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Incidence, salience, and spillovers: The direct and indirect effects of tax credits on wages

Ghazala Azmat
Sciences Po and CEP (LSE)

Tax credits are a popular way to alleviate in-work poverty. A common empirical assumption is that the benefit of the tax credit is borne solely by the claimant workers. However, economic theory suggests no particular reason why this should be the case. This paper investigates the impact of the Working Families' Tax Credit, introduced in the UK in 1999, on wages. Unlike similar tax credit policies, this tax credit was paid through employers rather than directly to workers, making it more salient to the employer. Using a novel identification strategy, we can separately identify the effect on wages associated with an increase in the amount of tax credit and that associated with the change in salience. We find evidence that: (1) through the salience mechanism the firm cuts the wage of claimant workers relative to similarly skilled nonclaimants by 30 percent of the tax credit, which is approximately 7 percent of the wage, and (2) there is a negative spillover effect onto the wages of claimant and nonclaimant workers of 1.7 percent, which is approximately 8 percent of the tax credit for claimant workers.

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JEL classification. I38, J30, H22, H23.

1. Introduction

Traditional welfare policy often struggles to provide support for low-income families without creating distortions in the labor market. The introduction of the Working Families' Tax Credit (WFTC) in the UK in October 1999, which replaced Family Credit, sought to help working families. WFTC aimed to alleviate poverty at the lower end of the wage distribution, reduce income inequality, and redistribute income by reducing the dispersion of earnings. Unlike similar tax credit policies in different countries, such as the Earned Income Tax Credit (EITC) in the United States and the Self-Sufficiency Program in Canada, the WFTC was paid through employers rather than directly to workers. The motivation for payment through the wage packet was to make the tax credit more convenient to distribute and to reduce the stigma attached to receiving tax credits in the form of a welfare benefit, which could also then increase its take-up rate. However, payment in this way gave employers complete information about which employees were claiming and how much WFTC they were receiving, thus making it more salient to the employers.

Ghazala Azmat: ghazala.azmat@sciencespo.fr

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The policy change in the UK from Family Credit to WFTC had two important components. First, the amount of the tax credit was increased. Second, the tax credit was paid through employers rather than directly to workers. It is typically assumed that the incidence of the tax credit is solely on the claimant worker. That is, it is assumed that the worker receives the full tax credit; however, this is unlikely to be the case—it is more likely that the employers share in the gains from the tax credit. It can be shown with a simple general equilibrium model that the eligible employee and the employer can share the incidence of the increased tax credit and that a spillover effect onto noneligible workers can occur. Moreover, the increased visibility of the tax credit can be exploited to identify a channel through which the incidence of the tax credit can be shared, even in the absence of a change in the amount of tax credit paid.

In this paper, we propose an approach to estimate the direct effect of the tax credit on the wage of eligible workers, as well as the indirect (i.e., spillover) effect on similarly skilled noneligible workers. We separately identify the effect on wages associated an increase in the amount of tax credit and the effect associated with a change in salience. Interestingly, we find that while the change in the amount of tax credit received by families has virtually no effect on wages, the increased visibility has a strong effect on wages, such that there is a shift in the incidence of the tax credit through the salience mechanism. We find, first, that the employer cuts the wage of the eligible (male) worker relative to a similarly skilled noneligible worker, allowing the employer to extract 38 percent of the WFTC gains—30 percent directly and 8 percent via a "spillover." Second, we find that there is a negative spillover effect of WFTC onto the wages of comparable workers, resulting in as much as 1.7 percent decrease.

Traditionally, the literature has focused on the financial incentives for the recipient created by taxes and subsidies. Leigh (2009) and Rothstein (2010) investigated the impact of increased labor supply resulting from changes in the EITC in the US on the equilibrium wage. Using variation across states in EITC supplements, Leigh (2009) finds that a 10 percent increase in the generosity of EITC is associated with a 4 percent decrease in the wages of high school dropouts and a 2 percent decrease in the wages of college graduates. Rothstein (2010) uses variation across the wage distribution in the implementations of the mid-1990s federal EITC expansion and finds that low-skilled mothers kept only 70 percent of every dollar they received under the EITC. Unlike the EITC, an interesting feature of the WFTC in the UK is the payment through employers rather than directly to workers, which allows the employer to have information on the amount of subsidy received. In the US, the employer is not responsible for income-tax filing on behalf of employees, so the EITC is not visible to the employer.

Recent research has highlighted that the salience and transparency of tax incidence may matter as much as, if not more than, the financial incentives alone. For example, in an experiment to test whether people underreact to sales taxes, Chetty, Looney, and Kroft (2009) showed that by posting price tags with the tax-inclusive price below the original pretax price tag, demand decreases by 8 percent. Finkelstein (2009) analyzed how tolls change after toll facilities adopt electronic collection, such that drivers are less aware of tolls. She finds that tolls are as much as 40 percent higher than they would have

been without the electronic method. With regard to tax credit salience, a recent experiment by Chetty and Saez (2013) shows that when providing recipients with information about the work incentives of the EITC, there was a significant effect on earnings. In another (field) experiment on the EITC, Jones (2010) found that reducing informational costs regarding Advance EITC, which allows EITC recipients to receive a portion of the credit early—in incremental payments in each paycheck during the tax year instead of in a one-time EITC payment—does little to increase participation in the program. While most of the salience literature focuses on the recipient, in this paper, the distinctive nature of this policy allows us to focus on the change in salience from the employer's perspective. Tax credits existed in the UK before WFTC, but they were paid, like the EITC, directly to the recipient. The change in payment method allows us to investigate the impact on employers' behavior.

The empirical strategy in this paper uses the eligibility criteria based on multiple household characteristics to create an accurate comparison group. A counterfactual wage for each worker is constructed using a rich set of worker characteristics prior to implementation of the WFTC. We adjust for changes in common trends, such as changes in the average earnings and general inflation, to measure the direct effect of WFTC on eligible workers and the spillover effect on all workers (eligible and noneligible) as the deviations from their effective wages with respect to their predicted wage. To estimate the indirect effect of WFTC on all similar workers, we measure the change in wages for workers in different education (industry) groups, where we weight each group by the average amount of WFTC that workers in those groups are eligible for and the fraction of eligible employees. In addition, we use the change in generosity from the previous policy, Family Credit, to understand the mechanism behind sharing the tax credit incidence. Family Credit differed from WFTC in terms of generosity and the method through which is was paid, however, it was similar to WFTC in terms of the eligibility criteria, such as being contingent on working a certain number of hours and on the presence of children.

Our analysis of the effects of the tax credit considers both, men and women. Traditionally, the tax credit literature focused mostly on women, and in particular, single women (see, for instance, Eissa and Liebman (1996) for the US and Blundell, Duncan, McCrae, and Meghir (2000); Francesconi and van der Klaauw (2007) for the UK). However, institutional structure of WFTC specified that either parent could claim the tax credit through his or her employer. Given that fathers were as likely as mothers (if not more likely) to claim the tax credit through their employer, it is relevant for us to include men in the analysis. In addition, the policy's labor supply impact has been shown to be different for men and women. While the policy resulted in a labor supply increase only for single mothers, with no overall effect on married mothers (but it may increase or decrease depending on partners' employment status), there was a small and negligible negative effect on the labor supply of fathers (for studies on labor supply effects on single parents; see Blundell et al. (2000), Gregg and Harkness (2003), Brewer, Duncan, Shephard, and Suarez (2006), Francesconi and van der Klaauw (2007), Leigh (2007), Gregg, Harkness, and Smith (2009), Azmat (2014), Shephard (2017)). For studies on married couples, see Blundell et al. (2000), Blundell (2001), Blundell and Hoynes (2004), Francesconi, Rainer, and van der Klaauw (2009), Shephard (2017)). In the analysis, we

look separately by marital status of men and women. Finally, Azmat (2014) showed that there are no significant effects of the policy on the inactivity-activity margin for women and only very modest effects on the employment-unemployment margin.

