reliance on administrative data, though, may be less to do with these trade-offs and more to do with the kind of variation that researchers have exploited. Randomised controlled trials have been widely used as have discontinuities in the application of treatment that generally requires administrative data, such as those based upon the a claimant's precise age when they began a claim (see Table 6 in the Appendix). Such strategies often cannot be implemented using survey data. The approach we follow here, however, is a difference-in-difference strategy which divides single parents into treatment and control groups based upon the age of their youngest child. This assignment can be made in survey data.

The three surveys covering he entire population of possibly directly affected individuals that we use are:

Labour Force Survey (LFS), a quarterly rotating panel of around 75,000 adults per quarter, measuring key labour market outcomes, as well as indicators for benefit receipt (though the value of benefits received, taxes, and net incomes are not measured). Respondents are surveyed for five consecutive waves and we include all observations of them. Pay is only measured in the first and fifth wave. The LFS is the largest survey among those we use and it is also the key source of UK's official labour market statistics.

Family Resources Survey (FRS), an annual cross-sectional household survey of 35,000 adults which has detailed information on incomes (including benefit receipt and taxes) and the basis for the UK's official income distribution and poverty statistics. It also has information on labour market outcomes for all adults in the household.

UK Household Longitudinal Survey (UKHLS), an annual panel of around 50,000 adults. Like the FRS, this has information on incomes (including benefit receipt and taxes) and labour market outcomes for all adults in the household, but also has measures of mental health and reported life satisfaction.

In our main analysis we treat all three datasets as a repeated cross-section. Each of these datasets are designed to (with weights) be representative of the UK population. In order to increase sample size, where outcomes are common across datasets we pool datasets together (this is particularly important for the income and tax outcomes that are not in the LFS). The datasets change in their relative sample size over time, most importantly with the UKHLS only starting in 2009. This could potentially introduce a bias if there are average differences between datasets (for example, if one has a higher employment rate than the others). We therefore control for dataset fixed effects in the analysis. For the analysis of mental health and reported life satisfaction we use UKHLS; for benefit take-up, net income and fiscal effects, UKHLS and FRS. For the narrower labour market outcomes we use all three datasets. In the baseline specification we use unweighted regressions, but for descriptives and in robustness checks we will use sample weights to account for differential non-response.

In Table 1, we present descriptive characteristics of our analytical sample: single mothers aged 25-55 with a youngest child aged 0-18, observed between 2004 and 2016 for FRS and LFS or between 2009 and 2016 for UKHLS. We focus on mothers rather than fathers because although the policy affects both, there are very few single fathers in the data and they are likely to systematically differ from the single mothers. We also restrict our attention to those aged 25-55 to avoid complications around educational and retirement decisions. The characteristics are similar across the datasets. UKHLS has some differences to the other two - for example, a larger fraction with degrees and fewer inactive - but much of this is explained by UKHLS sampling a later period. When we examine average characteristics across 2009-2016 in all three datasets, these differences largely disappear. Overall, our sample represents a little under 200,000 individual observations over the 13 year period. The sample is relatively low educated, with more than half having no qualification beyond GCSEs, the qualification generally achieved in the last year of compulsory schooling (age 16). They have on average 1.7 children, and most (61%) are employed when we observe them, with the bulk of the remainder (31%) being inactive. Around one in three single mothers receive the main out-of-work benefits (IS/JSA), while a substantial majority (78%) claim tax credits (that can be received out-of-work, or in-work so long as earnings are low enough). Many fewer claim incapacity and disability cost benefits (5% and 6% respectively). Single mothers have a net income of around £22,000/year on average, out of which a little over half is accounted for by benefits. Average benefit receipt is considerably higher than average taxes paid (including employer, employee, and self-employed National Insurance contributions - the UK's payroll tax), implying a negative net annual fiscal contribution of around £8,600 on average. Equivalent unweighted statistics are available in Table 7 in the Appendix; the values of almost all cells are very similar.

Table 1: Descriptive characteristics of single mothers, 2004-2016 (2009-2016 for UKHLS), weighted

	All	LFS	FRS	UKHLS
Highest qualification				
Still in edu.	0.01	0.00	0.02	0.02
GCSEs	0.59	0.60	0.57	0.48
A-levels	0.17	0.17	0.17	0.20
Degree	0.23	0.23	0.24	0.30
Housing tenure				
Social renter	0.43	0.43	0.43	0.44
Private renter	0.23	0.23	0.23	0.24
Homeowner	0.34	0.34	0.34	0.32
Age of youngest child				
0-4	0.27	0.28	0.26	0.24
5-6	0.12	0.12	0.11	0.11
7-9	0.16	0.17	0.17	0.15
10-11	0.11	0.11	0.11	0.11
12-15	0.21	0.21	0.22	0.22
16-18	0.13	0.12	0.15	0.16
Benefits claimed				
IS/JSA	0.30	0.30	0.30	0.26
Incapacity benefit	0.05	0.05	0.05	0.06
Disab. cost benefit	0.06	0.06	0.08	0.09
Housing benefit	0.47	0.46	0.52	0.55
Tax credits	0.78	0.78	0.76	0.84
Age	38.29	38.20	38.63	38.96
Number of children	1.73	1.74	1.69	1.71
White	0.85	0.84	0.86	0.88
Employed	0.61	0.61	0.62	0.64
Inactive	0.31	0.31	0.32	0.25
Hours worked	17.29	17.13	17.66	18.81
Employee earnings	10,027	10,010	10,182	9,896
Benefit receipt	12,477	.	12,293	12,733
Tax paid	3,891	.	3,870	3,921
Net income	21,726	.	21,756	21,685
Sample size	195,627	163,351	20,345	11,931

Notes: This table includes single mothers aged 25-55 with a dependent child aged 0-18, observed between 2004 and 2016 (2009-2016 for UKHLS), and uses sample weights. The variables listed under 'benefits claimed' are individual dummies indicating receipt of these benefits. The 'housing benefit' variable includes any receipt of council tax support, as the LFS only measures receipt of either benefit (in the other surveys, receipt of housing benefit alone is approximately 5ppts lower). All financial variables are annualised and expressed in 2021 prices (deflated with CPI). All variables, including earnings, hours, and tax paid, are unconditional averages (i.e. include zeros). Tax credit receipt in LFS is only measured from 2007. Tax paid, benefit receipt, and net income are not measured in LFS. Taxes are comprised of income tax, National Insurance contributions, and council tax. These are measured directly in the data, except for the National Insurance contributions formally levied on the employer; we calculate these based on the individual's earnings. LFS only records employee earnings in the first and last wave of the panel (of five waves). Highest qualification in the FRS before 2008 is not recorded and is imputed using the age the individual left full-time education. 'GCSEs' are exams taken at age 16 (the age at which school attendance ceases to be compulsory); A-levels at age 18. Those with no qualifications or other qualifications are combined with the GCSE group. UKHLS observations from the immigrant and ethnic minority booster sample in wave F are dropped.

Note that for the analysis of the children level outcomes that we will run as explained in Section 4.7, we will use the **National Pupil Database (NPD)** which is England administrative data, providing information about pupils' attainment over their whole formal schooling in private and public schools[8] as well as the matched **UKHLS-NPD** dataset which links parental responses from the UKHLS survey to their children data.

Our empirical approach is a staggered difference-in-difference, using repeated cross-sectional data, exploiting the fact that the policy was rolled out to those of different ages at different times.

The policy produces four treatment groups: single women with a youngest child aged 5-6, 7-9, 10-11, and 12-15, who became subject to conditionality at different times. We add on two control or 'never treated' groups: those with a youngest child aged 0-4 and those with a child aged 16-18. The former group do not have conditionality applied to them at any point, while the latter has conditionality applied throughout. Note that our specifications are biased if the roll-out of the policy produces any general equilibrium effects that affect the control groups, for example, by reducing employment through increased job competition. Nonetheless, we show that the results are consistent to changes in the control groups and descriptively we do not observe any decline in the employment rates of these groups. We examine changes in these groups at a quarterly frequency around treatment.

As is now well known, standard OLS methods, employing two way fixed effects, can deliver biased estimates in a staggered difference-in-difference design (Roth et al. 2022; Athey and Imbens 2022). In our baseline specification therefore use Borusyak et al. (2021), an alternative estimator which is robust in this setting. We check the robustness of our estimates to using Callaway and Sant'Anna (2021), another recent estimator developed for use in staggered difference-in-difference designs.

We briefly outline the Borusyak et al. (2021) approach. Its key assumptions are the standard ones in the difference-in-difference setting. First, treated and control groups would have had "parallel trends" had the LPO not been introduced. Second, the reform has no causal effects before implementation - that is, no anticipation effects. Third, since we are using repeated cross sectional data, we also require the reform not cause treated or control units to switch to the other group (e.g. via childbirth) in a way that is correlated with the outcome. We provide evidence later that no such switching occurs. Given these assumptions, the data generating process takes the familiar difference-in-difference form:

[8]The high quality data is available from early 2000s onwards, and each cross-section includes the full universe of students in English schools. Each year panel includes around 9 million students aged 2-21. In total, over the whole period, there are over 23 million students who were part of the NPD panel. NPD has a large and diverse set of children demographic characteristics and potential outcome variables (albeit these are not matched with parental information at this point). In terms of cognitive development, NPD offers all exam data, in class performance/grades in the Key Stage 1-5 and Year 7 exams, as well as post-graduation destinations. In terms of non-cognitive outcomes, NPD offers information on: absences, exclusions, behavioural/emotional issues. In terms of children's health NPD has data on learning difficulties, referrals to doctors for any reasons, and children hospital episodes, as well as the reasons for all these events.

$$y_{i,g,t} = \alpha_g + \beta_t + \sum_{k=0}^{\bar{K}} \delta_k D_{t=e(g)+k} + \epsilon_{i,g,t} \tag{1}$$

Where $y_{i,g,t}$ is an outcome for individual i, a member of treatment/control group g, at time t. In our repeated cross-sectional setting, group membership is defined according to the age of the i's youngest child at time t. α_g measures time invariant, group specific fixed effects for group g, and β_t time fixed effects. With $e(g)$ the time period in which group g is treated, $D_{t=e(g)+k}$ is a dummy indexing whether those in group g were treated k periods ago. D dummies are zero for all individuals in groups that are never treated, and for individuals who are observed in a group before that group is treated. Thus, the δ_k describe the dynamic impact of the treatment on $y_{i,t}$, allowing for the possibility that the treatment effect varies over time. Note that, unlike the standard practice with two-way fixed effects models, the D dummies in equation 1 index periods since treatment, but not before - so this equation cannot be used for 'pre-testing' - a point we will return to shortly. $\epsilon_{i,g,t}$ is the error.

