Can policy foster working-time flexibility for all? Evidence from a right-to-request reform in the UK

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Abstract

The latest surge in flexible working-time arrangements may draw new attention to their regulation. Yet there is little research on whether public policy can stimulate widespread access to working-time flexibility. Besides, the latter often comes bundled with wage penalties and lower earnings. Such compensating differentials have been implicated in the persistence of gender gaps in labour markets, particularly when it comes to wage penalties for mothers. It is unclear though if such trade-offs may also affect women and men without children, if and when working-time flexibility becomes more accessible.

I investigate whether a popular public policy, right-to-request legislation, can bolster working-time flexibility and if compensating differentials may ensue. I focus on a 2014 reform that provided most employees in the UK the right to request reductions in working hours or flexible working times, regardless of the presence of children in the household. Using multiple sources of data and a variety of difference-in-differences estimators, I find no evidence that such reforms affected hours of work, uptake of flexible working-time arrangements, wages and earnings, and job-related well-being among women and men without children. Estimates may suggest that universal right-to-request laws risk being ineffective, particularly if replicating the UK's policy design and in contexts where working-time flexibility may be deemed undesirable for its career costs.

JEL Codes: J16, J22, J28, J31

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*E-mail: mari@essb.eur.nl. Data in this study come from the UK Household Longitudinal Study, a.k.a. Understanding Society (SN: 6614, University of Essex [ISER], 2019), and from the UK Quarterly Labour Force Survey (GN: 33246). All errors or omissions are the author's own.

Working-time flexibility grants employees the ability to adjust when, how much, and from where they work for pay. When working-time flexibility is valued as a job amenity, it could stay in high demand even if bundled with disamenities, such as a lower wage rate. Compensating differentials of this sort can explain stylised facts about labour markets, first and foremost the persistence of gender gaps in earnings (e.g. Goldin, 2014). Recently, working-time flexibility has also been soaring, especially in the form of telework, due to the COVID-19 pandemic and related lockdown restrictions. A widely-held prediction, both in the media¹ and scientific research (e.g. Barrero et al., 2021), has been that such “flexibility is here to stay”. Policymakers may thus want to design new regulations for flexible work, if the shift in preferences indeed proves lasting or with the explicit aim of stimulating it further. Yet there is little research on whether, to what extent, and for whom compensating differentials may ensue when public policy aims to incentivise widespread access to working-time flexibility.

I provide an assessment of such questions by studying a 2014 reform that extended to all employees in the UK the ‘right to request’ flexible arrangements, a right previously granted only to parents of young children. Whereas working-time flexibility is often attained by moving across employers (e.g. Euwals, 2001; Manning & Robinson, 2004; Böheim & Taylor, 2004; Blundell et al., 2008), the right to request allows employees to demand a change in working-time arrangements with their current employer, either by reducing working hours or moving to flexible options such as part-time or flexitime. Right-to-request legislation has been a hallmark of working-time policies in Europe and elsewhere (Hegewisch, 2005; Skinner & Pocock, 2011; Donnelly et al., 2012), and the UK provides a litmus test for its effects. First, incentives to make a request are little. Compared to its continental counterparts in particular, the UK opted for ‘light-touch’ legislation, with no mandates to accept requests and no right to challenge employers’ decisions in court (Hegewisch, 2005). Second, working-time flexibility can be costly for employees, as the quality of flexible jobs, especially part-time work, is relatively low in the UK (in terms of training, wage rates, promotion opportunities; see, e.g., Arulampalam & Booth, 1998; Manning & Petrongolo, 2008; Matteazzi et al., 2014; Adams-Prassl

¹See, for example, <https://www.bloomberg.com/news/articles/2021-03-31/home-working-is-here-to-stay-survey-of-u-k-companies-shows> (last accessed: 18 October 2021).

et al., 2020).

To assess the 2014 reform's effects, I first rely on rich panel data from two waves of the UK Household Longitudinal Study (UKHLS, University of Essex [ISER], 2019). I examine uptake of flexible working arrangements, as well as corresponding monetary (wages, earnings) and non-monetary job features (job-related well-being), using a doubly-robust inverse probability weighting estimator. Although limited to access to flexible working-time arrangements, I then provide additional analyses using a difference-in-differences design with repeated cross sections, thanks to high-frequency data from the UK Labour Force Survey (LFS). Analyses of UKHLS data are suggestive of a decrease in hours of work, of around 1h per week, for women, and a parallel increase in the chances of working from home for men affected by the reform. Yet both estimates are not robust to multiple sensitivity and robustness checks, nor do they replicate when using LFS data. Similarly, I cannot detect any change in the uptake of other flexible working-time arrangements (part-time, flexitime, term-time, compressed week) nor on hourly wages, earnings, or job-related well-being scales. There is no evidence, hence, that expanding the UK's right-to-request law in 2014 boosted access to flexible working-time options among people without care obligations, nor, thereby, that compensating differentials ensued.