The results in the paper have important policy implications. In particular, they imply that there is a significant shift in the burden of tax credits and that it is the salience of the tax credit—not the change in its amount—that is important for this shift in incidence. This is of critical policy importance, as we can no longer assume that the effects of the tax credit are concentrated on the beneficiary. Moreover, the method by which tax credits are distributed will have important consequences. These results are central to our understanding of the consequences of the expansion, application, and generosity of tax credits.

2. Analytical and institutional framework

In this section, we discuss how in a simple market economy, with and without worker heterogeneity, and in an imperfect competition economy there are likely to be shifts in the burden of the tax credit away from the eligible employee. Moreover, the salience of the tax credit is likely to play an important role. There are two important findings from both settings that can be empirically tested: first, there is a direct effect on the wages of those eligible; second, there is an indirect effect on eligible and noneligible workers.

2.1 *Analytical framework*

Typically, discussions of tax credits presume that the incidence of the tax credit is entirely on workers. However, this is unlikely to be the case. When studying the economics of taxation for goods, a basic result is that the economic incidence of the tax is not necessarily the same as the statutory incidence. In particular, the economic incidence will depend on the elasticities of supply and demand for the good being taxed. We might expect this to be the case for tax credits as well. To model some degree of partial incidence of the tax credit on wages, it would be necessary that both, eligible workers and employers have some degree of bargaining power over wages. Moreover, since tax credit recipients typically compete in the same labor market as others who are not eligible for the tax credit, we might observe spillovers on noneligible workers. This would be the case if there is some degree of substitutability between eligible and noneligible workers. There are a number of models that incorporate these features and help us understand the empirical setting and the results.[1]

Even in a simple economy setting, assuming that workers are perfect substitutes and the law of one wage holds, if we extend the simple tax incidence model and allow for some workers to be eligible for a tax credit and others not, we would expect that the tax credit, as is the case for taxes, would create a distortion. Computing the equilibrium labor demand and supply, we would expect that a change in tax credit will affect wages. In particular, depending on the proportion of eligible workers, the larger the labor supply elasticity of the eligible group and the more elastic the labor demand, the larger the shift

[1]For a formal discussion of the different models, see an earlier version of the paper (Azmat (2012)).

in the subsidy from the worker to the employer. In equilibrium, firms would be indifferent between hiring an eligible and an ineligible worker, such that the direct incidence effect and the spillover effects would be the same for eligible and noneligible workers. Empirically, the presence of bargaining or other frictions may entail that a firm treats an eligible or a noneligible worker differently. We discuss this in more detail later in the section.

Harberger's (1962) general equilibrium analysis derived the burden of a tax on capital in one sector. Azmat (2012) adapted the Harberger model to show the general equilibrium effect of a tax credit on input compensation in a one-sector model that uses two different types of labor to produce one good. More specifically, extending the simple model described above to allow for worker heterogeneity, where heterogeneity is based on eligibility criteria, it is shown that there exist wage effects of a tax credit on both the eligible and noneligible groups. Namely, a direct effect of a change in the tax credit on the wages of eligible workers, which, as in the simple case, would be a result of bargaining between employer and worker in the incidence of the tax credit. In addition, there exists an indirect (spillover) effect on the wages of noneligible workers, where the magnitude of the wage effect on this group, relative to the eligible group, depends on the level of substitutability between the two groups as well as the proportion of the groups.

While the direct effect is the result of the individual bargaining between the firm and the worker, the indirect effects are equilibrium effects and do not rely on any specific form of wage-setting and could be the result of a wage-posting model. For example, incorporating some of the above features, Shephard (2017) used a wage-posting search model with frictions (á la Burdett and Mortensen (1998)) to show that if firms set wages and if eligible workers respond the reform by increasing labor supply, firms may respond by lowering wage offers. In which case, the effective transfer to eligible families is reduced, while noneligible families may become worse off if they are competing within the same labor market. Even in a partial equilibrium monopsony, following Manning (2003), it can be shown that the gross wage of a tax-credit claimant will fall, and in the general equilibrium setting, a revenue-neutral increase in tax progressivity reduces the average wage the employer pays.

The visibility of the tax credit is likely to play an important role in the shift in tax credit incidence. If the employer has some information/knowledge about which of her workers is eligible for the tax credit and there is some degree of individual wage bargaining between the firm and each given worker, wages can react to the tax credit that the employer infers and that the worker receives. If the tax credit is paid through the employer, the employer can see clearly if the worker is claiming (and how much she is receiving) rather than inferring it when it is paid by the state.[2] These bargaining effects may be the results of a rational response or of a behavioral response on the side of the employer or employee.

On the employer side, the employer may internalize that an eligible worker is unambiguously better off as a result of the tax credit. If we assume market frictions, we might

[2]In the absence of direct visibility of the tax credit, the employer needs to rely on some form of statistical discrimination by inferring the likelihood of the worker claiming the tax credit.

expect that the employer will realize that if she cuts the worker's gross wage, the worker will not quit immediately. In turn, the employer can absorb part of the benefits of the tax credit by paying lower wages. In this case, we would expect two potential effects: first, the employer can cut the wage of the eligible worker, and/or second, she can average out the effect for all workers.[3] The second, indirect, effect is more relevant when workers within the same firm have very similar roles, for example, as the size of the firm increases, it is more common to have workers with the same job title. In Section 7, we show that there are differences depending on the size of the firm. Note that, while the visibility effect requires some degree of individual wage bargaining, the effects of tax generosity and the spillover effects can operate through direct wage posting.

In the absence of an employer making optimization errors when setting the wages of eligible and noneligible workers, there may be other rational or behavioral responses that could explain a shift in tax credit incidence. For example, in terms of a behavioral response, while an employer may not find it fair to "steal" a subsidy the government is paying directly to the worker as a welfare payment, the employer may find it fair to "share" a subsidy the government is paying to the firm to help the firm pay the employee. In this case, the method of payment might make a difference in their response. However, there are still potential rational responses that might be a consequence of asymmetric information. If, for example, the employer approximately knows that the worker receives the tax credit but there is an asymmetric response on the side of workers (e.g., he or she would leave the firm if underpaid), then the cost of undershooting or overshooting with the incidence is also asymmetric. We might, therefore, expect employers to take a cautious approach before reducing wages. The noisier the signal is, the more cautious the approach of firms. By knowing exactly who receives the tax credit and the exact amount, we would expect the incidence effect to increase.

On the worker side, there may also be behavioral effects that would imply an effect of tax credits on wages. The most obvious response would that that the worker adapts labor supply.[4] However, there are other channels through which the worker might respond, even in the absence of a labor supply response. For instance, workers may perceive that their wages are higher if they are "topped up" with tax credits rather than receiving tax credits as a separate payment and this perception may induce them to accept a lower wage. With this in mind, there may potentially be a number of behavioral effects. For example, there may be reference points for what the worker considers an acceptable wage. When the tax credit is paid through the employer rather than directly to workers, workers might be willing to accept lower payments from the firm because the overall paycheck goes beyond those reference points. Moreover, if workers are behavioral, rational firms would react to this and would try to exploit these biases.

In summary, we have discussed how in various settings a tax credit has consequences for the wages of both eligible and noneligible workers. This is the case even

[3]It is reasonable to assume that the employer cannot substitute eligible for noneligible workers because, given that the eligibility criteria are not "physically" apparent at the interview stage (only after they have been employed), the employer cannot prescreen workers and discriminate.

[4]Empirically, the studies have found only small effects of WFTC on labor supply. See Brewer and Browne (2006) and Brewer, Francesconi, Gregg, and Grogger (2009) for a review of studies and see page 20 for a detailed discussion of the literature.

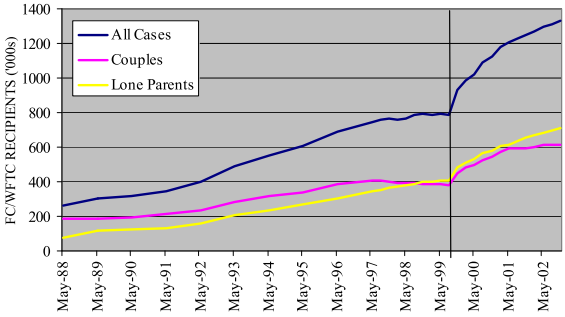
in a simple competitive framework, and we see that the effects persist even in a more realistic noncompetitive framework. Overall, we may expect that the tax credit will have a negative effect on the wages of both eligible and noneligible employees. In the subsequent sections, we will empirically investigate the effect of tax credits on wages.