Borusyak et al. (2021) propose what they term an 'imputation' estimator, which is implemented in three steps. First, we take all untreated observations (i.e. those where $D_{t=e(g)+k} = 0$). This includes observations from groups that are never treated (e.g. those with a child aged 0-4), and observations from groups that are not yet treated (e.g. those with a child aged 12-15 observed in 2007). With only these untreated observations we can use OLS to estimate α_g and β_t; call these estimates $\hat{\alpha}_g$ and $\hat{\beta}_t$. So long as there are no anticipation effects (individuals responding to the treatment in advance of the treatment), and the standard parallel trends assumption holds, these estimates are unbiased. Second, we can use these estimates to impute what outcomes would have been observed for the treated observations in the absence of the treatment: $\hat{y}_{i,g,t}(0) = \hat{\alpha}_g + \hat{\beta}_t$. By comparing this to the actual outcome, we retrieve an estimated treatment effect: $\hat{\delta}_{i,t} = y_{i,g,t} - \hat{y}_{i,g,t}(0)$. This treatment effect is i,t specific, but as a result is overfitted to the data - any error ($\epsilon_{i,g,t}$) shows up as a treatment effect. In other words, the i,t specific treatment effect, together with $\hat{\alpha}_g$ and $\hat{\beta}_t$, perfectly predict the actual outcome. This leads to the third step, which is to average $\hat{\delta}_{i,t}$ treatment effects across many observations. There are various ways one might want to aggregate treatment effects. We focus on the standard 'dynamic' or 'event study' form of aggregation, which generates one treatment effect per period after treatment:

$$\hat{\delta}_k = \frac{1}{|I_k|} \sum_{i \in (I_k)} \hat{\delta}_{i,e(g)+k} \tag{2}$$

Where I_k is the set of observations that are observed k periods after treatment. $\hat{\delta}_k$ is a consistent estimate of the impact of treatment k periods after treatment. Standard errors in this setting can be estimated conservatively with an auxiliary model which itself need not be correct. Interested readers are directed to Borusyak et al. (2021), but in brief, we estimate treatment effects under the assumption that they vary by g, k jointly; if there is in fact more treatment effect heterogeneity than this, that will tend to inflate our standard errors; if the g, k cells are small, that will tend to downward bias the standard errors because of overfitting.

We turn now to testing for parallel trends and anticipation effects, or 'pre-testing'. In the standard two way fixed effects approach, pre-testing and treatment effect estimation are done in a single regression, by extending the number of δ_k coefficients to incorporate pre-treatment periods. This is subject to some of the same problems that treatment effect estimates in two way fixed effects are; moreover, even when there are no parallel trends this approach can cause overly conservative standard errors (Roth 2018). The proposal in Borusyak et al. (2021) is to do the estimation and pre-testing tasks separately. In the pre-testing exercise, the sample is untreated observations (from never-treated groups and from treated groups prior to treatment), and an OLS regression analogous to equation 1 is used, except where δ_k dummies indicate periods prior to treatment. With $\underline{K}$ δ_k dummies, the reference group is observations more than $\underline{K}$ periods prior to treatment. Testing whether $\delta = 0$ provides evidence on the parallel trends and no anticipation effect assumptions implicit in the estimator.

Two brief points on the presentation of our estimates are worth noting. First, because the treatment effect estimation and pre-testing exercises are distinct, we follow the suggestion of Borusyak et al. (2021) to emphasise this fact by displaying the two sets of δ_k coefficients in different colours when showing the typical 'event study plot'. Second, a consequence of this approach to pre-testing is that a coefficient is estimated for δ_{-1}. In the two way fixed effects model (and in some of the more recently proposed estimators) the period immediately prior to treatment is standardly the reference, and so no effect is estimated for it.

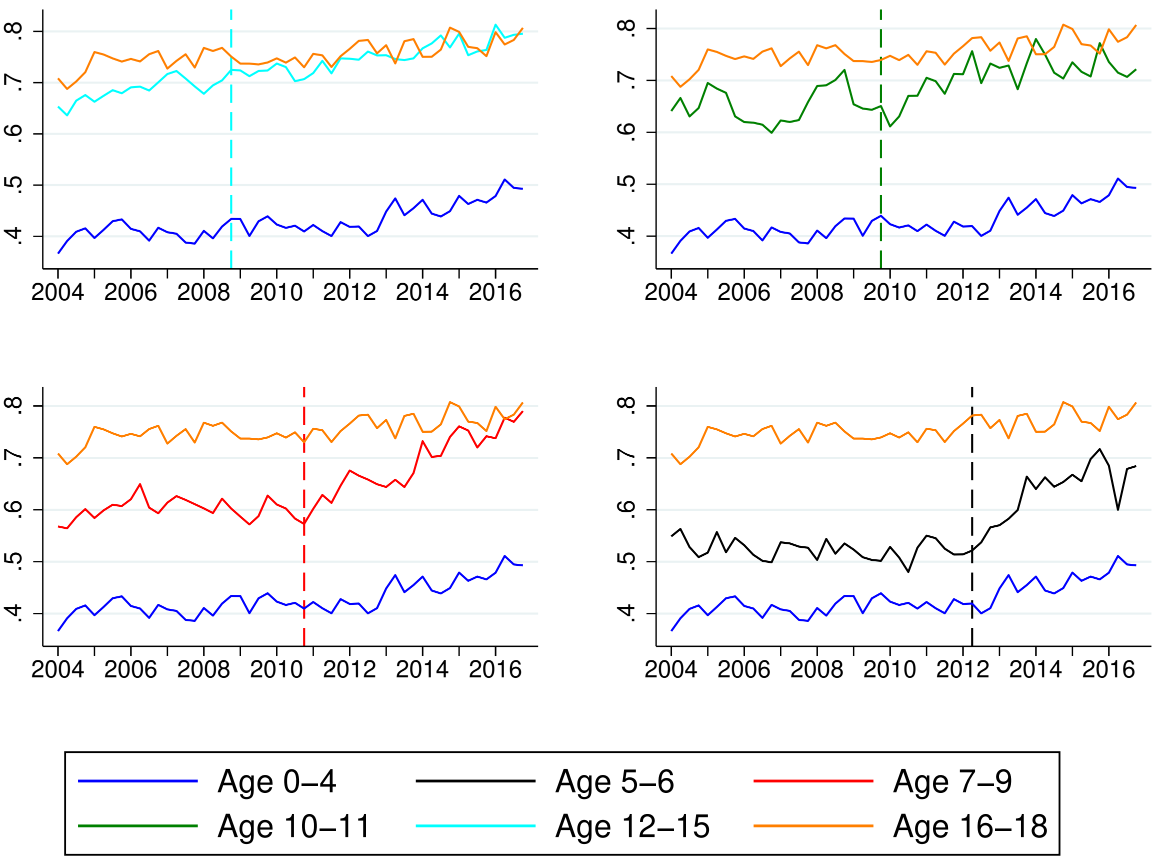
4 Results

4.1 Employment and benefit claiming

We begin by descriptively examining at the evolution of employment rates over time. Figure 2 shows the employment rate in each of our treatment and control groups. For visual clarity, we show four panels, each containing one treatment group and both control groups (age 0-4 and age 16-18). The vertical dashed lines shows when conditionality was implemented for each group. Prior to conditionality being implemented there is little change in the employment rate of each of the treatment groups, with the possible exception of the those with a child aged 12-15 - (though there is also a small increase for those with a child aged 16-18). In case this reflects a secular trend, we run additional specifications where we allow for treatment group specific time trends (discussed in further detail below); this makes very little difference to results. Following implementation there is a rise in employment rates, with the rise appearing to be larger for those with younger children.[9]

[9]A small increase in employment is visible for those with a child aged 0-4 in 2013. This appears to be due to the 'benefit cap' reform discussed in Section 3 - there is no such rise visible for those who are not exposed to the policy. Since we control for an individual's exposure to the benefit cap, this should not affect our estimates.

Figure 2: Quarterly employment rates for single mothers by age of youngest child, 2006-2014



Note: Calculated using a weighted combination of LFS, FRS, and UKHLS data. The two control groups (age 0-4 and age 16-18) are shown in every panel. Each panel then also shows one treatment group. The vertical dashed lines indicate the implementation of conditionality for the treatment group in question. The tick marks on the horizontal axis indicate the first quarter of the year.

The basic descriptives are confirmed in the formal difference-in-difference specification for employment, the estimates of which are shown in the first subfigure of Figure 3. Reassuringly, we find no evidence of significant pre-trends. The impact on employment begins almost immediately after the implementation of the policy, and steadily increases for about 5-6 quarters. This is likely a combination of some single mothers taking time to find work, and existing IS claimants remaining eligible for the benefit for up to a year after the policy was implemented for otherwise identical new claimants (as discussed in Section 2). After six quarters the impact plateaus at around 4.4ppts and remains remarkably constant until the end of the window we examine - almost four years after treatment. Against a baseline non-employment rate of 36% this means that around one in eight of those who would otherwise have been out of work get into employment as a result of the policy. That the full effect of the policy is felt quickly is consistent with the existing evidence on similar policies: Card et al. (2018) report that the average employment effect of 'job search assistance' schemes changes little between 1-3 years after treatment.

Treated claimants can also respond to the policy by changing the benefits that they take-up. Most obvious is the take-up of IS or JSA, since anyone who gets into work will have to stop claiming. But even those who do not get a job may also end their claim. There are two effects at work here. First, there is a substitution effect: the cost of claiming IS or JSA has gone up as claimants have conditionality applied. Second, there is an income effect: the application of conditionality makes lone mothers worse off, which may lead them to claim another benefit which they are entitled to but hitherto had not claimed because the costs of take-up were too high.