These null findings contribute, first, to the debate on compensating differentials. This study is in line and extends previous research arguing for the "pervasive absence of compensating differentials" (Bonhomme & Jolivet, 2009). Whereas previous evidence was limited to men and investigated satisfaction with working times though, I show that similar conclusions can be drawn for women and when examining actual uptake of flexible working times. The relevant caveat is that the target population in this study is women and men without (young) children. When examining employed mothers instead, research has found some support for compensating differentials, with non-monetary working conditions explaining part of the wage penalty for motherhood (e.g. Felde, 2012b,a). This may suggest that search constraints disproportionately falling on mothers (e.g. childcare availability, opening hours of childcare facilities, resulting commuting choices and costs, etc.) are key to compensating differentials, in line with

broader labour market models (Manning, 2003, 2021). Structural models, however, predict and show how compensating differentials may arise with respect to working-time flexibility, particularly for (college-educated) women, without incorporating search constraints related to motherhood (Flabbi & Moro, 2012). This paper provides indirect evidence for the relevance of such search constraints by showing little willingness to move to, and thereby perhaps pay for, working-time flexibility among those who have no search constraints related to parenthood.

Of course, this is contingent on the policy change and context examined in this study. In line with a research agenda concerned with whether public policy can engender compensating differentials (Smith, 1979), a second contribution of this study is to evaluate right-to-request legislation, whose design is still topical today. In September 2021, the UK government, taking stock of the rise in working-time flexibility during the pandemic, uncovered plans to grant the right to request to all employees from day one, rather than after the 26 weeks of tenure necessary in the current regime. Right-to-request legislation also remains a cornerstone of the policy agenda of the European Union (as in the recent directive 2019/1158), as well as of other high-income countries. Despite its popularity, this policy instrument has not been extensively scrutinised in academic research. This study shows that, when it comes to policy design, the UK's "light-touch" approach seems to have not been sufficient to bolster working-time flexibility, at least for those without care obligations towards young children. In addition, findings are also coherent with a low willingness to seek out flexible working-time options, perhaps given the aforementioned career costs this typically entails in the UK context. Findings in this study contribute to shed light on how the effectiveness of right-to-request laws may depend on policy design and national labour market conditions.

1. Background

1.1. Right-to-request legislation in the UK

Under the provisions of the *Employment Act*, and effective since April 2003, parents and carers of children under the age of six² have been granted the statutory right to

²Parents and carers of children living with a disability and under the age of eighteen were also entitled from 2003 onwards.

request, once per year, a reduction in working hours or to work flexibly (Hegewisch, 2005). Eligibility was restricted to dependent employees, continuously employed for at least 26 weeks and excluding those on a temporary work agency (TWA) contract. Employers, who largely opposed more binding legislation (Lewis & Campbell, 2007), were charged with the duty to consider such requests and refuse them only on the basis of business reasons defined by the law.

Early on, expansions of the right to larger segments of the workforce were advocated for (e.g. Fagan et al., 2006), based on the example of similar policies in other countries. In both Germany and the Netherlands, for example, employees became entitled to request hours adjustments and flexible work in 2000, regardless of care obligations (albeit with restrictions on firm size, Hegewisch, 2005; Fouarge & Baaijens, 2009). In the UK, the right to request was gradually extended over time. Parents of children older than 6 (under 18) became eligible with the Work and Families Act of 2006, effective from 2009. The intention to further expand the right to request was spelled out as early as January 2011 by then Deputy Prime Minister Nick Clegg, stating in a speech that “the Coalition Agreement commits us to a universal right to request flexible working” (Pyper, 2018). Only later though, in 2014, the *Children and Families Act* amended the right to request. From 30 June 2014, all employees fulfilling the aforementioned tenure and contract requirements became eligible, *de facto* extending the right from parents to individuals without (young) children. This latter reform is the one of interest here.

The Department for Business, Innovation and Skills (BIS) monitored the reform process via the Work-life Balance Survey, a survey of employers and employees about working-time needs, availability, and practices. Employee (employer) surveys stopped in 2011 (2013), thus prior to the enactment of the reform of interest in 2014. Yet findings suggest that, already in 2011, awareness of right-to-request policies was fairly high among employees without children (73%) and only slightly lower than that of employed parents (79%) (Department for Business, Innovation and Skills [BIS], 2012: 44). Evidence also suggests that the majority of requests were fully granted by employers, with acceptance rates ranging from over 60% to around 80% depending on the survey (Department for Business, Innovation and Skills [BIS], 2011, 2012). High

awareness and high acceptance rates in years prior may suggest that employees without (young) children may have been ready to take advantage of the 2014 reform. At the same time, it is worth stressing that, despite such readiness in the target population and the policy being announced as early as 2011, there is little evidence of employees (or employers) taking action prior to the actual policy change. For one, Google data on web searches for the UK shows a clear peak in the popularity of the keywords “request flexible working”³ in July 2014, when the reform came into effect (Figure 1A in the Appendix). Analyses of long-running LFS data in the next sections support such lack of anticipation, showing rather stable trends in the uptake of flexible work among the target population prior to the 2014 reform.