2.2 *Institutional framework*

The Working Families' Tax Credit (WFTC), introduced in the UK in October 1999, was designed to target low-income families with an income supplement that was contingent on being employed. Although in-work cash benefits existed in the UK prior to 1999, WFTC was more generous and extended further up the income distribution. Compared with its predecessor, Family Credit, twice as many families became eligible under WFTC. Figure 1 shows how the number of claimants changed from 1988 to 2002.[5] From 1.1 million claims for WFTC in August 2000, the number increased to 1.3 million in August 2001, nearly 430,000 more than claimed under Family Credit in August 1999.

The eligibility for WFTC and the amount received were based on four factors: family income being less than £92.90 per week; the presence of children in the household; a minimum of 16 hours of work in the family per week; and low household savings. If household income was above £92.90 per week, the maximum WFTC was reduced. In particular, the marginal deduction rate was 55 percent. That is, there was a reduction of £0.55 for each pound over £92.90. The maximum weekly rate of WFTC consisted of a credit for each child and a bonus if the claimant or his/her partner worked for 30 hours or more each week.

The government spent £5 billion per year on WFTC (which accounted for 1.5 percent of the government budget and 0.6 percent of the GDP in 2000). This was nearly £2 billion more than was spent under Family Credit. The increase in expenditure came from the increased credit per child (e.g., for children under 11 years old, it increased from £15.15 to £26.00); the threshold support increase from £80.65 per week to £92.90 per week; and there was a decrease in the marginal deduction rates from 70 percent to 55 percent, as



Source: Working Families' Tax Credit Statistics, Inland Revenue Quarterly Enquiry (2003).

FIGURE 1. FC/WFTC recipients by family type, May 1988–November 2002.

[5] In April 2003, WFTC changed again to the Working Tax Credit and Child Tax Credit.

described above.[6] In addition, the childcare cost changed from paying £60 (£100) for one child's (more than one child's) weekly childcare to paying 70 percent of childcare costs that account for weekly childcare costs up to a maximum of £135 for one child and £200 for two or more children. The policy parameters for the different years are shown in Table A.1 in the Online Supplemental Material (Azmat (2019)).

The changes with respect to Family Credit implied that those who were previously receiving the maximum payment would see a small increase if they had children under the age of 11; those with a income between £80.65 and £92.90 per week would receive full tax credit support. Others would benefit from the decrease in the marginal deduction rate from 70 percent to 55 percent, and the largest cash gain would go to those who were previously just outside the eligibility bands. Figures A.1 to A.3 show the change in budget constraint from Family Credit to WFTC for different family types.

It is important to note, however, that the change in generosity of tax credits did not necessarily correspond to changes in income because of its interaction with other taxes and benefits. For example, the increase in income (for single mothers) was small for those who worked fewer than 25 hours a week because of the interaction between WFTC and the Housing Benefit (Blundell and Walker (2001)). Shephard (2017) developed an equilibrium job search model and takes an integrated view of the UK tax system to study the effects of WFTC. The analysis shows that non-WFTC reforms, such as changes in income tax over the same period, which affected all workers, had only a very small effect on the labor supply, which is mostly concentrated on single parents. In the analysis, we look at the effect of WFTC separately for different subgroups. Another possible interaction is between WFTC and the national minimum wage, introduced in 1999. In the analysis, however, it imposes a lower bound below which the employer cannot cut the gross wage. We use the minimum wage as a point of censor when measuring the policy effectiveness.

The key characteristic for estimating the policy change was that the policy became salient to the employer. Moreover, unlike other transfers, WFTC became salient to the employer. As mentioned previously, the main difference between Family Credit and WFTC was that the WFTC payment was made through the employer rather than directly to the worker. This method appealed to the government because the payments became more convenient to distribute, and it reduced the "welfare benefit" stigma attached to the tax credit. From October 1999, the eligible claimant would apply for the tax credit from the Inland Revenue, which would work out the amount of tax credit payable.[7] The Inland Revenue would then notify the relevant employer of the amount of tax credit to be paid, and the employer would pay it out of the tax and National Insurance contribution that she would otherwise have forwarded to the Inland Revenue. This increased the visibility of the tax credit from the viewpoint of the employer.

[6]These are based on the final Family Credit parameters in 1999 and the 2001 WFTC parameters. For all policy parameters, see Table A.1 in the Online Supplemental Material.

[7]WFTC started in October 1999, although Family Credit recipients stayed on Family Credit until their 6-month reward ran out.

3. Data

The empirical investigation is performed using the UK's Quarterly Labour Force Survey (LFS). The LFS is a quarterly representative survey of households that contains detailed information on individuals, households and families. This includes information on employment, earnings and a variety of control variables needed for our analysis. In our analysis, we mostly use data from 1997 to 2003. Although data are available beyond this period, we do not use them because the Working Tax Credit and Child Tax Credit replaced WFTC in April 2003.

The sample includes men and women aged 21 to 60. Full-time students, the sick/disabled, and individuals in a government training program are omitted from the sample. In addition, to remove outliers, observations of gross hourly wages below £2 and above £60 are excluded, which excludes approximately 1.4 percent of the sample over the whole period. The hourly wage variable is corrected in two ways. To adjust for changes in common trends, we correct our wage variable quarterly for the changes in average earnings and general inflation using quarterly average earning indexes (AEI) and the retail price index (RPI) from the UK's Office of National Statistics.

The LFS contains information on benefit receipts, so we can identify those who are eligible for WFTC and those who say they claim it. We can also estimate the amount of benefit for which a household is eligible using data on household income, hours worked, and the presence of children (i.e., the eligibility criteria), all of which are included in the dataset. In addition, we can calculate—based on these characteristics—the difference in tax credit entitlement compared with Family Credit, the preexisting tax credit.

4. Identification strategy

In this section, we describe the strategy we use to identify the effect of WFTC on wages. In particular, we want to estimate the direct effect of WFTC on eligible workers and the indirect (i.e., spillover) effect on all similar workers. In addition, we want to separately identify the effect on wages that comes from a change in salience versus a change in generosity. We do this in two stages. First, we estimate a counterfactual wage, which is a prediction of the wage a worker would receive in the absence of WFTC. Second, we separately measure the direct effect of WFTC on eligible workers and the indirect effect on all workers.

We start by estimating a wage equation using data from the period preceding the introduction of WFTC. This is then used to predict future wages (i.e., to construct the counterfactual wage of those eligible for WFTC). This is described in detail in Section 5.1. Since the standard difference-in-difference approach is not feasible for estimating spillover effects, we employ a strategy that will allow us to estimate the direct, as well as indirect, policy effects.

In the policy evaluation literature, it is common to construct "treatment" and "control" groups from whose comparison we can measure policy effectiveness. In particular, the literature on tax credit policy evaluation compares people with children (i.e., "treatment" group) to those without (i.e., "control" group) and, quite commonly, single mothers with single women without children (see, for instance, Eissa and Liebman (1996),

Francesconi and van der Klaauw (2007), Gregg and Harkness (2003), Azmat (2014)). Here, we use an alternative identification strategy. Since WFTC eligibility is multidimensional, we identify as "treated" the group that fulfills the eligibility criteria. Moreover, rather than using a simple treatment identifier, we construct a more flexible variable that allows for variation in the *amount* of WFTC for which a worker is eligible.

The eligibility for WFTC is based on the presence and number of children, household income, and hours of work in the household. The differences in the factors determine not only whether a household is eligible but also the size of their entitlement (i.e., how much the household is eligible for). If both members of the household are working, only one worker in the household can claim the WFTC through his or her employer.

The workers' counterfactual wage is constructed using their estimated pre-WFTC wage. Based on their characteristics, two workers can have the same predicted wage, but one may be eligible for WFTC when it is introduced while the other is not. We later show that eligible workers are similar in observable characteristics to noneligible workers with the same predicted wage. Note that the "frontier" between eligible and noneligible includes multiple dimensions and is a continuous treatment. This is richer than using the children criterion alone.