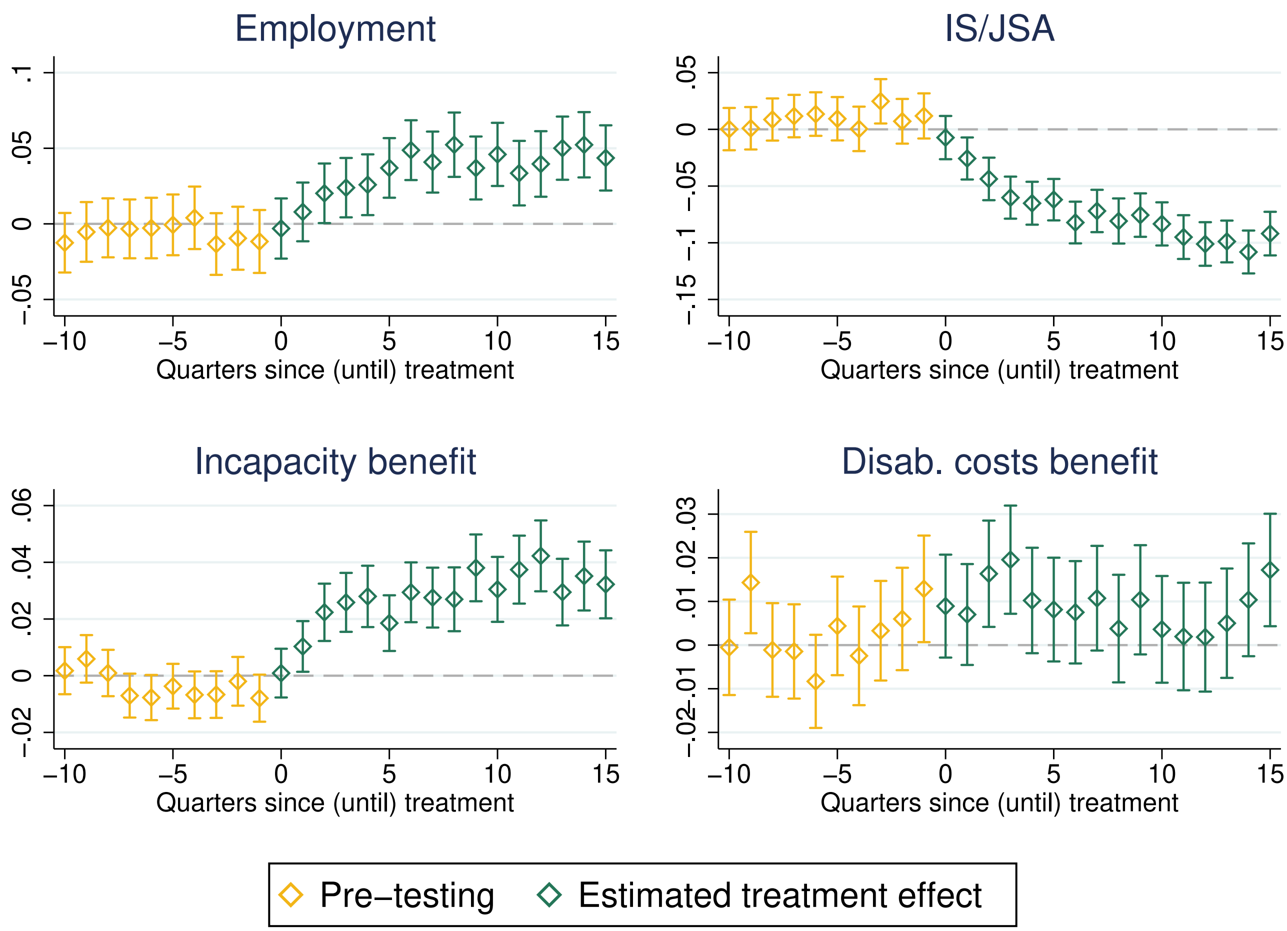
We therefore also investigate the impact of conditionality on benefit claiming. Figure 3 shows estimates of the impact of conditionality on three benefits, where the outcome variable is a dummy indicating whether the mother claims the benefit in question (note that y axes are not common across subfigures). Again we see little evidence of pre-trends for any of the three. The largest effect, unsurprisingly, is on the share claiming the out-of-work benefit to which conditionality is applied (IS/JSA), which falls by 8.9ppts (averaging across quarters 6 to 15, from a pre-policy rate of 33%). However, we see part of that decline is driven by a switch to incapacity benefit, claim rates of which increase by 3.3pts. Putting these results together with the employment estimates, the substantial majority of the decline in IS/JSA claiming is accounted for by claimants moving into work or switching to incapacity benefits.[10] Relatively few mothers, therefore, remain out-of-work but stop claiming any out-of-work benefit.

We also find a small but statistically significant 0.7ppt ($p < 0.01$) increase in the share claiming disability cost benefits (eligibility to which is independent of eligibility to any other benefit). Incapacity and disability benefits can be claimed simultaneously, and the rise in the latter seems to be mostly accounted for by those who begin a claim to the former - the increase in the share claiming at least one of the two is 3.4ppts - only a little more than the 3.3ppt increase in incapacity benefits alone. These changes are important from a fiscal perspective because, on average, incapacity benefit entitlement is £1,500 per year more for lone mothers that claim it than IS/JSA entitlement is for those that claim either of them; and disability cost benefit entitlement averages £4,000 per year among its lone mother claimants.[11] In the Appendix (Figure 8), we show no effects on claiming tax credits and housing benefit. This is likely related to the fact that those that receipt of IS/JSA guarantees eligibility for tax credits and (if the claimant rents their home) housing benefit, so baseline take-up is high - limiting the scope for additional increases.

[10] That is, $\underbrace{8.9\text{ppts}}_{\text{IS/JSA}} - \underbrace{4.4\text{ppts}}_{\text{employment}} - \underbrace{3.3\text{ppts}}_{\text{incap. ben.}} = 1.2\text{ppts}$

[11] The marginal claimant that is induced to claim these benefits because of conditionality may have less severe disabilities and thus entitled to less than average.

Figure 3: Impact of conditionality on employment and take-up of various benefits



Note: Sample is comprised of single mothers with a dependent child aged 0-18. Data used are pooled LFS, FRS and UKHLS surveys. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals indicated with whiskers. Note that y-axes are not common across subfigures

4.2 Decomposing the role of inflows and outflows

As discussed above, the existing literature has been able to say very little about the impact of conditionality on disincentivising (or 'deterring') inflows to benefits and/or unemployment, by reducing the utility of claiming out-of-work benefits. Such a deterrent effect is implicitly in the results above (together with the standard direct effect on outflows), but we now turn to explicitly quantifying its role, using the panel element of the LFS. To do this, we run event-study type specifications as before, but changing the sample and outcome so that our results can be interpreted as the impact on flow rates. To study the impact on job separation, we take the sample of single mothers who were in work in one quarter, and set the outcome variable to a dummy which takes the value one if they are no longer employed next quarter (someone who moves directly from one job to another, therefore, does not count as having experienced a job separation[12]). Equivalently, to study the effect on the job finding rate, we set the sample to those without a job in one quarter and the outcome variable to a dummy which is one if they have a job next quarter.

The results are shown in Figure 4. For visual clarity, we report the 6-15 quarter average treatment effects (the period over which the treatment effects in Figure 3 have generally plateaued); the full event-study estimates are available in Figure 9 in the Appendix.[13] The figure shows that the entirety of the change in employment is driven by conditionality increasing the job finding rate, which increases by around 3ppts, with almost exactly zero effect on job separation. However, the same is not true for IS/JSA, where we find a 'deterrent' effect on the inflow rate of 0.8ppts. Though this is much smaller than the effect on the IS/JSA outflow rate, because most single mothers are not on IS/JSA the inflow rate has an outsized effect on the overall share of mothers claiming at any one time. In fact, compared to only studying the impact on outflows as is standard, incorporating the deterrent effect on inflows raises the overall impact of conditionality on the IS/JSA claim rate by roughly 40%.[14] The figure also shows the impact on flows into or out of claiming either IS/JSA or incapacity benefits (i.e. any out-of-work benefit). The effect on inflows to any out-of-work benefit is 0.4ppts (not statistically significant), meaning that around half of the deterrent effect of conditionality on new IS/JSA claims is accounted for by diverting single mothers who would otherwise have begun an IS/JSA claim to instead claim incapacity benefits.

Thus, conditionality seems not to incentivise single mothers to keep their jobs (or quickly find another job after losing one). But once job separation has occurred, they are less likely to claim IS/JSA, in part because they sometimes choosing to claim incapacity benefits instead. This suggests that conditionality reduces the utility of claiming, and some unemployed mothers respond to this by accepting lower income for a period in return for avoiding the hassle of job search conditionality, or by applying for a benefit without conditionality requirements attached (incapacity benefits).[15]

[12] Those who have a very short spell of unemployment also will sometimes not be captured as having had a 'job separation', if the spell occurs entirely between interviews.

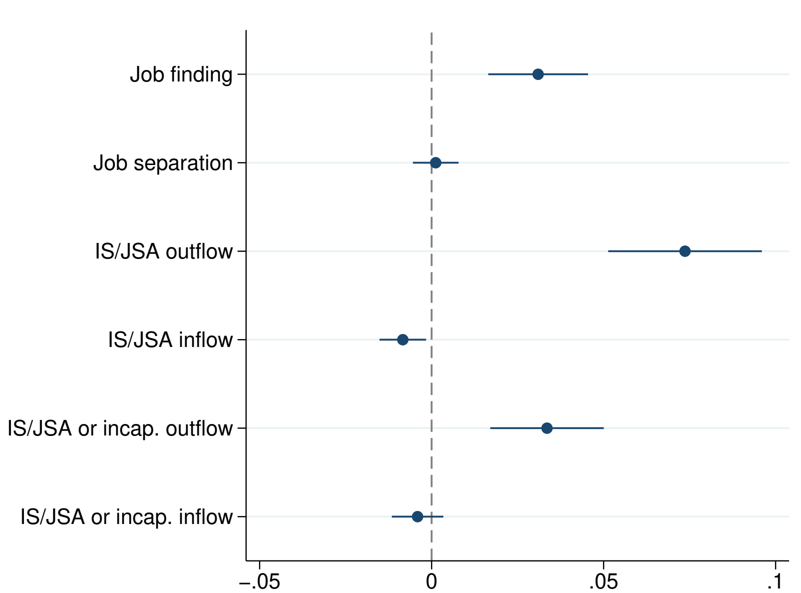
[13] There is a small amount of visual evidence of pre-trends in IS/JSA outflows; we suspect this is just noise, given that we do not see any such trends in overall IS/JSA claim rates in Figure 3, though it could be some mothers responding in anticipation of the loss of IS/JSA. If so, our estimates of the impact on the IS/JSA outflow rate would be downward biased.

[14] We calculate this as follows. Let the inflow rate be indicated with μ_i and the outflow rate with μ_o (we estimate both of these with pre-reform data). The claim rate in steady state before treatment is $CR(i,o) = \frac{\mu_i}{\mu_i+\mu_o}$. After treatment the flow rates become i' and o', which we calculate by adding on the treatment effect on flows to the baseline flow rates. We find that $\frac{CR(i',o')-CR(i,o)}{CR(i,o')-CR(i,o)} = 1.4$.

[15] Another possible explanation relates to sanctions. The conditionality regime means that someone who voluntarily quits their job, or is fired for misconduct, can be sanctioned upon claiming JSA. Functionally this means that they have to wait a few weeks or months before their JSA payments begin. Since IS has no conditionality, the same is not true for IS. One might therefore think that the decline in IS/JSA inflow rates could be driven by some newly unemployed mothers after the reform being simply unable to claim

This finding contrasts to McVicar (2010), the only other paper with the statistical power to study the effect of conditionality on benefit inflows. He finds no impact of reduced job search monitoring of claimants on inflows, which, as discussed above, may reflect the short-term and highly localised nature of the reduced monitoring that he studies: potential claimants may not know about the reduced monitoring in their local area at the time they become unemployed, and so not be any more likely to begin a claim. Even if they do know about the reduced monitoring, that it will only last for a short period may not significantly change the incentives around claiming. In our context the change in policy is a permanent one and applies across the whole country at the same time, making it an ideal setting to study the long term effects of conditionality on inflows.

Figure 4: Impact of conditionality on flows into and out of employment and IS/JSA



Note: Data used are the two-quarter LFS. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals indicated with whiskers.

JSA for a short period. To test this, we construct a variable indicating the likelihood that a mother would be sanctioned if she went to claim JSA, using the reported reason for why she left her last job. Those who were made redundant, had a temporary job that ended, or left for health reasons are coded as having a low likelihood of being sanctioned were they to try to claim JSA, as are those who have never worked; those who gave up work to look after family, for education, or for 'some other reason' a middling likelihood; and those who were dismissed, resigned, or took early retirement a high likelihood. Because neither 'voluntarily leaving a job' nor 'misconduct' are precisely defined in the rules, employment officers have some discretion in applying sanctions; these codings are our best approximations to the guidance they are given (Child Poverty Action Group 2008). We then run the difference-in-difference specification three times where the outcome variable is a dummy which is one if the mother both begins a new claim to IS/JSA and is in a specific one of the three sanction likelihood groups. Of the 0.8ppt effect on the inflow rate, 0.5ppts are accounted for by the low likelihood group, 0.2ppt by the middling likelihood group, and 0.1ppts by the high likelihood group (both of the last two being statistically insignificant). This suggests that sanctions relating the way the mother's last job ended are unlikely to be driving much of the overall impact of conditionality on the IS/JSA inflow rate.