Early Work-Life Balance surveys also point to important cleavages in the awareness, uses and outcomes of the right to request (*ibidem*). Women were more likely than men to make a request, yet men’s requests were more likely to be denied by employers. Further, awareness was larger among the highly educated, for whom flexible arrangements were (are) also more widely available. Overall, though, uptake of flexible working-time arrangements increased little in the aftermath of early reform, possibly due to the fact that large shares of employed mothers were working flexibly even prior to being granted the new right (*ibidem*; see also Avendano & Panico, 2018).

1.2. Previous evidence

Few have examined, more broadly, the consequences of right-to-request legislation. For the UK, Avendano and Panico (2018) studied the effects of the introduction of the right in 2003. They relied on data from mothers of young children, all of whom gained the new right after the reform. They thus contrasted mothers whose employers did not provide flexible arrangements prior to the reform and mothers employed in workplaces that did. They found evidence of an increase in uptake among the former group, particularly with respect to flexitime. This was by and large not accompanied by changes in self-reported measures of long-term illness, general health, or life satisfaction. They noted, however, that their comparison might be tainted by unobservable

³Note that permutations of the same keywords or other possible keywords (e.g. “right to request”) were much less popular in the same time frame.

differences across the groups they selected, differences that might underlie the quality of the job match at the baseline and the subsequent chances to use and benefit from flexible working time (for a discussion, see also Heywood et al., 2007).

Others have looked into the effects of right-to-request laws on job and hours mobility. For the Netherlands, evidence suggests that right-to-request law did not alter patterns of job and hours mobility for employees in large firms, as compared to control-group employees in small firms (Fouarge & Baaijens, 2009). Even though the new law regulated the pursuit of flexibility with one's current employer, both male and female employees exposed to the reform did not become less inclined to change work hours by changing employer too (see also, Euwals, 2001). Coherently, studies have found no change in the probability of reducing working hours, either via employer changes or not, for Dutch first-time mothers in the decade following the reform (Begall & Grunow, 2015).

Last, right-to-request provisions have also been investigated in the Spanish context, where a 1999 law provided the new right to parents of young children and prohibited their layoff in the aftermath of a request. Whilst mothers (on a permanent contract) took advantage of the new provisions to move into part-time work, findings point to negative effects on the labour market chances of women of childbearing age (Fernández-Kranz & Rodríguez-Planas, 2021). Per effect of the reform, employers appeared, for example, less likely to hire and promote, and more likely to fire, women of childbearing age as compared to men. Protection from layoff spurred unintended effects, as employers insured themselves against such protection by substituting female employees with male employees.

Hence, evidence to date points to null or perverse effects of right-to-request legislation. Reflecting the emphasis put on parents by existing laws, relatively little is known on if and how individuals without care responsibilities for young children may use right-to-request provisions. A plurality of outcomes has been investigated, rarely though in a unifying framework. Gender gaps, albeit seldom examined, may amplify per effect of right-to-request reform.

2. Data and methods

2.1. UKHLS data

I use data from two waves of the UK Household Longitudinal Study (UKHLS, University of Essex [ISER], 2019), covering the period 2011-2016. UKHLS is a household panel survey, following a large representative sample of the UK population (comprising around 40,000 households at its inception in 2009) in multi-year interviews. I picked wave 4 and 6 as only even waves include detailed data on flexible working-time arrangements, and these two waves were by and large timed one before and one after the 2014 reform.

The first set of outcomes captures working-time arrangements. I examine a counter for weekly working hours (divided by 100) and five dummies for working-time arrangements other than full-time recorded in UKHLS (see Table 1 for definitions), namely: part-time, term-time, flexitime, compressed week, and telework⁴. Respondents were asked if they were “currently working in any of these ways”, and could choose one or a combination of the flexible arrangements, if applicable.

Table 1: Flexible working-time arrangements: types and definitions.

Working-time arrangement	Definition
<i>Part-time</i>	Working 30 hours or less per week
<i>Term-time</i>	Working only during school term working times
<i>Flexitime</i>	Working a set number of hours per week or per month, but can adjust start and finish times
<i>Compressed week</i>	Working full-time over a fewer number of working week days
<i>Telework</i>	Working from a location different than one’s workplace all or some of the time

Notes: See, e.g., Department for Business, Innovation and Skills [BIS] (2011, 2012).