To estimate the direct effect of WFTC on eligible workers, we compare the wages of eligible and noneligible workers with the same predicted wage once WFTC is introduced. The predicted wage is corrected for average earning changes and general inflation using quarterly indexes from the UK's Office of National Statistics. Since we have quarterly data, we can look at wages in the narrow periods before and after the introduction of WFTC. We can then compare the relative wages before and after wage changes. Of course, a key consideration is that workers do not alter their behavior to become eligible if they were not previously. In particular, we find that they do not change their hours worked.[8] We address this further in Section 5.

Using information on the previous program, Family Credit, we can identify whether the effect is driven by a change in generosity or a change in visibility. We do this by estimating the change in generosity from Family Credit (the last levels when in operation) and WFTC. As a robustness check in Section 7, we conduct a "falsification" test by repeating this analysis on placebo treatments in years prior to the 1999 change to ensure that this relative difference was not there before the WFTC reform, when the tax credit was not salient to the employer. This also reassures us that the common trend assumption we impose holds on a sample outside our treatment period.

To estimate the indirect effect of WFTC on all similar workers, we categorize all workers into education and industry groups separately and identify the fraction of WFTC-eligible workers in each group. This is described in detail in Section 5.1. We test whether an increase in the presence of eligible workers in a group leads to a change in the wage of all workers in that group.

Another important feature of our analysis is that we study the wage changes for both male and female workers. The institutional structure of WFTC specifies that either parent can claim the tax credit through his or her employer. Although, we cannot

[8]We are not concerned by the presence of children because, at least in the short run, this will not be altered. In addition, we use predicted weekly wages to calculate household income (this will become clear in the next section).

identify whom in the household claims—we calculate incidence for the respondent of the survey—the calculations are a lower bound for the actual incidence of the claimant. Given that in a coupled household, it is more likely that the male member of the household will work, and it is, therefore, more likely that he will be the tax credit claimant. There are other important reasons to study workers separately by gender, as well as marital status. While the policy resulted in small labor supply increases for single mother, there was no overall effect on married mothers and a small and negligible negative effect on the labor supply of fathers (see Blundell et al. (2000), Gregg and Harkness (2003), Brewer et al. (2006), Francesconi and van der Klaauw (2007), Leigh (2007), Gregg, Harkness, and Smith (2009), Azmat (2014), Francesconi, Rainer, and van der Klaauw (2009), Shephard (2017)).

5. Estimation strategy

In the previous section, we described the main features of the empirical analysis. We now proceed to the estimation strategy. We begin this section by describing the main specification and then describe the two stages of the analysis.

5.1 *The main specification*

We want to estimate the change in the (log) wage, W, over and above the wage that would have prevailed without the tax credit reform, that is, the "counterfactual" (log) wage, W^C. In particular, we want to measure the *direct effect* of the introduction of the tax credit on the wage of the eligible; the *indirect effect* of the tax credit on all workers; and the effect on the wage that can be attributed to the change in generosity of the tax credit from its predecessor's. The latter variable allows us to understand whether increased salience, rather than increased generosity, explains (potential) wage changes because some workers will receive an increase in tax credit from Family Credit to WFTC and because others who previously were not eligible are now eligible. We use the variation in the change in the amount of tax credit using the last set of criteria for Family Credit before being abolished to capture the effect of a change in generosity with respect to WFTC.

We estimate the following for worker i:

$$W_{it} = \beta_0 + \beta_1\left(W^c_{it}\right) + \beta_2\left(TC^d_{it}\right) + \beta_3\left(TC^s_{gt}\right) + \beta_4 \Delta TC_{it} + u_{it}. \tag{1}$$

The counterfactual wage, W^C, is the wage that would prevail for worker i at time t in the absence of the tax credit, WFTC. We correct this wage variable for changes in average earnings and general inflation using quarterly indexes, t. In turn, we do not restrict β_1 to 1 to allow for additional flexibility in dealing with the aggregate evolution of wages. With adjustments for these trends, the coefficient is likely to be close to but not exactly one. TC^d measures the direct effect of WFTC on eligible workers. TC^s measures the indirect (or spillover) effect of tax credit on all workers in group g, where g is the determined by industry or education grouping. ΔTC measures the change in the generosity from Family Credit to WFTC. We describe each variable in detail below. In addition, we include time fixed effects for the whole sample period and we deal with the estimated regressor

problem by bootstrapping standard errors. In the analysis, we look at men and women separately.

The direct effect, TC^d, is the amount of WFTC, and β_2 measures the extensive margin (going from no tax credit to WFTC). A straightforward way in which we could measure TC^d is using a dummy variable for eligibility. However, to have TC^d and ΔTC in the same units, such we can separately measure the effect of saliency from a change in tax credit generosity, we compute continuous measures of WFTC and its predecessor, Family Credit, to disentangle the two effects. In the analysis, in Section 6, we present the results for both measures.

Using the policy-eligibility criteria, we calculate the amount of WFTC a household is eligible for and use this to measure the direct effect of WFTC. We weight the WFTC variable by weekly household income, giving us a "WFTC-rate" (i.e., WFTC/weekly household income), which is a weighted non-linear variable. ΔTC measures the change in the generosity from Family Credit to WFTC, also weighted by households' weekly income, where β_4 measures the intensive margin (increased payment for those moving from FC to WFTC). In principle, those effects that affect β_4 should be similar to those that affect β_2, but the magnitudes may differ. This seems the most parsimonious specification, as the generosity term is in the same units as the saliency term, and hence, it should absorb the linear impact of generosity. The nature of these variable allows us to distinguish between the two important changes with regard to the WFTC: the change in generosity from Family Credit to WFTC, and the change in salience from payment as a welfare benefit to payment through the employer.

Since the amount of WFTC (and Family Credit) for which a household is eligible is computed using household income rather than the individual wage, we match earners within each household and then estimate the amount of WFTC the household is entitled to claim using the eligibility criteria. The weekly WFTC payment has three main parts: (1) a basic credit of £59.00 (one for each family); (2) a 30-hour tax credit bonus of £11.45 (where either worker in the couple works at least 30 hours per week); and (3) a tax credit of between £19.85 and £26.00 for each child, depending on their age, in the eligible household.[9] The payable WFTC is based on all the components added together to make a maximum credit. If household income is above £92.90 per week, the maximum WFTC is reduced. In particular, there is a reduction of £0.55 for each pound over £92.90. If income is below £92.90, the maximum WFTC is payable. In the analysis, we account for changes in the rates over the sample period.[10] Similarly, when computing ΔTC, the change from Family Credit to WFTC, we use the eligibility criteria for each policy to compute the amount.

The indirect effect, or spillover effect, TC^s, is a vector that includes the average entitlement of WFTC workers (weighted by the fraction of those eligible) in each education

[9]The criteria also specify that the household should have low savings. The LFS does not report data on savings, so we cannot use savings in constructing the WFTC variable. However, because only 3.6 percent of couples and 2.7 percent of single parents report having savings over £5000 and no one on maximum awards reports having savings over £5000, this should not pose a problem (Inland Revenue Quarterly Enquiry (2001)).

[10]The year-on-year changes in tax credit rates are incorporated when calculating WFTC. A summary of policy parameters (1999–2002) can be found in Table A.1 of the Online Supplemental Material.

group and each industry group, separately, where β_3 measures this spillover. The analytical discussion in Section 2 suggests that as the elasticity of substitution between eligible and noneligible workers increases and/or as the fraction of eligible worker increases, there is a spillover effect onto the wages of all "similar" workers. These externalities are often ignored in the literature, such that the full policy effect is not measured. Here, we measure the effect of WFTC on the wages of all similar workers, regardless of whether they are eligible.