4.3 Earnings and hours of the newly employed

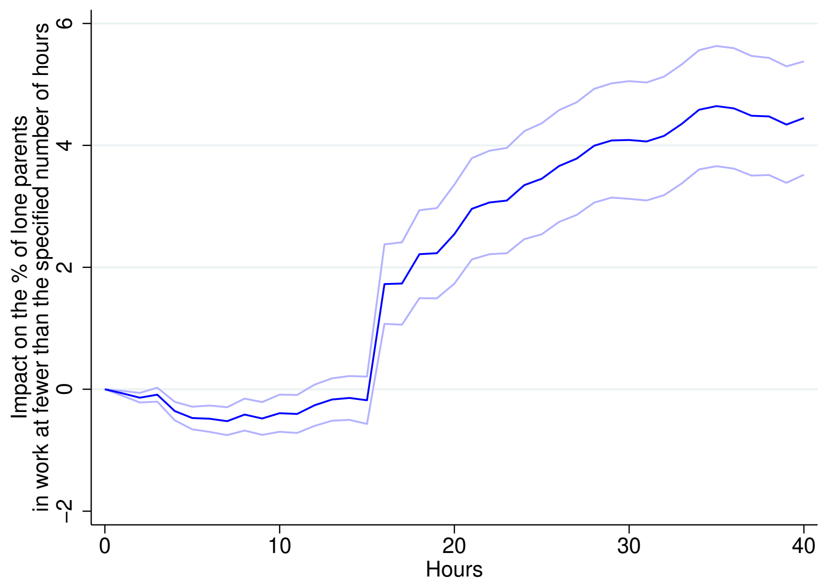
We now turn to understanding what kind of jobs the newly employed single mothers end up working in. To estimate the distribution of hours and earnings of the new workers, we adopt a simple approach: using the same specification as before, we let the outcome variable be a dummy which takes the value 1 if the mother is both in work and working no more than a certain number of hours per week (or earning no more than a certain amount). We run this many times with different cut-offs. This gives us the impact of the policy on the cumulative distribution of hours and earnings among workers. The results can be seen in Figure 5. We show the average impact at 6-15 quarters after treatment - a period over which Figure 3 shows the employment impact has plateaued.

Consistent with their financial incentives to work at least 16 hours per week (required for working tax credit eligibility), the entirety of the effect on employment was in jobs with hours at least that high. But, importantly, Figure 5a shows that there was no effect on full-time employment (30 hours plus). This is not simply because lone mothers with children aged 5-15 rarely work full-time - before the policy was introduced, around half of those in work were in full-time jobs. (The pre-policy distribution of hours and the impact of conditionality is shown in Figure 10 in the Appendix). The jobs that the claimants got into were also low paid ones - Figure 5b shows the mothers that were induced to work because of conditionality essentially all earned between roughly £5,000 per year (approximately the minimum wage for a 16 hours per week job) and £20,000 per year, with a median of £8,000.[16] To give a sense of scale, between 2008 and 2013 £8,000 was around the 13$^{\text{th}}$ percentile of earnings among all employees, and the 23$^{\text{rd}}$ among single parent employees. So, for both earnings and hours, the marginal workers that enter employment because of the policy have considerably weaker labour market outcomes than the average existing stock of single parent employees. That the whole effect was in part-time jobs also suggests that these jobs are not likely to be stepping stones to higher paid work well after treatment - as shown by Blundell et al. (2016), part-time work for women accumulates close to zero human capital. The low earning nature of the jobs the newly employed get into have important implications for the fiscal effects of the policy, to which we turn next.

[16]Here and throughout we express financial values in annualised terms (though the surveys record them at a weekly or monthly periodicity), in 2021 prices, deflated with the Consumer Price Index.

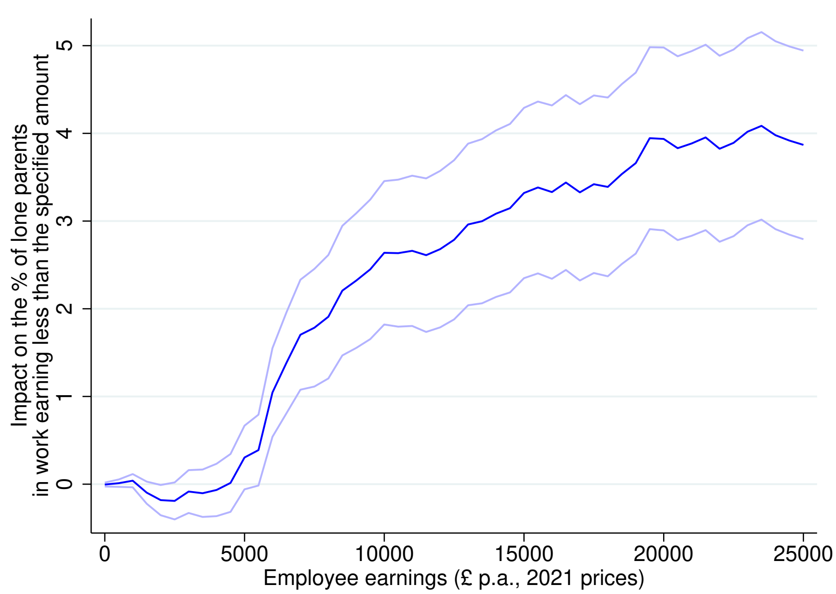
Figure 5: Hours and earnings of new workers

(a) Hours per week



Note: Sample is comprised of single mothers with a dependent child aged 0-18. Data used are pooled LFS, FRS, and UKHLS surveys. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals are shown in light blue.

(b) Employee earnings per year



Note: Sample is comprised of single mothers with a dependent child aged 0-18. Data used are pooled LFS, FRS, and UKHLS surveys. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals are shown in light blue. Effects of the policy on earnings from self-employment not included, as these are not recorded in LFS data. Earnings for employees in LFS who do not report their level of earnings (largely proxy respondents) are imputed, separately for each calendar year, using qualification, hours, region, age, occupation, and industry.

4.4 Incomes and fiscal effects

So far, we have shown that while conditionality pushed a significant number of single parents into work, a sizable share of those affected responded instead by claiming incapacity or disability cost benefits, increasing their benefit receipt. For those who did get into work, it was in part-time and low paid work. These findings have implications for the incomes of claimants as well as the fiscal effects of conditionality. Both of these are important for a full evaluation of conditionality: the impact on incomes is key for understanding how it affects living standards among a potentially vulnerable group, and the fiscal effect is critical for assessing the performance of conditionality in addressing moral hazard and fiscal externalities - the core justification for the policy.

Figure 6 shows the effect of conditionality on net income (i.e. income after taxes and benefits) as well as tax liabilities,[17] benefit receipt, and the net fiscal contribution (tax minus benefits). Despite the fact that the policy moved a sizable share of IS/JSA claimants into work, it did very little for tax liabilities: we estimate a (statistically insignificant) £130 average increase among all treated single parents. This reflects the low earnings of the jobs that claimants got into: as Figure 5b showed, the median pay of a claimant induced to work because of the policy was £8,000 per year, few earned more than £15,000, and essentially zero more than £20,000. These are low levels relative to the tax system - over the period we study, no tax was due on the first £6,900-8,700 of earnings. We also estimate only a small (and statistically insignificant) decline in average benefit receipt. This is for two reasons. First, the low level of earnings and hours among those induced to work because of the policy, together with the UK's high level of in-work benefits for single parents working part-time, means that many are entitled to similar levels of support in-work as they were out-of-work (see Figure 1 for an example mother's benefit entitlements as she increases her earnings). Second, those who claim incapacity benefit or disability cost benefit in response to the policy see their entitlements rise; as mentioned in Section 4.1, on average, incapacity benefit entitlement for single mothers that claim it is £1,500 per year more than IS/JSA entitlement for those that claim either of them; and disability cost benefit entitlement averages £4,000 per year among its single mother claimants.

Bringing together the effects on taxes and benefits, we find that the impact on the average net fiscal contribution is low. Our central estimate is a £270 effect per single parent and not statistically significantly different from zero. Once we account for hiring additional employment officers to deal with the higher JSA caseload, we estimate that this saving falls to £210 per single parent.[18] In contrast, the government expected to save £640 per claimant - an amount that we can reject at the 5% level.[19]

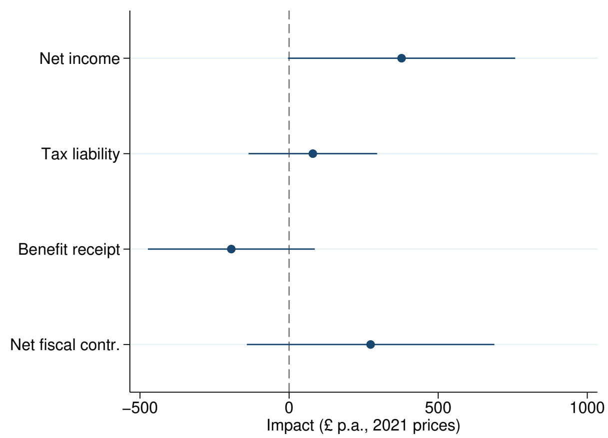
[17]We include income tax, payroll tax (National Insurance contributions; we include both that formally levied on the employee and the employer), and property tax ('council tax'). These taxes are directly measured in the data, with the exception of National Insurance contributions levied on the employer, which we calculate from the employee's reported earnings.

[18]Calculations available from authors upon request. Note that this does not include estate costs associated with hiring more employment officers, nor the cost of processing additional incapacity and disability cost benefit claimants. On the other hand, it also does not include additional VAT revenue from single parents having higher spending.

[19]Government costing derived from HM Treasury (2012) and DWP (2007).

The lack of a sizable fiscal effect suggests that the policy is ineffective at reducing fiscal externalities, undercutting the key justification for conditionality. This is critical from a welfare perspective: without much in the way of fiscal externalities, the impact of conditionality falls solely on the claimant herself, and in a standard model for her it can only be a negative as it removes a choice. Additionally, the policy is negatively affecting the insurance/life cycle consumption smoothing motive of out-of-work benefits, due to the decrease in the number of claimants.

Figure 6: Impact of conditionality on incomes and fiscal outcomes



Note: Sample is comprised of single mothers with a dependent child aged 0-18. Data used are pooled FRS and UKHLS surveys. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals are indicated with horizontal lines.

4.5 Life satisfaction, wellbeing, and mental health

The previous section indicated that the policy was likely welfare worsening, since it did little to alleviate fiscal externalities and, by removing a choice from claimants, can only have made them worse off. In this section we briefly investigate the second part of that claim directly, by examining the impact of the policy on reported life satisfaction, wellbeing, and mental health.