A second set of outcomes helps to assess whether changes to working-time arrangements also affected monetary and non-monetary job amenities, as a signal of “job quality”. On the monetary side, I consider (gross) hourly wages and monthly earnings,

⁴UKHLS data also records use of job sharing, annualised hours, zero-hours contracts, and on-call working. The latter two were not used by any employee in my sample. Job sharing was mentioned only in 83 person-year records and annualised hours were mentioned in 96 records, in the full sample. Due to the sparsity of these outcomes, I decided to omit them from the analyses. All other arrangements were observed in at least 388 person-year records (for term-time) in the full sample, with the only exception of compressed week (183 records).

taking the log of the former and expressing the latter in levels (1 = 1,000 GBP). Both are indexed at 2018 prices (via the CHAW-Retail Price Index). Investigating both outcomes allows to disentangle changes in pay that might mechanistically derive from reductions in working hours (earnings) from those who derive from changes in pay rates (hourly wages), as the latter could signal a transition to a lower-quality job or one in which employees are less productive. As for non-monetary job amenities, I can rely on two subscales from the Job-Related Well-Being measure first developed by Warr (1990). The first aims to measure job-related anxiety (borrowing items from Warr’s “Anxiety-Contentment” scale), via three items asking respondents to what extent they feel “tense”, “uneasy” and “worried” about their job ($\alpha_{Women} = .85$, $\alpha_{Men} = .84$). The second taps onto job-related depression (from Warr’s “Depression-Enthusiasm” scale), via three items asking respondents to what extent they feel “depressed”, “gloomy” and “miserable” about their job ($\alpha_{Women} = .90$, $\alpha_{Men} = .89$). Both subscales are reverse-coded so that higher values represent higher levels of anxiety/depression, and standardised, separately for women and men in the sample, so that treatment effects will express deviations in SD units.

2.2. UKHLS sample

The starting sample is restricted to adult members of UKHLS (age 29-58⁵) who were dependent employees in wave 4 and/or 6, i.e. 18,167 individuals and 29,121 person-wave observations. I exclude 225 observations in which respondents reported working on a TWA contract, for these contracts were never covered by right-to-request legislation. Differently, information on tenure is not sufficiently detailed to only include individuals with at least 26 weeks of tenure, needed to file a request. I then define two groups based on exposure to policy change. An individual i is exposed to the 2014 reform ($D_i = 1$) if s/he did *not* report being the (natural, adoptive, step or foster) parent of a child under 16 or being responsible for a child under 18 in wave 4 (hence, prior to 30th June 2014). I further refine the treated group by excluding individuals who report a new birth in either wave 5 or wave 6, as the presence of children immediately gives access

⁵Given that the comparison group for women only includes mothers of children aged 11-18, this age range allows me to include in it those who had a child as early as age 18 and as as late as age 40.

to the right to request also under the pre-2014 regime and, regardless, will surely alter labour supply, particularly among women.

Parents of young children at wave 4 are candidates for the comparison group, being subject to the same ‘right-to-request’ regulations before and after June 2014. As my focus lies on changes before and after the reform, one concern is that employed parents may also be on different outcome trends due to other time-dependent dynamics, i.e. labour supply changes as new children are born and grow older or responsiveness to other policies, such as in-work benefits typically targeting single mothers in the UK (e.g. Francesconi & Van der Klaauw, 2007; Francesconi et al., 2009). Therefore, I exclude from the comparison group ($D_i = 0$): *a*) parents that experience a new birth in wave 5 or 6, same as for the treated; *b*) single parents, overwhelmingly mothers in the sample, for they might be on different outcome trajectories due to responsiveness to other policies, esp. in-work benefits; *c*) for women only, mothers whose youngest child living in the household is 10 or younger. Age 11 is the typical age at which secondary school starts in the UK, and mothers, but not fathers, incur in the largest labour supply changes before children reach that age (e.g. Paull, 2008). In practice, without such sample restriction, evidence of an increase in the uptake of, say, part-time work among women without children after the reform might be a statistical artefact, a by-product of mothers of younger children moving back from part-time to full-time.

Hence, limited to suitable treatment arms, the sample comprises 10,726 individuals and 17,376 observations. I further exclude 4,394 observations due to missing data. Finally, I exclude observations that would not contribute to the estimation due to the focus on before-after changes (see also Section 4.2): these include records for individuals who were observed in wave 4 and wave 6, but both interviews occurred prior to 30th June 2014 (3,783), as well as records for individuals observed only once, either in wave 4 or wave 6 (3,851). The final analytical sample thus comprises a balanced panel of individuals observed in dependent employment in both wave 4 and wave 6, corresponding to 2,122 women, of which 1,710 (81%) are treated by the 2014 reform, and 2,741 men, 1,347 (49%) of which are also treated.

Table 2 reports a selection of sample features as measured at wave 4, before the reform

Table 2: Selected sample features (mean or proportions) at the baseline (Wave 4, UKHLS data).