The representation of eligible workers differs across both the industry and education groups. According to a survey of employers report, only 15 percent of employers employed WFTC-eligible workers (Coleman et al. (2003)). There is no difference in outsourcing of payroll services by firms with a higher proportion of WFTC workers; however, WFTC employees are more concentrated in some industries than others (see Tables 1a and 1b). To construct a variable that captures the spillover effect, we use the variation in WFTC eligibility across the industry and education groups separately. We calculate the average entitlement to WFTC among workers in each industry and in each education group and then weight it by the fraction of eligible workers. Tables 1a and 1b report the number of eligible workers, the proportion of eligible workers, and the average WFTC in each education and industry group, respectively. From the tables, we see that there is a great deal of variability in these groups, and thus, we would expect the indirect effect of WFTC on the wages of workers to be stronger the larger the fraction (and importance) of this group.

Table 1a. The number of WFTC—eligible by industry.

	Total	No. of Eligible	% of Eligible/Total	Average WFTC Rate
Agriculture & Fishing	1512	174	11.51%	0.37
Energy & Water	2518	57	2.26%	0.2
Manufacturing	35,086	1585	4.52%	0.28
Construction	10,033	507	5.05%	0.28
Distribution, Hotels & Restaurants	39,054	5369	13.75%	0.64
Transport & Communication	14,011	776	5.54%	0.33
Banking, Finance, Insurance	30,189	1576	5.22%	0.37
Public Admin, Education & Health	62,676	5331	8.51%	0.44
Other Services	9746	1068	10.96%	0.61

Table 1b. The number of WFTC—eligible by education (no underline) group.

	Total	No. of Eligible	% of Eligible/Total	Average WFTC Rate
University	37,696	572	1.52%	0.01
High School_18 (A-Level or equiv.)	79,036	5181	6.56%	0.13
High School_16 (GCSE or equiv.)	65,914	7818	11.86%	0.23
No Qualifications	21,355	2828	13.24%	0.11

Note: Data from the UK Quarterly Labour Force Survey (2001–2003). *Total* describes the total number of workers (aged 21 to 60) in each group. *No. of Eligible* are the number of workers eligible for WFTC, and the *Average WFTC Rate* is the average WFTC amount divided by (predicted) household income for all eligible in each group. These figures show the averages by combining men and women, but in the analysis, we use the averages for each group separately.

The estimate of the spillover effect relies on the assumption that the trend element introduced in the counterfactual wage correctly captures the aggregate evolution of wages. We add time (quarterly) dummies to capture common trends and to adjust the wage for changes in country-level average earnings and general inflation using quarterly indexes. In addition, in Section 7.1, we test for the common trends assumption using a placebo treatment. Any aggregate shifts in wages associated with WFTC should be captured by the spillover variable. Note, however, that the calculation of the direct effect of the WFTC on eligible workers does not rely on this assumption. We later show that there is a good match between the eligible and noneligible workers with similar predicted wages; thus, one can interpret the estimates of the direct effect of WFTC as a matching estimate in which non-eligible workers who are marginally different from eligible workers act as a control group.

5.2 *Step one: The predicted "counterfactual" wage*

Correctly estimating the predicted "counterfactual" wage is key to the rest of our analysis. This is the first step in our two-step procedure. We first describe the construction of this variable and then provide evidence for its validity. Note that throughout the period, Family Credit was in operation, such that effects of WFTC are estimated relative to this previous reform.

We estimate the expected counterfactual (log) wage, W^c, using a linear regression on the (log) wage before 1999. This is done by controlling for individual, family and job characteristics in the vector, X, where X is a $1 \times K$ vector of conditioning variables. The controls include the following: age, education, region, ethnicity, experience (plus higher orders), tenure (plus higher orders), marital status, number and age of children, firm size, public ownership, occupation type, industry type, and full-time status. The aim of this exercise is to predict the wage as closely as possible to the earned wage without WFTC.[11]

The predicted wage, $\hat{W}^c$, is given by

$$E(\log Wage \mid X)_{it} = \hat{\alpha} X_{it} = \hat{W}^c_{it}. \tag{2}$$

There are 39,890 observations for men and 40,121 for women. The R-squared in both cases is approximately 55 percent. Table 2 reports these results for men and women separately.

5.2.1 *Validity of the predicted wage* In this section, we address three important assumptions. First, eligible and non-eligible workers with the same predicted wage are comparable. Second, the residual wage is similar for eligible and noneligible workers in the absence of WFTC. Third, while there are some compositional changes over time,

[11]Given that there were other policies similar to WFTC in operation prior to the introduction of WFTC, our analysis will give us only the relative change from these policies. As a robustness check, in Section 8, we repeat our analysis on an earlier time period to ensure that the differential effects between eligible and non-eligible workers did not exist.

TABLE 2. The predicted (log) wage.

	Men	Women	Cont.	Men	Women	Cont.	Men	Women
Age (25–34)	0.240***	0.164***	South West	0.0700***	0.0285**	Ind. (Construction)	0.157***	0.00106
	[0.00706]	[0.00635]		[0.0156]	[0.0141]		[0.0190]	[0.0286]
Age (35–44)	0.285***	0.193***	Wales	0.0385**	0.0434***	Ind. (Distri, Hotels, Rest)	−0.0158	−0.112***
	[0.00799]	[0.00720]		[0.0168]	[0.0150]		[0.0185]	[0.0249]
Age (45–54)	0.272***	0.162***	Strathclyde	0.0315*	0.0325**	Ind. (Transp & Com)	0.102***	0.0365
	[0.00873]	[0.00780]		[0.0171]	[0.0153]		[0.0190]	[0.0260]
Age (55–60)	0.186***	0.121***	Rest of Scotland	0.0573***	0.0396***	Ind. (Banking, Finance, Ins)	0.225***	0.0869***
	[0.0106]	[0.0104]		[0.0164]	[0.0146]		[0.0188]	[0.0249]
Black	−0.110***	−0.0499***	Northern Ireland	−0.00504	−0.00859	Ind. (Public Admin)	0.0898***	−0.0670***
	[0.0171]	[0.0143]		[0.0180]	[0.0161]		[0.0199]	[0.0251]
Asian	−0.148***	−0.0639***	Part time	−0.0842***	−0.0429***	Ind. (Other Services)	0.029	−0.0811***
	[0.0129]	[0.0129]		[0.0105]	[0.00409]		[0.0204]	[0.0257]
Other ethnic	−0.140***	−0.0271	Public Sector	0.0163**	0.0863***	Ind. (Work outside UK)	0.323***	−0.361**
	[0.0291]	[0.0253]		[0.00774]	[0.00547]		[0.0944]	[0.170]
Married	0.107***	0.0130**	No. Children (1)	0.00203	0.00342	Tenure	−0.00446	0.00125
	[0.00582]	[0.00529]		[0.0143]	[0.0118]		[0.0116]	[0.00975]
Divorced/widowed	0.0712***	0.0180**	No. Children (2)	0.0303**	−0.00642	Tenure^2	$-2.36e{-}05$	$6.58e{-}06$
	[0.00933]	[0.00721]		[0.0154]	[0.0127]		$[6.12e{-}05]$	$[5.15e{-}05]$
High school_18	−0.199***	−0.157***	No. Children (3)	0.0163	−0.0382***	Experience	0.00243***	0.00266***
	[0.00600]	[0.00595]		[0.0172]	[0.0148]		[0.000190]	[0.000192]
High school_16	−0.287***	−0.240***	No. Children (4+)	−0.0414*	−0.0576**	Experience^2	$-1.13e{-}05$***	$-1.62e{-}05$***
	[0.00662]	[0.00629]		[0.0235]	[0.0238]		$[2.07e{-}06]$	$[2.38e{-}06]$
No qualifications	−0.377***	−0.327***	Occ. (Manager)	0.421***	0.497***	Experience^3	$2.99e{-}08$***	$5.31e{-}08$***
	[0.00874]	[0.00788]		[0.00887]	[0.0134]		$[7.34e{-}09]$	$[9.61e{-}09]$
Rest of NE	0.021	0.00196	Occ. (Professional)	0.348***	0.657***	Experience^4	$-2.35e{-}11$**	$-6.09e{-}11$***
	[0.0186]	[0.0165]		[0.00971]	[0.0144]		$[6.89e{-}12]$	$[1.22e{-}11]$
Gtr Manchester	0.0419**	0.0259*	Occ. (Asso. Prof)	0.283***	0.431***	Constant	1.439***	1.418***
	[0.0174]	[0.0155]		[0.00982]	[0.0137]		[0.0301]	[0.0339]

(*Continues*)

TABLE 2. Continued.