We use three measures from UKHLS. First, a life satisfaction score, based on a seven point Likert scale ranging from 'completely dissatisfied' to 'completely satisfied'. Second, a subjective wellbeing score derived from the General Health Questionnaire, based on twelve questions on issues such as self-confidence, loss of sleep, general happiness and ability to overcome difficulties (Jackson 2007). Third, a mental health score (SF-12) based on twelve questions taken from the (36 question) SF-36 Medical Outcomes Study (Jenkinson et al. 1997, shows that results using the twelve questions are almost identical to those using 36). We refer to this suite of measures as 'wellbeing outcomes'. We normalise all wellbeing outcomes to have a mean of zero and a standard deviation of

Table 2: Impact of conditionality on life satisfaction, wellbeing and mental health

	(1) Life satisfaction score	(2) Wellbeing score	(3) Mental health score
Treatment effect	-0.012	0.084	-0.072
	(0.065)	(0.071)	(0.066)
Observations	6492	6480	6718
Pre-LPO mean	-0.235	-0.268	-0.186

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Sample is comprised of single mothers with a dependent child aged 0-18, excluding those whose youngest child is 12-15. Data used is the UKHLS survey. Estimated in a difference-in-difference framework using Borusyak et al. (2021). The treatment effect measured is the average impact 6-15 quarters after treatment. All outcomes normalised to have mean of zero and standard deviation of one among mothers; the 'pre-LPO mean' cells are non-zero because the normalisation is done among all mothers, whereas the regression is run on single mothers alone. Higher scores indicate better life satisfaction, wellbeing, or mental health. Standard errors, clustered at the individual level, are in parentheses.

one among mothers, and invert them where necessary such that a higher score indicates greater life satisfaction or better wellbeing or mental health. Because the first wave of UKHLS begins in 2009, we cannot analyse the first phase of the LPO which affected those whose youngest child was aged 12-15, as it was implemented at the end of 2008. We therefore exclude this group from the analysis.[20]

We use the same empirical strategy as for the other outcomes, the results of which are shown in Table 2. Despite the fact that the policy removed a choice from claimants, we find no significant evidence of a worsening of life satisfaction, wellbeing or mental health.

One possible explanation for this is that the average impact across all single mothers is hiding important heterogeneity between those who do and do not get into work as a result of the reform. Because the empirical approach

[20]There is also one paper in the public health literature (Katikireddi et al. 2018) which studies the health impact of the LPO reform (including mental health, using the same SF-12 measure and UKHLS data we use). They use a panel difference-in-difference strategy, where the treatment group is those who will be treated over the next year, and the control group is lone parents who will not be treated over the next year. We are able to study the same issue using weaker identification assumptions. First, their control group includes those who had been treated slightly earlier, meaning that they require close to instantaneous treatment effects for the control group to be valid (an assumption which is inconsistent with our findings on the dynamic treatment effects above). By comparison, we only include never-treated or not-yet-treated lone parents as controls. Second, since they analyse outcomes only over a year, again they require treatment effects to plateau very quickly to see the full impact of the policy; we are able to look over a much longer period. Third, by comparing in a *panel setting* treated lone parents to those with older or younger children, they require a strong parallel trends assumption - that the change in mental health of (for example) single parents when their child ages from 7 to 8 is the same as for when they age from 1 to 2. Because we treat the data as a repeated cross section, there is no ageing 'within' our treatment and control groups in this way. That we can apply weaker assumptions may be why Katikireddi et al. (2018) find negative impacts of the LPO on mental health, in contrast to our null results.

we have relied upon so far treats the data as a repeated cross-section, it is not well suited to studying this possibility. Instead, we use a panel triple difference-in-difference approach, differencing across time, single/couple status, and age of the youngest child. Because we are focused on the impact of introducing conditionality, we include in our sample only those who were not subject to conditionality last period.[21] We then begin with the following specification:

$$y_{i,t,s,g} = \gamma^0_{s,t} + \gamma^1_{g,t} + \gamma^2_{g,s} + \delta C_{g,s,t} + \epsilon_{i,t} \tag{3}$$

Where $y_{i,t,s,g}$ is outcome y for individual i at time period t with single/couple status s and whose youngest child is indicated by g (here we use the same treatment groups as in the main analysis). $\gamma^0_{s,t}$, $\gamma^1_{g,t}$, and $\gamma^2_{g,s}$ are fixed effects controlling for time invariant differences between single and coupled mothers with children of different ages, and changes in the impact of single/couple status and youngest child age over time. $C_{g,s,t}$ is a dummy taking the value one if an individual with single/couple status s, a youngest child aged g, observed at time t would be subject to conditionality if they claimed IS/JSA, and so δ is the parameter of interest. For couples, this is always zero. $\epsilon_{i,t}$ is the error term. This approach identifies the impact of conditionality on y so long as trends for singles with a child of a particular age do not differ from trends for coupled mothers with a child of the same age, beyond the single/couple status specific trends and child age specific trends.

Reassuringly, this approach gives very similar results to those in Table 2 (i.e. small and statistically insignificant), despite using a different identification strategy.[22] However, it of course tells us nothing about how outcomes vary for those that do and do not get into work. We amend equation 3 in three ways. First, we include a term interacting a four way indicator variable ($f_{i,t}$) of employment flows into and out of work with an indicator for being single. Second, we interact the $C_{g,s,t}$ term with the employment flows indicator. Third, we include last period's outcome on the right hand side. This gives the following specification:

$$y_{i,t,s,g} = \gamma^0_{s,t} + \gamma^1_{g,t} + \gamma^2_{g,s} + \beta_f \mathbf{1}(s = \text{single}) + \delta_f C_{g,s,t} + \omega y_{i,t-1,s,g} + \epsilon_{i,t} \tag{4}$$

The coefficients from this amended regression cannot be confidently interpreted as causal, because we do not have

[21] This is important for when we introduce heterogeneity by employment flows: for example, those who move from unemployment to employment when they are under conditionality in both periods might be more likely to see improvements in wellbeing than those who have the same labour market change without conditionality in the first period. But that improvement does not mean that conditionality is improving the wellbeing of those that move into work - instead it is worsening the wellbeing of being out of work.

[22] We do not use this approach more generally because, since it can only be applied with the UKHLS panel, it gives a smaller sample of treated mothers - this would be especially true for a dynamic version of equation 3 where attrition would further erode the sample

identifying variation in employment flows. In particular, perhaps there is reverse causation, where conditionality is more likely to push those into work whose mental health was improving anyway than those whose mental health was deteriorating. However, what this approach does allow us to do is to establish the association between employment flows and wellbeing outcomes for single mothers without conditionality (β_f), and how that changes when conditionality is applied (δ_f). This amounts to splitting the overall (identified) treatment effect (δ) by observed changes in employment, shedding light on what drives the overall result.

The results of this exercise are shown in Table 3 (we show the results for the β_f and δ_f terms as these are the key ones). The top three rows of the table show the association between employment flows and wellbeing outcomes for single mothers in the absence of conditionality. The excluded dummy is being out-of-work (referred to in the table as 'unemp') in both periods. We find clear evidence that getting into work is associated with an improvement in wellbeing (relative to remaining unemployed).

The next four rows show how the the employment flow-wellbeing relationship changes when conditionality is applied. Remaining unemployed after the application of conditionality (row 4) is associated with a significant decline in wellbeing for two of our measures ($p = 0.101$ on the other), relative to remaining unemployed without conditionality. Importantly, we see no significant change in the impact of getting into work (row 5).

This suggests that the overall muted impact of conditionality on wellbeing is the combination of offsetting heterogeneous effects. Those who remain out of work when conditionality is applied experience a worsening in wellbeing. Those who get into work see an improvement in wellbeing - of a similar magnitude to that experienced by those who get into work without conditionality. Again, we cannot rule out reverse causation driving some of this effect, but if these effects were causal, they would indicate that conditionality 'redistributes' wellbeing from those that remain out-of-work to those who get into work – and the former group of course are especially likely to face additional difficulties anyway. Moreover, since those single mothers who get into work could have done so before the application of conditionality, one interpretation of these results is that single mothers have biased perceptions about employment: getting into work may be better than they expect.

4.6 Heterogeneity by age of youngest child

So far we have focused on the impact from all four 'phases' of the LPO. We now briefly examine hetereogeneity in responses by the age of the youngest child and thus when treatment occurred. Figure 7 splits treatment effects on some of the key variables by the age of the mother's youngest child. There are quite sharp differences. Those with younger children tend to see larger employment and IS/JSA responses, especially those with a child age 5-6 - though no group sees a significant increase in full-time work. The larger response for those with younger children is consistent with their lower baseline employment rates (see Figure 2) and higher IS/JSA rates, meaning that there

Table 3: Heterogeneity in the impact of conditionality on life satisfaction, wellbeing and mental health

	(1) Life satisfaction score	(2) Wellbeing score	(3) Mental health score
β (Unemp $\rightarrow$ Emp)	0.229**	0.231**	0.229**
	(0.101)	(0.0918)	(0.0936)
β (Emp $\rightarrow$ Unemp)	-0.154	-0.0153	-0.0226
	(0.133)	(0.123)	(0.124)
β (Emp $\rightarrow$ Emp)	0.0598	0.0183	0.0829
	(0.0499)	(0.0515)	(0.0505)
δ (Unemp $\rightarrow$ Unemp)	-0.166	-0.269***	-0.259**
	(0.101)	(0.103)	(0.105)
δ (Unemp $\rightarrow$ Emp)	-0.0339	0.0744	0.106
	(0.156)	(0.151)	(0.138)
δ (Emp $\rightarrow$ Unemp)	0.0587	-0.620**	-0.144
	(0.257)	(0.282)	(0.247)
δ (Emp $\rightarrow$ Emp)	0.0503	0.0386	-0.0473
	(0.0942)	(0.0934)	(0.0910)
Observations	29131	29044	29038

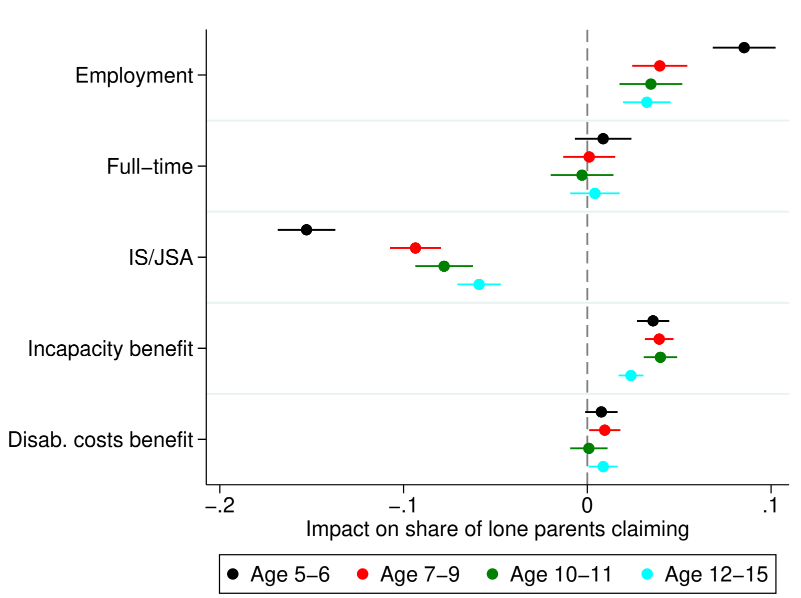
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Sample is comprised of mothers with a dependent child, who, at $t-1$, would not have been subject to conditionality had they been single and claiming IS/JSA. Data used is the UKHLS survey. Estimated using a variant of equation 3 where right hand side terms are interacted with a four way variable indicating flows into and out of employment. 'Emp' and 'unemp' refer to employment and any form of non-employment, respectively. The terms interacted with S show the association between the employment flow in question and the outcome for singles who are not subject to conditionality at t; the base category is (Unemp $\rightarrow$ Unemp) x S. The terms interacted with S and C show how that association changes when conditionality is applied at t. The γ_1 terms from equation 3 are not shown. All outcomes are normalised to have mean of zero and standard deviation of one among mothers. Higher scores indicate better life satisfaction, wellbeing, or mental health. Standard errors, clustered at the individual level, are shown in parentheses.

is more scope for response when the policy is applied. Moreover, those women who are still not working when their children are somewhat older are perhaps the least inclined to work and have experienced the greatest deterioration in human capital - and hence are the furthest from the labour market. Consistent with that hypothesis, those with older children who stop claiming IS/JSA because of the policy are much more likely to begin to claim incapacity benefits. For example, of the reduction in IS/JSA among those with the youngest children, 24% is offset by higher incapacity benefit take-up, but for the other groups that figure is 40-50%. Though we do not have the power to study heterogeneity in impacts on net fiscal contribution due to smaller sample size, this is suggestive that conditionality may have been even less effective in correcting the fiscal externality for those with the oldest children.