	Women		Men	
	Controls	Treated	Controls	Treated
ln(Hourly wage)	2.45	2.51	2.74	2.66
Monthly earnings (1000s)	1.97	2.29	3.30	3.01
Job-related anxiety (z-score)	-0.011	0.036	0.034	-0.018
Job-related depression (z-score)	-0.14	0.038	0.0074	-0.025
Weekly work hours/100	0.29	0.34	0.39	0.39
<i>Use of working-time arrangements</i>				
Part-time	0.40	0.21	0.053	0.045
Term-time	0.15	0.058	0.015	0.013
Flexitime	0.11	0.13	0.12	0.12
Compressed week	0.0073	0.022	0.014	0.020
Telework	0.053	0.061	0.10	0.068
<i>Availability of working-time arrangements</i>				
Part-time	0.77	0.70	0.43	0.47
Term-time	0.33	0.23	0.12	0.14
Flexitime	0.32	0.35	0.33	0.33
Compressed week	0.14	0.18	0.16	0.17
Telework	0.12	0.17	0.21	0.19
<i>N</i>	412	1,710	1,394	1,347

took place. Women without children have higher hourly wages and monthly earnings than mothers in the control group. They also score higher on both measures of job-related well-being, indicating *higher* incidence of job-related anxiety and depression. As for working-time arrangements, women without children are around 20 percentage points less likely to work part-time and around 9 percentage points less likely to work on term-time contracts at the baseline, whereas the shares employed in other flexible positions is much more similar. Similar patterns can be found when looking at

how treated and controls sort themselves across workplaces that offer or not flexible working-time arrangements, an important source of confounding in this setting (Heywood et al., 2007). As for men, the picture is reversed, with fathers earning more on average than childless men, but also reporting higher scores of job-related anxiety and depression. Hours of work, use of flexible working-time arrangements, and availability at the current workplace are typically similar between treatment arms. Overall, women without children seem to have more to gain from right-to-request laws, in terms of reducing working hours, taking up flexible working times, and perhaps, thereby, lower job-related anxiety and depression. At the same time, if moving to flexible working times has monetary costs, they might also lose part of their earning advantages per effect of the reform.

2.3. Identification and doubly-robust IPW estimator using UKHLS

The quantity of interest is the average treatment effect on the treated (ATT) by the 2014 reform on a given outcome Y_{it} , obtained via the following difference-in-differences:

$$\begin{aligned} ATT &= E(Y_{i1}(1) - Y_{i0}(1)|D_i = 1) - E(Y_{i1}(0) - Y_{i0}(0)|D_i = 1) \\ &= E(Y_{i1} - Y_{i0}|D = 1) - E(Y_{i1} - Y_{i0}|D_i = 0) \quad (1) \end{aligned}$$

where potential outcomes for the treated in the two periods $t = 1$ and $t = 0$ are contrasted under treatment and no treatment. For the first difference, one assumes consistency between the potential outcomes and the observed outcomes for the treated (i.e. $E(Y_{i1}(1) - Y_{i0}(1)|D_i = 1) = E(Y_{i1} - Y_{i0}|D = 1)$). The difference in outcomes for the treated under no treatment, by definition, cannot be observed, and is thus substituted with the observed difference in outcomes for the comparison group (i.e. $E(Y_{i1}(0) - Y_{i0}(0)|D_i = 1) = E(Y_{i1} - Y_{i0}|D = 0)$).

The main identifying assumption, hence, is one of parallel trends, requiring that the difference in outcomes for the treated and controls would have continued to remain constant in the absence of treatment, thus lending credence to the equivalence between the observed outcomes for the controls and the potential outcomes of the treated un-

der no treatment. Examining pre-reform differences in outcome trends for treated and controls would provide guidance on the validity of such assumption. As most of my outcomes are observed in only two waves prior to the reform (Wave 2 and 4), however, such visual inspection would provide only weak evidence for parallel trends. I therefore opt to rely on a vector $\mathbf{X}$ to enhance the credibility of parallel trends, *conditional on covariates*. I take the pre-treatment value (wave 4) of the following covariates: age, dummies for year and month of interview, a dummy for whether the respondent has a degree (v. A-levels or less), firm size (less than 25, 25-100, 100-500, 500+), region of residence, regional unemployment rates (Office for National Statistics [ONS] data), whether the respondent holds a second job, whether they hold supervisory/managerial positions in their primary job, and a set of dummies for whether each flexible arrangement is available at their current workplace. Although self-reported, this last set of measures helps to address differential sorting between treated and controls across firms where flexibility is more (less) available (cfr. Heywood et al., 2007; Avendano & Panico, 2018).

Identification thus entails comparing changes in the outcome of interest, contrasting on wave prior and one wave after the 2014 reform, among treated and controls that are comparable on a set of observables. As for estimation, I rely on doubly-robust inverse-probability weighting (DR-IPW, see, e.g., Słoczyński & Wooldridge, 2018; Sant’Anna & Zhao, 2020). First, a model of treatment assignment conditional on $\mathbf{X}$ is computed via logistic regression and propensity scores are used to derive inverse-probability weights. Weighted by the inverse probability of treatment assignment, I then fit a regression model for the difference in observed outcomes between $t = 1$ and $t = 0$, ΔY_i , separately for treated ($D_i = 0$) and controls ($D_i = 1$):

$$\Delta Y_i = \alpha + \mathbf{X}_i \gamma + \epsilon_{it} \quad (2)$$

with $\mathbf{X}_i$ including the same covariates used to estimate inverse-probability weights. Predicted outcomes for each individual i are averaged and the contrast between estimated means restricted to the treated subset provides an estimate of the ATT⁶. Given

⁶This procedure is automated in Stata (version 13 or higher) using the routine `teffects ipwra`,

that the indicator D_i distinguishes treated and controls based on *eligibility* for the expanded right to request, rather than actual *use* of such right, all estimates are to be interpreted as intention-to-treat. Throughout, estimation is carried out separately for men and women. Standard errors are robust to arbitrary heteroskedasticity.