	Men	Women	Cont.	Men	Women	Cont.	Men	Women
Merseyside	−0.00152	−0.00648	Occ. (Clerical)	−0.0172*	0.208***	Observations	39,890	40,121
	[0.0200]	[0.0175]		[0.00996]	[0.0127]	**R-squared**	**0.546**	**0.553**
Rest of NW	0.0407**	0.0281*	Occ. (Craft)	0.0439***	0.0641***			
	[0.0168]	[0.0150]		[0.00915]	[0.0133]			
South Yorkshire	0.0168	−0.0056	Occ. (Personnel)	0.120***	0.109***			
	[0.0189]	[0.0171]		[0.0113]	[0.0136]			
West Yorkshire	0.0494***	0.0399**	Occ. (Sales)	−0.0472***	0.0144			
	[0.0175]	[0.0156]		[0.00937]	[0.0140]			
Rest of Yorkshire	0.0582***	0.00242	Occ. (Plant/Mach.)	−0.0998***	−0.0392***			
	[0.0180]	[0.0163]		[0.0104]	[0.0137]			
East Midlands	0.0499***	0.0225	Occ. (Others)	−0.0778***	−0.0526***			
	[0.0159]	[0.0143]		[0.0172]	[0.0157]			
West Midlands	0.0774***	0.0395***	Size Firm (11–19)	0.0775***	0.0472***			
	[0.0168]	[0.0153]		[0.00812]	[0.00646]			
Rest of W. Mid.	0.0729***	0.0296**	Size Firm (20–24)	0.108***	0.0630***			
	[0.0164]	[0.0148]		[0.00939]	[0.00788]			
Eastern	0.160***	0.105***	Size Firm (25–49)	0.111***	0.0702***			
	[0.0155]	[0.0140]		[0.00712]	[0.00595]			
Inner London	0.282***	0.292***	Size Firm (50+)	0.197***	0.124***			
	[0.0182]	[0.0161]		[0.00571]	[0.00473]			
Outer London	0.255***	0.248***	Ind. (Energy & Water)	0.242***	0.127***			
	[0.0161]	[0.0144]		[0.0226]	[0.0332]			
South East	0.187***	0.131***	Ind. (Manu)	0.142***	0.0452*			
	[0.0151]	[0.0136]		[0.0185]	[0.0251]			

Note: Data from the UK Quarterly Labour Force Survey (1997–1999). The coefficients and standard errors (in parenthesis) are reported. *** represents significance at the 1% level, ** represents significance at the 5% level, and * represents significance at the 10% level. The sample includes individuals aged 21–60. Full-time students, the sick/disabled, or those in government training programs are removed from the sample. A detailed description of all variables can be found in the Online Supplemental Material.

there are no important differences between eligible and noneligible workers after WFTC is introduced.[12]

Using the main observable characteristics, we check whether eligible and noneligible workers who have the same predicted wage are similar outside the variables that determine eligibility. In Tables 3a and 3b, we compare the observable characteristics of matched eligible and noneligible workers before the introduction of WFTC, where the match is based on the predicted wage, $\hat{W}^c$. We make this comparison at different percentiles of $\hat{W}^c$, with the objective of showing that when eligible and noneligible workers have the same predicted wage, they are observably similar. We do this separately for men and women.

Table 3a shows that for women, as we would expect, there are differences in the variables that determine eligibility (hours of work and number of children). However, in the other demographic variables, the groups are very similar.[13] We capture a majority of our eligible sample in the bottom 40 percent of the distribution.[14] In Table 3b, for men, we see similar patterns. Here, we capture most of our sample in the bottom 30 percent of the distribution.

We estimate the residual wage difference of the predicted wage from the actual wage separately for eligible and noneligible workers before the introduction of WFTC. We then compare the distribution of the residuals. From Figures A.6a and A.6b, we see that for both men and women, the patterns are very close. These figures highlight the comparability of the groups. As a further robustness check, we perform a placebo treatment of the same nature. We predict the counterfactual wage using one year pre-WFTC (1997) and then estimate the residuals for another pre-WFTC year (1998). Here, again, we find that distributions for eligible and noneligible workers are comparable and are similar to those presented in Figures A.6a and A.6b.

An important concern would be if the composition of the eligible versus non-eligible group changes as a response to the introduction of WFTC. We show that the relative rates of return, α, on the vector X remain the same in the post-WFTC period. This is not to

[12]We estimate the wage model on the preperiod using employed workers only and then predict counterfactual wages on the post-period for employed workers, which would include those who were not working before. The implicit assumption is, therefore that, had nonworkers in the preperiod worked, their salary would be close to other employed workers with similar observable characteristics. Azmat (2014) showed that there are no significant effects of the policy on the inactivity-activity margin and only very modest effects on the employment-unemployment margin. Moreover, given that the minimum wage is binding for the lowest paid workers, and that the policy mostly affects low pay workers, there should be common bounds for the wages of employed workers and the counterfactual for nonworkers.

[13]In Figures A.4 and A.5, we plot the distributions of the continuous characteristics for eligible and noneligible group, matched by predicted wages, for men and women, respectively. We see that, with the exception of age, the matching is satisfactory. The issue with respect to age relates to the presence of children. While the eligible group, by construction, have children aged 16 or under, the noneligible group may not have children or have children who are older than 16. For this reason, the distribution is skewed further to the right for the noneligible group. We plot the age distribution conditioned on the presence of children under the age of 16 and find that the match is satisfactory. As a robustness check, we restrict the analysis to those with children under the age of 16 and find that the main results are unchanged. These results are reported in Table A.4.

[14]When we look at the rest of the distribution, the eligible and noneligible workers continue to be similar in their observable characteristics, but there are fewer observations.

TABLE 3a. Descriptive statistics for women—matched (pre-WFTC).

	$5\% \leq \hat{W}^c < 10\%$					$10\% \leq \hat{W}^c < 15\%$					$15\% \leq \hat{W}^c < 20\%$				
	Ineligible		Eligible			Ineligible		Eligible			Ineligible		Eligible		
	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value
Age	37.47	10.77	34.86	7.25	**0.06**	38.00	10.45	35.38	7.05	**0.06**	38.54	10.39	36.19	7.14	**0.07**
White	0.97	0.17	0.95	0.22	**0.15**	0.96	0.19	0.97	0.17	**0.26**	0.97	0.18	0.97	0.18	**0.47**
No qual.	0.36	0.48	0.28	0.45	**0.11**	0.32	0.47	0.26	0.44	**0.12**	0.29	0.45	0.25	0.44	**0.22**
Public	0.13	0.33	0.13	0.34	**0.41**	0.18	0.39	0.20	0.40	**0.29**	0.21	0.40	0.25	0.43	**0.18**
Size of firm	2.91	2.08	2.97	2.10	**0.36**	3.45	2.17	3.30	2.13	**0.23**	3.78	2.16	3.70	2.10	**0.33**
Tenure	24.52	24.08	20.96	20.62	**0.12**	29.66	27.04	27.72	26.65	**0.24**	33.16	27.41	30.08	26.49	**0.19**
Experience	37.73	48.24	25.22	29.63	**0.05**	45.26	50.52	35.06	38.77	**0.08**	54.89	57.21	38.87	39.41	**0.06**
Single	***0.24***	***0.43***	***0.25***	***0.43***	***0.41***	***0.22***	***0.41***	***0.22***	***0.42***	***0.43***	***0.21***	***0.40***	***0.22***	***0.41***	***0.35***
Hours	***24.81***	***13.42***	***20.71***	***11.40***	***0.05***	***26.24***	***13.32***	***20.99***	***12.52***	***0.05***	***27.08***	***12.79***	***21.25***	***10.96***	***0.04***
Children	***0.43***	***0.49***	***1.00***	***0.00***	***0.01***	***0.43***	***0.50***	***1.00***	***0.00***	***0.01***	***0.42***	***0.49***	***1.00***	***0.00***	***0.01***
Obs.	1185		396			1194		343			1157		301		