Figure 7: Impact of conditionality on various outcomes, split by age of the youngest child



Note: Sample is comprised of single mothers with a dependent child aged 0-18. Data used are pooled FRS and UKHLS surveys. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals are indicated with horizontal lines.

4.7 Parental responses and effects on children

Our results have focused on how parents respond to the policy. But their children are likely to be indirectly affected, and indeed such potential impacts may themselves shape how the parents respond. We briefly discuss how this may affect our results, and avenues for future research.

The literature has conceptualised how work-incentivising benefit reforms affect children's future outcomes as a trade-off between two key mechanisms (Agostinelli and Sorrenti 2021): encouraging parents to move into work reduces parental contact time or the quality thereof (a "time substitution effect"), but also increases income (an "income effect"). Job search conditionality reduces parental contact time not only for those who get into work, but also for those who remain out of work and spend (more) time looking for work. A rich array of papers studying various settings have found important negative time substitution effects on children's educational achievements (Baum II 2003; Ruhm 2004; Bernal and Keane 2011; Del Boca et al. 2014; Bono et al. 2016; Løken et al. 2018), economic outcomes (Carneiro et al. 2015) and behavioural development (Agostinelli and Sorrenti 2021). But (as we have shown) job search conditionality on average raises income, with more parents working or claiming (more generous) incapacity or disability benefits - though it reduces income for those who are sanctioned, or choose not to claim at all in response to the policy. Most of the previous research indicates a small positive effect of persistent increases in income on child achievements (Dahl and Lochner 2012; Løken et al. 2012; Hoynes et al. 2016), though virtually no effect coming from changes in transitory income (Blau 1999; Price and Song 2018). In addition, role model and information effects of parents claiming benefits or being in work could also affect children's long term outcomes (Dahl et al. 2014). A final channel through which children could be affected is free school meals, entitlement to which is (in the UK) dependent upon a child's parents claiming IS/JSA or incapacity benefits (and, therefore, not being in work). Existing research shows positive effects of free school meals on attainment (Brown et al. 2012).

These effects could themselves feed back to the parents and drive some of the responses we have studied. For example, a parent might be concerned that having to substitute away from contact time with the child towards searching for work could affect the child's outcomes, in line with the research discussed above. In response, they may try to avoid such job-search requirements by claiming incapacity benefits, or offset the time substitution effects by getting a job and increasing family income (with the attendant income effect). Conversely, a parent who does not want to lose free school meal entitlements for their children faces, implicitly, a higher tax rate on getting into work, and so might be more inclined to remain on IS/JSA even after the implementation of conditionality, or to try to claim incapacity benefits, rather than getting into work.

As well as driving some of the parental responses to conditionality, these effects on the children are themselves of course an additional ingredient for a comprehensive assessment of the policy. Ongoing research by the authors studies the (possibly heterogenous) impact of the LPO on children's outcomes using a dataset covering the universe of pupils' attainment (the National Pupil Database). Relatively little is yet known in the literature about whether the effects of welfare reforms on children are transitory or persistent, what precise changes in parental behaviour drive the income and substitution effects, and what family characteristics make children most vulnerable to these changes. This research will make progress on these key questions.

5 Identification challenges and sensitivity checks

We now turn to a number of sensitivity checks and challenges to our empirical set-up.

Beginning with sensitivity checks, Table 4 repeats our estimates of the LPO's impact on key outcomes under our baseline specification, as well as under a number of alternatives. Our first check is excluding demographic controls from the specification and thus the imputation of the counterfactual outcome. Second, we try two variations on the 'never treated' control groups. In our baseline results, these groups are single mothers with a youngest child aged 0-4 or 16-18. One alternative is to exclude those whose youngest child was aged 3-4, both because they might respond to the policy in anticipation of their child turning 5, and because in 2010 the government expanded free childcare for 3 and 4 year olds from $12^1/_2$ to 15 hours per week. We also experiment including single women without children as a control group, giving us a much larger control sample (though likely at the cost of reducing the likelihood that the common trends assumption holds). Third, we use sample weights. Fourth, we cluster standard errors at the group level - which significantly increases standard errors since we only have six groups (four treated, two control). Fifth, we use the standard two way fixed effects estimator rather than Borusyak et al. (2021). Sixth, we include a linear group-specific time trend, estimated off pre-LPO data; counterfactual outcomes are imputed on the assumption that this trend would have continued had the LPO not been implemented.

In virtually all cases the results are very similar, both in terms of statistical significance and magnitude. The one clear exception is that the estimated impact of the policy on net income is much larger when the control group includes single women without children. But this is an outcome where this control group is probably less than ideal, since single women without children are much less reliant on benefits, and have a considerably lower level of income, than those with children.

Table 4: Sensitivity checks on key results

	Employment	Full-time	IS / JSA	Incapacity benefit	Disab. costs benefit	Net income	Net fiscal contr.
Baseline specification							
Impact of LPO	0.044***	0.003	-0.089***	0.033***	0.007***	377.228*	273.405
	(0.004)	(0.004)	(0.004)	(0.002)	(0.003)	(194.420)	(211.776)
Excluding demographic controls							
Impact of LPO	0.046***	0.005	-0.091***	0.033***	0.008***	509.574**	427.861*
	(0.005)	(0.004)	(0.004)	(0.002)	(0.003)	(200.018)	(219.100)
Including single women without children in control group							
Impact of LPO	0.054***	0.005	-0.107***	0.037***	0.011***	1651.912***	356.270*
	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(169.194)	(187.437)
Excluding those whose youngest child is aged 3-4 from control group							
Impact of LPO	0.049***	0.003	-0.089***	0.030***	0.005*	401.812*	258.392
	(0.005)	(0.005)	(0.004)	(0.002)	(0.003)	(209.026)	(226.532)
Using sample weights							
Impact of LPO	0.048***	0.004	-0.096***	0.035***	0.008***	572.018**	208.040
	(0.005)	(0.004)	(0.004)	(0.002)	(0.003)	(222.241)	(238.143)
Clustering standard errors at the group level							
Impact of LPO	0.044***	0.003	-0.089***	0.033***	0.007***	377.228	273.405
	(0.013)	(0.003)	(0.025)	(0.008)	(0.003)	(297.527)	(367.858)
Two way fixed effects estimator							
Impact of LPO	0.049***	0.009**	-0.092***	0.032***	0.005**	394.089*	344.513
	(0.005)	(0.005)	(0.004)	(0.002)	(0.003)	(208.677)	(224.853)
Including a group specific linear time trend							
Impact of LPO	0.051	0.015	-0.109	0.044	0.005	744.034	-211.451
	(-)	(-)	(-)	(-)	(-)	(-)	(-)

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The Stata command which implements the Borusyak et al. (2021) estimator currently does not calculate standard errors in a repeated cross-sectional setting when a group specific linear time trend is included, and so standard errors are omitted for that check; we will calculate them for future versions of this paper. Treatment effects shown are the average impact 6-15 quarters after treatment. Sample is comprised of single mothers with a dependent child aged 0-18. Data used are pooled LFS, FRS, and UKHLS surveys; the income and fiscal contribution outcomes only use the latter two datasets. Estimated in a difference-in-difference framework using Borusyak et al. (2021).

Finally, our identification strategy relies upon the treatment not causing single mothers to select into or out of treatment or control groups in a systematic way. But the reform changes incentives around partnership and fertility. After the implementation of the LPO, a single mother no longer loses the ability to claim out-of-work benefits without conditionality when she forms a couple (because, coupled or not, she now cannot claim IS). So the reform incentivises single mothers to form a couple, and disincentivises already coupled mothers from breaking-up. The incentives around fertility are more ambiguous. In a static sense, after the LPO is applied to a lone mother she can avoid conditionality by having another child (since then her youngest child will be zero). But in the dynamic incentive goes the other way - whereas before the LPO having a child would buy a lone mother the right to claim out-of-work benefits without conditionality for 16 years, after its full implementation it only buys 5 years. If mothers change their fertility and partnership choices in response to these incentives, that would bias our estimates (it would make our employment estimates upward biased, for example). We develop and apply a method for testing the impact on the reform on fertility and partnership choices, the details and results of which are described in Appendix D. Reassuringly for our strategy, we do not find any significant impact, either economically or statistically.

6 Conclusion

Work search conditionality requirements for benefit claimants are a widely - and increasingly widely - used policy tool to encourage claimants back into work, and to address the fiscal externalities associated with providing such benefits. The extant literature on this tool is large, but is focused on the effect on changing the likelihood that existing claimants exit benefits and begin employment - limiting its usefulness for comprehensively assessing the policy. In this paper, we have used a UK reform allowing us to make two key contributions. First, we show that incorporating the 'deterrent' effect of conditionality on discouraging newly disemployed workers from beginning a claim is large, increasing the impact of conditionality on benefit caseload by 40% - suggesting, on that margin, that the policy is more effective than the existing literature gives it credit for. But, second, we show that claimants get into part-time, low paid jobs, with limited tax liabilities and high benefit entitlements. Others substitute to more expensive incapacity or disability benefits. Together these effects mean that the fiscal consequences of the policy are small, and so as a tool for addressing fiscal externalities it is weak. In addition, we have provided evidence that while those that get into work following the implementation of conditionality see an improvement of mental health and wellbeing, those that remain out-of-work see a deterioration. Moreover, some out-of-work individuals stop claiming benefits as a result of the policy, thereby affecting the capacity of the benefit system to provide insurance.