In total, I thus examine reform effects for ten outcomes and across two sub-samples. To account for the occurrence of false positives due to multiple inference, I report conventional p -values as well as so called sharpened q -values (Benjamini et al., 2006⁷), which are meant to adjust for the *false discovery rate* (FDR). The latter expresses, in expectation, the proportion of true null hypotheses being rejected among rejected hypotheses in a given set, i.e. the expected proportion of rejections that are false positives. Following previous applications (e.g. M. L. Anderson, 2008), I compute q -values grouping estimates in two sets, i.e. separately for men and women.

2.4. Additional analyses with LFS data

UKHLS has the advantage of providing information on a wide array of outcomes, necessary to assess compensating differentials, and allows for identification conditional on a rich set of covariates, crucially including sorting across workplaces that offer different working-time arrangements at the baseline. Yet I rely on a relatively small sample from two waves of data, each comprising interviews spread over multiple years, with little chance to assess pre-reform trends across treated and controls. To overcome these limitations, I provide additional analyses using Labour Force Survey (LFS) data.

Specifically, I combine cross sections from the second and fourth quarter of each year, from 2009 to 2016. Data from these specific quarters include information on all flexible working-time arrangements examined via UKHLS. In addition, these data come with a much larger sample and the chance to refine sample selection, as compared to UKHLS. Similar to the former, I restrict my analyses to valid observations for individuals aged 29-58 and in dependent employment but excluding agency workers. Different from UKHLS, I can further eliminate those who have accumulated less than six months (≈ 26 weeks) of tenure with their current employer, for whom right to request legislation

atet.

⁷Code: http://are.berkeley.edu/~mlanderson/downloads/fdr_sharpened_qvalues.do.zip.

does not apply. Finally, I define treatment arms as before, contrasting women without children against mothers (but not single mothers) of children aged 11 or older, and men without children against fathers. The final sample comprises 265,783 observations, with 107,735 women, 81,638 of which are treated (76%), and 158,048 men, 78,742 (50%) of which are treated.

The goal of these additional analyses is to assess pre-reform trends in the outcomes of interest and gauge reform effects from clear discontinuities in such trends in the aftermath of the reform (e.g. Kahn-Lang & Lang, 2020). I estimate the following model via OLS:

$$Y_{iq} = \alpha + \beta D_i + \sum_{q \neq 2013q4} \eta \mathbb{1}[Quarter = q] + \delta (D_i \times \sum_{q \neq 2013q4} \mathbb{1}[Quarter = q]) + \mathbf{X}_i \gamma + \epsilon_{iq} \quad (3)$$

where Y_{iq} is either weekly working hours or one of the flexible working-time arrangements defined in Table 1, as measured for an individual i in quarter q . The coefficients of interest (δ) express observed outcome differences between treated and controls (again denoted by D_i) across quarters, with the fourth quarter of 2013 serving as the omitted period. The vector $\mathbf{X}_i$ includes a subset of the covariates used in the UKHLS analysis. LFS lacks data on sorting across employers providing different working-time arrangements. Besides, all job characteristics – managerial status, firm size, second-job holding, etc. – are measured simultaneously with the outcome, due to the cross-sectional nature of the data. Hence, $\mathbf{X}_i$ is only included to ameliorate precision. It comprises covariates that predate or are independent from treatment assignment, namely dummies for the week in which the interview was carried out, age of the respondent, region of residence, and educational level (university degree or not). Separate models are estimated for men and women, with robust standard errors. Adjustment for the false discovery rate is performed separately for each model, thus evaluating sets of 15 null hypotheses each time (16 quarters – 1 reference quarter).

3. Main findings

Figure 1 displays my main results using the UKHLS sample. Treatment effects reflect outcome changes in the target population vis-à-vis outcome changes in an observationally similar control group. In panel a) I investigate the uptake of flexible working-time arrangements and find little evidence of change per effect of the reform. For women, estimates for the whole range of flexible options points to null effects in relatively tight confidence bounds around 0. There is suggestive evidence of a modest decrease in work hours, of around 1 hour per week ($p = .053$). For men, estimates not only point to no evidence of reform effects, but are also substantially smaller across the board. The only exception is telework, whose usage increases by around 2 percentage points ($p = .054$). Both findings though, for women's work hours and men's telework, are not robust to adjustment for multiple comparisons, as the q -value in both cases equals 1, indicating that the null cannot be rejected at any value of the false discovery rate. Coherently, panel b) suggests little evidence of reform effects on monetary job features such as hourly wages or monthly earnings, nor could I detect changes in job-related anxiety or depression.