	$20\% \leq \hat{W}^c < 25\%$					$25\% \leq \hat{W}^c < 30\%$				
	Ineligible		Eligible			Ineligible		Eligible		
	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value
Age	38.96	9.97	36.40	7.18	**0.06**	39.80	9.91	36.18	6.34	**0.04**
White	0.97	0.18	0.95	0.22	**0.19**	0.97	0.17	0.93	0.25	**0.12**
No qual.	0.23	0.42	0.18	0.38	**0.15**	0.21	0.41	0.16	0.37	**0.15**
Public	0.24	0.43	0.28	0.45	**0.19**	0.30	0.46	0.30	0.46	**0.45**
Size of firm	4.03	2.10	4.00	2.13	**0.43**	4.13	2.12	3.85	2.16	**0.14**
Tenure	34.85	28.33	33.69	28.87	**0.34**	36.96	29.90	34.51	27.88	**0.24**
Experience	61.24	61.91	47.17	48.02	**0.07**	69.47	66.41	52.05	50.40	**0.06**
Single	***0.20***	***0.40***	***0.20***	***0.40***	***0.48***	***0.18***	***0.38***	***0.18***	***0.38***	***0.46***
Hours	***27.99***	***12.41***	***22.86***	***12.05***	***0.05***	***28.43***	***12.93***	***22.33***	***11.58***	***0.04***
Children	***0.41***	***0.49***	***1.00***	***0.00***	***0.01***	***0.37***	***0.48***	***1.00***	***0.00***	***0.01***
Obs.	1263		307			1336		291		

Note: Data from the UK Quarterly Labour Force Survey (1998). The sample includes individuals aged 21 to 60. Full-time students, the sick/disabled, or those in government training programs are removed from the sample. Using the predicted wage, $\hat{W}^c$, we match eligible and noneligible workers and compare workers at different percentiles in the distribution of $\hat{W}^c$. *Age* of individual ranges from 21 to 60. *White* takes value 1 if the individual is white and 0 otherwise. *No Qual.* takes value 1 if the individual has no qualifications and 0 otherwise. *Public* takes value 1 if the individual works in the public sector and 0 otherwise. *Size of firm* takes values 1 to 7, where size of firm is bracketed (1–10, 11–19, 20–24, 25–49, 50+), 1 represents the smallest firm size and 7 the largest. *Tenure* and *Experience* are measured in months. *Single* takes value 1 when the individual is single and 0 otherwise. *Hours* measures the hours of work. *Children* takes value 1 if dependent children are present in the household.

TABLE 3b. Descriptive statistics for men—Matched (pre-WFTC).

	$5\% \leq \hat{W}^c < 10\%$					$10\% \leq \hat{W}^c < 15\%$					$15\% \leq \hat{W}^c < 20\%$				
	Ineligible		Eligible			Ineligible		Eligible			Ineligible		Eligible		
	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value
Age	34.62	12.24	34.11	8.88	**0.30**	36.46	11.81	34.88	7.75	**0.14**	37.58	11.31	35.32	8.48	**0.12**
White	0.95	0.23	0.82	0.39	**0.07**	0.96	0.21	0.86	0.34	**0.10**	0.95	0.22	0.91	0.28	**0.21**
No qual.	0.24	0.43	0.32	0.47	**0.13**	0.22	0.41	0.27	0.45	**0.20**	0.21	0.41	0.24	0.43	**0.29**
Public	0.11	0.31	0.09	0.29	**0.27**	0.12	0.32	0.08	0.28	**0.20**	0.14	0.34	0.14	0.35	**0.44**
Size of firm	3.60	2.16	2.77	2.09	**0.06**	3.95	2.14	3.17	2.13	**0.08**	4.26	2.06	3.99	2.15	**0.22**
Tenure	26.69	25.62	23.40	23.73	**0.19**	27.92	26.53	25.15	24.66	**0.25**	31.29	27.88	27.43	24.29	**0.21**
Experience	41.65	56.44	35.02	42.81	**0.15**	50.15	67.89	56.95	78.00	**0.25**	63.09	75.75	42.17	49.79	**0.08**
Single	***0.59***	***0.49***	***0.29***	***0.45***	***0.04***	***0.48***	***0.50***	***0.19***	***0.39***	***0.04***	***0.41***	***0.49***	***0.19***	***0.40***	***0.06***
Hours	***43.97***	***15.27***	***35.26***	***10.41***	***0.03***	***45.09***	***14.19***	***37.93***	***7.75***	***0.03***	***45.43***	***11.70***	***35.98***	***7.44***	***0.03***
Children	***0.18***	***0.39***	***1.00***	***0.00***	***0.00***	***0.26***	***0.44***	***1.00***	***0.00***	***0.01***	***0.31***	***0.46***	***1.00***	***0.00***	***0.01***
Obs.	1474		188			1380		133			1463		104		

	$20\% \leq \hat{W}^c < 25\%$					$25\% \leq \hat{W}^c < 30\%$				
	Ineligible		Eligible			Ineligible		Eligible		
	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value
Age	38.49	11.01	36.10	7.37	**0.11**	38.76	10.83	38.86	7.21	**0.47**
White	0.96	0.20	0.83	0.38	**0.10**	0.96	0.20	0.95	0.22	**0.45**
No qual.	0.18	0.38	0.30	0.46	**0.12**	0.15	0.36	0.24	0.43	**0.20**
Public	0.15	0.35	0.09	0.28	**0.15**	0.16	0.36	0.21	0.42	**0.27**
Size of firm	4.44	2.01	4.56	2.06	**0.34**	4.53	1.95	4.24	2.15	**0.27**
Tenure	33.87	29.41	28.71	24.67	**0.19**	37.51	29.37	25.49	29.33	**0.14**
Experience	73.89	83.49	59.35	69.15	**0.16**	80.49	82.44	63.79	81.54	**0.21**
Single	***0.35***	***0.48***	***0.13***	***0.34***	***0.06***	***0.28***	***0.45***	***0.12***	***0.33***	***0.10***
Hours	***45.82***	***12.61***	***36.57***	***5.69***	***0.02***	***45.61***	***11.47***	***33.43***	***8.67***	***0.04***
Children	***0.32***	***0.47***	***1.00***	***0.00***	***0.01***	***0.38***	***0.48***	***1.00***	***0.00***	***0.01***
Obs.	1672		82			1181		42		

Note: Data from the UK Quarterly Labour Force Survey (1998). The sample includes individuals aged 21 to 60. Full-time students, the sick/disabled, or those in government training programs are removed from the sample. Using the predicted wage, $\hat{W}^c$, we match eligible and noneligible workers and compare workers at different percentiles in the distribution of $\hat{W}^c$. See Table 3a for a description of the variables.

say that the rates of return are unchanged throughout but that any change in the rates of return are similar for both eligible and noneligible workers with the same predicted wage. Below we discuss and show that the analysis does not suffer from these selection issues.

First, the Quarterly Labour Force Survey dataset used has a detailed education variable (which proxies for skill), so we do not have the issue of selection on observables. It can be seen from the descriptive statistics in Table 4 that the proportion of people with no education who are eligible for the tax credit does not increase relative to the noneligible group after 1999.