Taken as a whole, these results show that while conditionality can have sizable impacts on the typically measured outcomes - employment and benefit caseload - a more comprehensive assessment paints a gloomier picture. The interaction with the existing tax-benefit system is relevant here - a system which makes large net transfers to those in part-time jobs (similar to the UK's system for single parents) may strengthen financial incentives to work, but limits the value of conditionality in addressing fiscal externalities.

Of course, though our analysis is more comprehensive than that available to date, there is more that could be done. The impact of conditionality on the children of claimants could be an important channel, with conditionality changing household income, available parental time, and role model effects. Building on the conclusions of this paper and directly studying how conditionality interacts with financial incentives to work would also provide critical evidence for policymakers looking to design the optimal policy mix.

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Appendices

A Additional institutional details

Out-of-work benefit eligibility

The main text provides the key information about out-of-work (IS or JSA) benefit eligibility. Here we provide some minor additional details:

- There is a contributory version of JSA. This is available to unemployed workers with (fairly minimal) recent work histories. It provides identical support to the means-tested version (it is not related to prior earnings) and can be claimed for 6 months. The means-tested version is the one that is most relevant for our purposes, since that is the direct alternative to IS (anyone eligible for IS is also eligible for means-tested JSA).
- From 2013-14 some out-of-work claimants were placed on Universal Credit (UC), which eventually will replace IS and (the means-tested version of) JSA, along with housing benefit and tax credits. However, rollout of UC has been slow and initially was focused on those without children, such that over the period we analyse virtually no single parents received UC. In the handful of cases where someone reports receiving UC in the data, we code them as receiving the equivalent means-tested benefits that they would have been entitled to in the pre-UC system.
- Although we describe IS and JSA as 'out-of-work' benefits, technically a claimant could be in work at a low level of hours, but their benefits would be taxed away at a rate of 100% above a very small allowance.
- Over the period we study some people on incapacity benefits, and carers of disabled people, were potentially eligible for IS. These people are not subject to the LPO. If we observe someone claiming incapacity benefits and IS, we code them as an incapacity benefit recipient and not an IS recipient.
- IS claimants had to attend a 'work focused interview' every three or six months, or risk being sanctioned. In contrast to the requirements placed on JSA claimants, this was very light touch. Claimants had to discuss their employability and what they might be willing to do to enhance their employment prospects. They were required to assist in creating an 'action plan' where, together with an advisor, they agreed on things that are 'reasonable' for them to do - though there were no penalties on failing to do any of the things in the action plan. Child Poverty Action Group (2008)
- In mid-2013, a policy (the 'benefit cap') was brought in which capped the maximum benefit entitlement for some out-of-work families. In practice, it generally only affected those who were both renters and had several children, since only they were able to get enough out-of-work benefits to hit the cap (Joyce 2012). While not many were affected by the cap - only about 13,500 single parents (0.7%) at any point in time

during our period - those that were affected saw large out-of-work income falls, and there is evidence that it led to an increase in employment (DWP 2014). For our purposes, the key issue here is that those with younger children both have more dependent children on average, and are more likely to be renters. This means that these groups are more likely to be affected by the policy (though again, very few people in total were affected), perhaps causing differential trends between single women with differently aged children. To account for this, we construct a variable indicating exposure to the benefit cap (being a renter with at least two children, observed after the implementation of the benefit cap), and control for it in our estimation. It turns out that this has only a tiny effect on the estimated impact of the policy on employment (less than 0.01ppts).

Roll-out dates of the Lone Parent Obligations reform

Table 5: Roll-out dates of the Lone Parent Obligations reform

Phase	Single parents affected (by age of youngest child)	Roll-out begins (no new claims)	Roll-out ends (last claimant moved)
1	12-15	Nov 2008	Nov 2009
2	10-11	Oct 2009	Oct 2010
3	7-9	Oct 2010	Oct 2011
4	5-6	May 2012	Nov 2012

Note: ‘Roll-out begins’ is the date from which new claims for IS are disallowed, and when existing claimants begin to be moved over to JSA.

Nature of job search conditionality

The below information is largely derived from Child Poverty Action Group (2008).

Claimants to JSA “are required to look for work actively, report to a welfare office at least fortnightly, and can be sanctioned for not making sufficient efforts to look for work” (2013). In order to be considered that you are actively looking for work “you must take, such ‘steps’ as you can reasonably be expected to have to take in order to have the best prospects of securing employment. You are normally expected to take more than two steps during a week” (2013).

A ‘step’ has a broad definition including: applying for jobs, registering with an agency, preparing a CV, asking for a reference from a previous employer, looking for information about an occupation, and other similar actions.

The government considers that looking for work should be a full-tiSSme job for these individuals.

Claimants can be 'sanctioned' for a number of reasons, including failure to attend an interview at the welfare office, refusing to take an action they were instructed to by their employment officer, or failing to accept a job offer. Claimants who are sanctioned receive no JSA for a period of time, with the length depending on the offending action and the discretion of the employment officer. In 2007-08 sanctions varied from 1 to 26 weeks, and by 2014-15 that had increased to 4 to 156 weeks. Sanctioned claimants (especially those with children) can, at the discretion of the welfare office, claim 'hardship payments', which are equivalent to 60% of the JSA amount.

B Studies examining job-search conditionality

Table 6: Studies examining job-search conditionality

Study	Intervention	Estimation sample	Estimation method
Abbring et al. (2005)	Sanctions	Newly unemployed	TOE
Arni and Schiprowski (2019)	Monitoring	Stock of unemployed	IV (caseworker)
Ashenfelter et al. (2005)	Monitoring	Newly unemployed	RCT
Avram et al. (2018)	Mandatory SA	Stock of unemployed	DiD (cohort)
Bernhard and Kopf (2014)	Mandatory SA	Stock of unemployed	Correlation
Black et al. (2003)	ALMP threat	Newly unemployed	RCT*
Blundell et al. (2004)	Mandatory SA	Newly unemployed	DiD (geog & age)
Boockmann et al. (2014)	Sanctions	Unemployed workers	IV (office stringency)
Borland and Tseng (2008)	ALMP threat	Stock of unemployed	DiD (age)
Breunig et al. (2003)	Mandatory SA	Stock of unemployed	RCT
Centeno et al. (2009)	Mandatory SA	Stock of unemployed	DiD (geog)
Cobb-Clark et al. (2006)	Mandatory SA	Stock of unemployed**	RCT
Cockx and Dejemeppe (2012)	Monitoring	Stock of unemployed	RD (age)
Crepon et al. (2005)	Mandatory SA	Newly unemployed	TOE
Dolton and O'Neill (2002)	Mandatory SA	Newly unemployed	RCT
Dolton and O'Neill (1996)	Mandatory SA	Newly unemployed	RCT
Fougere et al. (2005)	Mandatory SA	Stock of unemployed	Structural
Freedman et al. (2000)	Various	Various unemployed	RCT
Gorter and Kalb (1996)	Mandatory SA	Newly unemployed	RCT
Hérault et al. (2020)	Mandatory SA	Whole population	RD (age)
Jensen et al. (2003)	ALMP threat	Stock of unemployed	Correlation
Joona and Nekby (2012)	Mandatory SA	Newly unemployed	RCT
Klepinger et al. (2002)	Monitoring	Newly unemployed	RCT
Lalive et al. (2005)	Sanctions	Newly unemployed	TOE
Lammers et al. (2013)	Mandatory SA	Newly unemployed	DiD (cohort)
Malmberg-Heimonen and Vuori (2005)	Mandatory SA	Stock of unemployed	Correlation
Manning (2009)	Mandatory SA	Stock of unemployed	DiD (cohort)
McVicar (2010)	Monitoring	Whole population	DiD (geog)
McVicar (2008)	Monitoring	Newly unemployed	DiD (geog)
Micklewright and Nagy (2005)	Monitoring	Newly unemployed	RCT
Petrongolo (2009)	Mandatory SA	Newly unemployed	DiD (cohort)
Rosholm and Svarer (2008)	ALMP threat	Stock of unemployed	TOE
Svarer (2011)	Sanctions	Newly unemployed	TOE
van den Berg and van der Klaauw (2006)	Mandatory SA	Newly unemployed	RCT
van den Berg et al. (2022)	Sanctions	Newly unemployed	TOE
van den Berg et al. (2012)	Monitoring	Stock of unemployed	TOE
van den Berg et al. (2004)	Sanctions	Newly unemployed	TOE
Weber and Hofer (2004)	Mandatory SA	Newly unemployed	TOE

Note: ALMP (active labour market programme) threat = claimants are told they will soon have to participate in an ALMP if they remain on benefits. Mandatory SA (search assistance) = claimants must get job counselling (e.g. help finding vacancies), and provide evidence of job search, to continue getting benefits. Sanctions = study examines the impact of sanctions on those who are in fact sanctioned or directly warned that they are at risk of being sanctioned. Monitoring = similar to mandatory search assistance, the employment office (more closely) monitors the job search of claimants.

The estimation sample is in some cases conditional on other characteristics. For example, several papers examine the stock of *long term* unemployed claimants; one newly unemployed *young* claimants; and one the whole population of *partnered women*. In all cases we ignore these other characteristics and report simply whether the paper studies people with such characteristics regardless of unemployment status (whole population), or whether they focus attention on those with these characteristics who are newly unemployed or part of the stock of unemployed.

RCT = randomised controlled trial. DiD = difference-in-difference. TOE = timing-of-events, as discussed in Abbring and Van Den Berg (2003). RD = regression discontinuity. IV = instrumental variables. Correlation = paper examines differences in outcomes between those subject to a intervention and not, without (quasi-)experimental variation, possibly controlling for observable differences. Structural = a structural model of claimant behaviour is used.

* Black et al. (2003) is not strictly an RCT; instead, in their setting, capacity constraints mean that employment offices sometimes randomly pick which claimants are treated. ** Cobb-Clark et al. (2006) in fact examine the *partners* of the stock of unemployed men.