Figure 1: Point estimates and 95% confidence intervals for ATTs of the 2014 reform (UKHLS data).

Turning to LFS data, I probe these null findings to a different design, using a much larger sample, as well as a longer time frame. Figure 2 displays difference-in-differences results for women. Estimates for post-reform changes in working hours point to reduction of around 1h per week, but do not reach conventional levels of statistical significance. Similar to analyses using UKHLS, I cannot detect reform effects on any flexible working-time arrangements, nor any post-reform trend in the differences between treated and controls displays any sizeable discontinuity with respect to pre-reform trends. Average null findings for women are thus replicated using LFS data.

For men in Figure 3, the estimates of most interest are displayed at the bottom of the panel and concern telework. An increase in telework after the reform reaches 2 percentage points in certain quarters, in line with analyses of UKHLS, yet no discontinuity can be detected with respect to pre-reform quarters. For this and all other working-time arrangements, hence, the picture is one of null findings for men too.

Figure 2: Point estimates and 95% confidence intervals for ATTs of the 2014 reform (LFS data).

Figure 3: Point estimates and 95% confidence intervals for ATTs of the 2014 reform (LFS data).

4. Robustness and sensitivity analyses

4.1. Changes at the extensive margin

Throughout, analyses have been restricted to employed individuals. Outcomes such as hourly wages or job-related depression and anxiety are truncated, in that they remain unobserved for those without paid work. No obvious value can be assigned in place of missing ones. Yet if individuals only appear in the sample provided that they deemed wage offers and other job features desirable (enough), this may distort estimates via sample selection bias. Further, by making working-time flexibility more accessible on paper, the 2014 reform may have affected the chances of accepting employment, as well as the composition of employed individuals over time.

Therefore, I investigate whether the 2014 reform affected the extensive margin of labour supply. For both UKHLS and LFS, I consider samples without conditioning on paid employment, and examine the latter as an outcome. Figure 2A in the Appendix reports the results. In panel a), estimates for UKHLS suggest a decrease in women's chances of

working for pay by around 3 percentage points ($p = .017$), and a smaller decrease for men's (1 percentage point, $p = .180$). Yet large-scale replication fails to yield similar estimates. Using LFS data, panel b) shows no evidence of a reform effect on the extensive margin of labour supply. Estimates for both women and men are tightly bound around 0 and post- and pre-reform differences between treated and controls are statistically indistinguishable, especially in the quarters closest to the reform cutoff.

4.2. Specification curves

Despite relying on two complementary sources of data and identification strategies, null findings in this study may still be dependent on model specification and sample construction. With its panel structure and richer set of outcomes and covariates, UKHLS data allows to probe my findings against a wide array of choices pertaining research design.

As for model specification, I consider possible alternatives to the doubly-robust inverse-probability estimator. As the latter relies on outcome regression (Equation 2), I complement it with a semiparametric solution also based on inverse-probability weights (Abadie, 2005). In line with analyses on LFS data, I also estimate the more conventional 2×2 difference-in-differences, both via OLS and also including individual fixed effects for the sake of precision⁸. The regression framework enables more flexible covariate adjustment to address violations of the parallel trend assumption. Recent appraisals of difference-in-differences (Zeldow & Hatfield, 2019; Frölich & Sperlich, 2019: 238) show that parallel outcome trends, conditional on covariates, might be invalidated when the relation between such covariates and the outcome changes over time. Hence, I add one specification in which the vector $\mathbf{X}_i$ is interacted with a dummy for the reform period (indicated by POST-14 in Figure 3A).

Other than model specification, I examine different sample selections. Given the focus on before-after changes, my main analyses exclude all records from individuals who were observed in wave 4 and wave 6, but both interviews occurred prior to the reform

⁸With two groups and two periods of panel data, regression models with and without individual fixed effects will yield the same point estimate for the difference-in-differences quantity of interest (group by time interaction). Both specifications are unaffected by time-constant confounding bias and, as such, individual fixed effects – that typically capture such confounding – may only add to precision in this setting.

becoming effective (30th June 2014), as well as records for individuals observed only once, either in wave 4 or wave 6. I now look at what would have changed by keeping these interviews in, albeit this is only possible in regression specifications (OLS and FE), as those rely on observations from both periods. In addition, as the reform only enhanced the chances of modifying working-time arrangements with one’s current employer, I examine a sample restricted to employees reporting no employer changes between waves 4 and 6. I also consider alternative definitions of the treatment groups. One possibility is that other reforms affected labour supply differently across treatment groups in the same period. Reforms to state benefits, particularly to the Working Tax Credit in 2011 and 2016, are leading candidates. In line with previous changes to such in-work benefits (Francesconi & Van der Klaauw, 2007; Francesconi et al., 2009), these reforms may have particularly affected single mothers, who have not been part of the control group throughout. Nonetheless, as most state benefits are means-tested, I re-estimate all specifications excluding units in the lower third of the (net) household income distribution⁹, most likely affected by benefit reform in the period. Finally, I investigate heterogeneous effects splitting samples by level of education. The latter may dictate differential access to, as well as awareness, quality, and opportunity costs of flexible working-time arrangements (e.g., for the UK, Department for Business, Innovation and Skills [BIS], 2012; Costa Dias et al., 2020; Adams-Prassl et al., 2020). Results are displayed in Figure 3A in the Appendix, focussing on women’s weekly working hours for illustrative purposes. Point estimates are ordered by magnitude, with the label “Main” referring to the specification and sample restrictions followed in the main analyses. Similar to the main analyses, findings across specifications indicate *at most* a reduction of around 1h per week. Estimates are also generally larger for degree holders, but much depends on model specification and differences with respect to women with lower relative education are statistically and substantially negligible. Regardless of model specification or sample selection, most estimates are close to 0 and by and large do not reach conventional levels of statistical significance. An overarching

⁹The lower tertile is derived from the distribution of net household income among *all* UKHLS households at Wave 4, thus in the pre-reform period, and weighted by the appropriate household weights provided by the survey.

pattern of null findings emerges for this and all other outcomes¹⁰, supporting evidence from the main analyses.

5. Discussion and conclusions

In this paper, I examined a 2014 reform extending the right to request reductions in working hours and flexible working-time arrangements to most employees, regardless of the presence of young children in the household. Findings provide little evidence that the introduction of a (nearly) universal right to request “moved the needle” in the UK, for better or worse: There is no robust evidence of reductions in working hours or of an increase in the uptake of flexible working-time arrangements and, thereby, I found no evidence that compensating differentials ensued for women and men without young children.

A first limit of my analyses is that I was only able to track uptake of flexible working-time arrangements and a limited range of monetary and non-monetary job features. Overcoming data constraints in this paper, future research could provide a more comprehensive picture in a number of ways. One is to assess if right-to-request reforms affect *preferred* working hours, and depending on their effects on *actual* working hours, whether such policies ease or exacerbate hours mismatches (cf. Böheim & Taylor, 2004). Another is to examine requests themselves, perhaps via administrative records, to go beyond the kind of intention-to-treat estimates provided here. Finally, research could assess if right-to-request reforms, even when not resulting in immediate changes in working times, may improve tenure accumulation and decrease separation rates, and with what consequences for job (mis)matches.

Despite the many opportunities to expand on the evaluation of right-to-request policies, this study provided no evidence of economically significant effects across a wide range of labour market outcomes. The null findings, often tightly bounded around zero, could stem from the light-touch policy design the UK opted for, and/or from the often sub-par working conditions typically associated with working-time flexibility – especially part-time jobs – in the UK. Future research from different contexts is crucial in this respect. Yet an alternative explanation and second limit of my analyses is that

¹⁰Figures are available upon request. Replication code will be made available upon publication.

equilibrium effects cannot be fully ruled out. It could be, for one, that by ‘normalising’ flexible work, the expanded right to request incentivised requests and/or improved chances of having one accepted among parents, my control group. Alternatively, an expanded right to request might have ameliorated labour market outcomes among parents, mothers in particular, by reducing statistical discrimination – a reversal of the effect reported for right-to-request laws that only target parents instead (Fernández-Kranz & Rodríguez-Planas, 2021). Arguably though, equilibrium effects might take time to materialise and, vice versa, I did not find evidence of reform effects even in the quarters right after the reform took place.

All in all, evidence from the UK’s light-touch approach suggests that right-to-request legislation may fail to stimulate demand for flexibility among people without care responsibilities for young children. Countries might look into expanding their more restrictive right-to-request laws, often limited to parents (Hegewisch, 2005; Skinner & Pocock, 2011; Donnelly et al., 2012). A recent European directive (2019/1158, Article 9), for example, still makes reference to parents of children up to at least 8 years and all carers. At the same time, this paper suggests that, as currently conceived in the UK example at least, this working-time policy may have little effects, supporting early characterisations of the law as “‘sound bite’ employment legislation” (L. Anderson, 2003). Compared to changes within occupations that occurred without policy intervention and brought both flexibility and a narrowing of gender gaps (Goldin, 2014; Goldin & Katz, 2016), promoting flexibility via right-to-request policy may be ineffective, depending on policy design and national labour market conditions.

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Working-time flexibility for all?

Evidence from a right-to-request reform

Appendix

Figure 1A: Google Trends data for Google searches of “request flexible working” in the UK (2009-2016, data last accessed on 21 October 2021). A score of 100 (0) expresses maximum (minimum) popularity for a given search term across all months in the selected time frame.

Figure 2A: Point estimates and 95% confidence intervals for the probability of being in paid employment (UKHLS and LFS data).

Weekly work hours/100

Note: $p < .05$ in 30 out of 60 estimates (range: .001, .865), point estimates range from -.014 to -.002.

Figure 3A: Point estimates and 95% confidence intervals for reform effects on women's weekly working hours (divided by 100), across different model specifications and sample selections (UKHLS data).