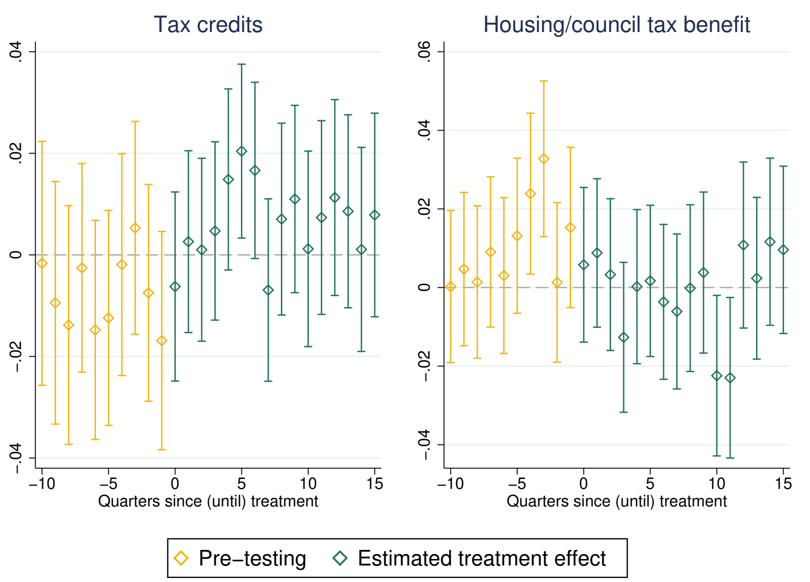
C Additional descriptives and analysis

Table 7: Descriptive characteristics of single mothers, 2004-2016 (2009-2016 for UKHLS), unweighted

	All	LFS	FRS	UKHLS
Highest qualification				
Still in edu.	0.01	0.00	0.02	0.02
GCSEs	0.59	0.60	0.58	0.48
A-levels	0.17	0.17	0.17	0.19
Degree	0.23	0.23	0.24	0.31
Housing tenure				
Social renter	0.42	0.42	0.45	0.43
Private renter	0.22	0.23	0.22	0.22
Homeowner	0.35	0.35	0.34	0.34
Age of youngest child				
0-4	0.26	0.26	0.27	0.24
5-6	0.12	0.12	0.11	0.11
7-9	0.17	0.17	0.17	0.16
10-11	0.11	0.11	0.11	0.11
12-15	0.21	0.21	0.21	0.22
16-18	0.13	0.13	0.12	0.16
Benefits claimed				
IS/JSA	0.30	0.30	0.32	0.27
Incapacity benefit	0.05	0.05	0.05	0.06
Disab. cost benefit	0.06	0.06	0.08	0.09
Housing benefit	0.46	0.45	0.52	0.53
Tax credits	0.78	0.78	0.75	0.83
Age	38.47	38.45	38.12	39.36
Number of children	1.74	1.74	1.74	1.74
White	0.86	0.86	0.88	0.73
Employed	0.61	0.62	0.60	0.63
Inactive	0.31	0.31	0.34	0.26
Hours worked	17.36	17.32	17.01	18.54
Employee earnings	10,072	10,137	9,863	10,082
Benefit receipt	12,598	.	12,607	12,583
Tax paid	3,861	.	3,760	4,035
Net income	21,725	.	21,855	21,503
Sample size	195,627	163,351	20,345	11,931

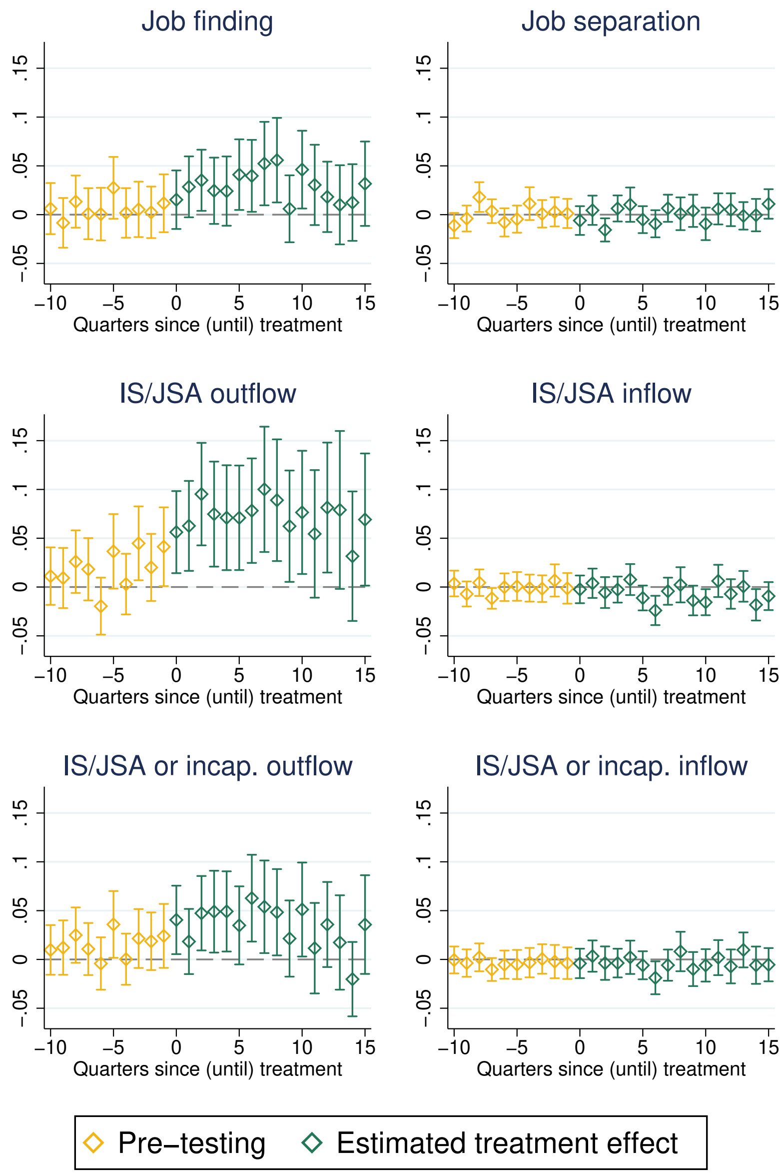
Note: See Table 1. Unlike that table, the values in this table are unweighted. Note that UKHLS intentionally oversamples ethnic minorities, resulting in the lower white share in this table. As Table 1 shows, this is accounted for in the sample weights.

Figure 8: Impact of conditionality on employment and take-up of tax credits and housing benefit



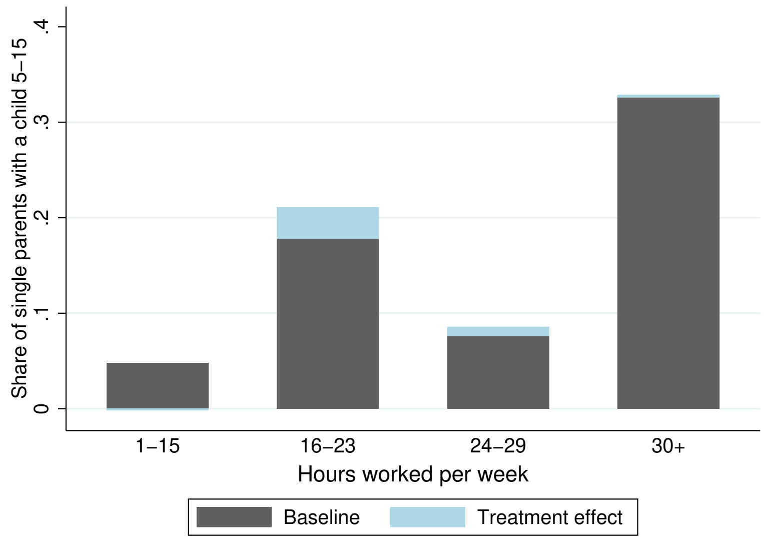
Note: Sample is comprised of single mothers with a dependent child aged 0-18. Data used are pooled FRS and UKHLS surveys. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals indicated with whiskers.

Figure 9: Impact of conditionality on flows into and out of employment, IS/JSA, and incapacity benefits



Note: Data used are the two-quarter LFS. Estimated in a difference-in-difference framework using Borusyak et al. (2021). 95% confidence intervals indicated with whiskers.

Figure 10: Baseline frequency of hours worked per week and treatment effect of LPO



D Fertility and partnership effects

As described in the text, the LPO changes incentives around fertility and partnership, and responses along these lines would threaten our identification strategy. In this section we describe how we test for the presence or otherwise of such effects.

D.1 Data

The UKHLS survey contains a module on natural children and past partnerships, where respondents list (respectively) all their biological children (including those who are no longer dependents), and all cohabiting or marriage relationships they have had in the past (including those that have ended). These modules were asked of all respondents in the first UKHLS wave (2009-2010), and a subset of respondents in the sixth wave (2015-2016).

Together with the panel nature of the dataset, this allows us to build, for each woman, a month-by-month history of the age of their children at that point in time and whether they had a cohabiting/married partner, up until the last time they were surveyed.

D.2 Methodology

The empirical challenge is that fertility and partnership responses change whether we observe an individual as being treated by the policy. That means that mothers cannot be easily separated into treatment or control groups according to the age of their youngest child (or the presence of a partner). The approach we take is to define their group according to how old their youngest child *would have* been, had they had no additional children since a specified time in the past. So long as any fertility or partnership responses do not occur too long before the implementation of conditionality, this provides a valid approach to studying its impact.

The equation we estimate is as follows:

$$y_{i,t} = \mu^{\rho}_{i,t} + \tau_t + \beta c^{\rho}_{i,t} + \epsilon_{i,t}$$

Where $y_{i,t}$ is a dummy indicating a birth, a partnership formation, or a partnership break-up of individual i at time t. $\mu^{\rho}_{i,t}$ is a sequence of dummies indicating the age that i's youngest child would be at t if she had not had any children since $t - \rho$. τ_t is a time dummy to capture population changes in fertility or partnership. $c^{\rho}_{i,t}$ is a dummy which is 1 in the following case: if i had not had any children since $t - \rho$ and was single, conditionality would be applied at t. Ideally, ρ would be set such that there are no partnership/fertility responses to conditionality more than ρ periods before the application of conditionality. Obviously this is unknown, and so we test various settings for ρ.

Intuitively, if there was no policy variation in conditionality, then the $\mu^{\rho}_{i,t}$ dummies would be perfectly collinear with $c^{\rho}_{i,t}$ - knowing the age of the mother's youngest child ρ periods ago would tell you whether or not she would be subject to conditionality today if she had not had any additional children. The policy variation generates variation in $c^{\rho}_{i,t}$ conditional on the age of the youngest child; assuming that any population changes in fertility or partnership are common across women with different aged children, this identifies the effect as β.

We examine three outcomes: birth, partnership formation, and partnership break-up. We hold the data at the mother-calendar year level. The birth, partnership formation, and partnership break-up variables are 1 if the event occurs at any point during the year. $\mu_{i,t}^{\rho}$ is calculated based on the age of the youngest child in January, and is binned into six categories (the two control and four treatment groups that we use in the main analysis). $c_{i,t}^{\rho}$ is 1 if at any point during the year a monthly version of $c_{i,t}^{\rho}$ would be 1.

For the birth and partnership formation outcomes, our sample is single mothers; for partnership break-up it is coupled mothers. Specifically we include those who, as of January of the year in question, would have been single/coupled mothers with dependent children, had they had no change in family circumstances (including no new births) since ρ periods before. We also restrict the sample to observations between 2004 and 2015.

D.3 Results

The results of these regressions, together with the 2004-2008 average rate of births, partnership formation, and partnership break-up, are in Table 8, for a ρ of 12 months. We report a β multiplied by 100; this means that the coefficient can be interpreted as - how much does conditionality raise the percentage probability of a birth or partnership change?

We find little evidence of significant effects across all outcomes, with central estimates close to zero. The estimates are not especially precise, but we can rule out large effects that compare to what we see for employment and benefit take-up. We find similar results if we set ρ to 24 months or 6 months, though in the last case the impact on birth is significant at the 10% level (of course, given that we have tested 9 coefficients across the three ρ values and three outcomes, one result of this size is not particularly surprising).

	(1) Birth	(2) Partnership formation	(3) Partnership break-up
$\beta \times 100$	0.0830	0.360	-0.0688
	(0.444)	(0.488)	(0.219)
2004-2008 average	4.766	5.529	3.227
Sample	Lone mothers	Lone mothers	Coupled mothers
Num. women	4719	4719	10808
Observations	25777	25777	73972

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Impact of conditionality on fertility and partnerships