Second, in the case of WFTC, there is evidence to suggest that its overall impact on employment and hours of work are small (and, in turn, have a small impact on the compositional change of workers). This too can be seen from Table 4, where hours worked does not change relative to the noneligible group after the policy introduction. A number of papers have studied the labor supply effect of WFTC on single parents (see Brewer and Browne (2006) and Brewer et al. (2009) for a review of studies). For single parents, mostly focused on single mothers, using different data and different methodologies, these papers find that employment increased between 0.6 and 5 percentage points.[15]

TABLE 4. Descriptive statistics before and after 1999—unmatched

	Ineligible					Eligible				
	Before		After			Before		After		
	Mean	SD	Mean	SD	p-value	Mean	SD	Mean	SD	p-value
Age	38.00	11.28	38.53	11.23	**0.04**	33.78	9.01	34.22	9.13	**0.12**
White	0.96	0.19	0.96	0.19	**0.33**	0.92	0.27	0.92	0.27	**0.32**
No qualifications	0.11	0.31	0.10	0.30	**0.05**	0.21	0.41	0.17	0.38	**0.06**
Public	0.28	0.45	0.29	0.45	**0.10**	0.24	0.43	0.24	0.43	**0.35**
Tenure	33.51	28.99	33.05	27.82	**0.13**	25.63	25.73	27.16	25.67	**0.11**
Experience	93.04	96.56	93.62	97.86	**0.25**	43.56	53.66	45.61	55.51	**0.15**
Single	0.30	0.46	0.30	0.46	**0.46**	0.29	0.46	0.31	0.46	**0.16**
Hours	37.01	13.48	37.00	14.12	**0.44**	24.21	12.69	24.02	12.47	**0.29**
Real hourly wage	8.38	5.39	8.90	5.78	**0.02**	5.26	3.12	5.64	3.25	**0.05**
Small firms	0.29	0.46	0.29	0.45	**0.19**	0.48	0.50	0.46	0.50	**0.17**
Medium firms	0.15	0.35	0.14	0.35	**0.30**	0.16	0.37	0.17	0.38	**0.15**
Large firms	0.56	0.50	0.56	0.50	**0.17**	0.35	0.48	0.35	0.48	**0.49**
Observations	63,350		56,930			5372		5002		

Note: Data from the UK Quarterly Labour Force Survey. The *Before* period is for 1997–1998, and the *After* period is for 2000–2001. Here, we see the means and standard deviations in brackets for each group. The sample includes individuals aged 21–60. Full-time students, the sick/disabled, or those in government training programs are removed from the sample. *Small Firms* is the percentage of workers working in firms with fewer than 25 workers. *Medium Firms* is the proportion of workers working in firms with 25 to 49 workers. *Large Firms* is the proportion of workers working in firms with more than 50 workers. See the notes for Table 3a for a description of all other variables.

[15] Blundell et al. (2000) developed a structural model of labor supply identified from past tax and welfare reforms, which they then used to simulate the effect of WFTC. Their model showed that WFTC would lead to a 2.2 percentage point increase in single parents' employment. Brewer et al. (2006) reported results from an updated version of this model, incorporating evidence over the period WFTC was introduced, and found that single mothers' employment rose by 3.7 percentage points. Francesconi and van der Klaauw

On the intensive margin, Blundell et al. (2005) showed there was an increase in average hours among single mothers who remained in employment. Moreover, Gregg, Harkness, and Smith (2009) showed there was an increase in hours among single mothers working fewer than 16 hours before the reform by approximately 3 hours, and there was a reduction in hours among those working more than 16 hours before the reform by 1.7 hours. For married couples, overall, studies show either no or small (negative or positive) effects of WFTC on the intensive and extensive margin. Francesconi, Rainer, and van der Klaauw (2009) found no statistically significant labor supply effect for married women overall and if their partners worked full-time but around 3 percentage point effect if their partner did not work or worked less than 16 hours. They find no measurable effect on the labor supply of married men, irrespective of their partner's employment status.[16] The small employment change is an important distinction between the WFTC and the US Earned Income Tax Credit, which Blundell and Hoynes (2004) discussed in detail. In the analysis that follows, we look separately at men and women and, for each, at the effect on the sub-samples of married and single parents.

Finally, the minimum wage in the UK, introduced in April 1999, imposes a lower bound below which the employer cannot cut the gross wage. This suggests that an influx of lower-skilled workers will not impact the wage as severely as it would have without a minimum wage. The minimum wage is a ceiling below which the employer cannot cut the wage. In our analysis, we use the minimum wage as a point of censor when measuring the policy effectiveness.

5.3 *Stage two: Estimating the wage change*

Using the counterfactual wage estimated in the previous section, we now estimate the wage change resulting from the introduction of WFTC. In this section, we describe the estimation strategy used.

We estimate

$$W_{it} = \beta_0 + \beta_1(\hat{W}^c_{it} + \beta_2\big(TC^d_{it}\big) + \beta_3\big(TC^s_{gt}\big) + \beta_4 \Delta TC_{it} + u_{it}, \tag{3}$$

where $\hat{W}^c$ is the predicted wage, which has been corrected for changes in average earnings and general inflation using quarterly indexes, t. We bootstrap with 200 replications

(2007) estimated the impact of the whole package of the 1997 Labour Government reform by employing a difference-in-difference approach, comparing employment of single mothers with that of single women with no children, and show that single mothers' employment rose by 5 percentage points. Using an equilibrium job search model, Shephard (2017) took an integrated view of the tax system to analyze the labor market impact of tax reform. He too finds around a 5 percentage point increase in single parents' employment. Leigh (2007) also compared eligible single women with and without children over the short term before and after the introduction of WFTC and found an (insignificant) employment effect of 0.6 percentage points. Gregg, Harkness, and Smith (2009) also employed a difference-in-difference approach and compared single mothers (parents) with different comparison groups and found that employment increases between 3.8 and 5.2 percent.

[16]In line with what is common in this literature, throughout the article, the terms "marriage," "couples," "married couples," and "marital unions" are used in a broad sense to include all types of live-in partnerships, such as cohabitations, stepfamilies, and blended families.

when constructing the counterfactual wage and when estimating the wage change. All variables are as described in equation (1) in Section 5.1.

To take into account the distortion in wages brought about by the introduction of the minimum wage in the UK, we use a censored regression model. In 1999, the minimum wage was set at £3.60 for adults aged 22 or older and £3.00 for those aged 18 to 21.[17] For those with a binding minimum wage, there exists a gap between the actual and predicted wages.[18] For those who are unaffected by the minimum wage (i.e., those who were previously earning above the national minimum), no gap exists between the actual and predicted wages.

We adjust the econometric specification to take into account the left-censoring that is generated by the minimum wage. A model that is directly relevant here is the censored least absolute deviation (LAD) (Powell (1984)).[19] This is a nonparametric specification that has the advantage over parametric methods, such as a censored Tobit, that the consistency of the estimator does not require knowledge of the distribution of the error term u_i; nor is it assumed that the distribution is homoscedastic, only that it has median zero. In turn, it is robust to nonnormality of the error terms, and it is robust to heteroskedasticity (which is common in most cross-sectional datasets).

Powell (1984) shows that the median function is equal to the function maximum, such that:

$$\begin{aligned} &q_{50}\big(W_{it} \mid \hat{W}^s, \boldsymbol{TC}\big) \\ &\quad = \max\big(w_{\min}, q_{50}\big(\beta_0 + \beta_1\big(\hat{W}^c_{it} + \beta_2\big(TC^d_{it}\big) + \beta_3\big(TC^s_{gt}\big) + \beta_4 \Delta TC_{it} + u_{it}\big) \mid \hat{W}^c, \boldsymbol{TC}\big) \\ &\quad = \max\big(w_{\min}, \beta_0 + \beta_1\big(\hat{W}^c_{it} + \beta_2\big(TC^d_{it}\big) + \beta_3\big(TC^S_{gt}\big) + \beta_4 \Delta TC_{it}\big), \end{aligned}$$

where q_{50} denotes the median of the distribution conditional on covariates and the median distribution of u_i is assumed to be zero. $w_{\min}$ is the national minimum wage and $\boldsymbol{TC} = \{TC^d, TC^S, \Delta TC\}$. The objective of the censored LAD is to consistently estimate the vector of β by the parameter vector that minimizes:

$$\sum_{i=1}^{N} \big|W_i - \max\big(w_{\min}, \beta_0 + \beta_1\big(\hat{W}^c_{it} + \beta_2\big(TC^d_{it}\big) + \beta_3\big(TC^s_{gt}\big) + \beta_4 \Delta TC_{it} + u_{it}\big)\big|$$

In the analysis that follows, we use this specification as well as standard ordinary least squares.

[17] The minimum wage increased to £3.70 (£3.20) in 2000, £4.10 (£3.50) in 2001, and £4.20 (£3.60) in 2002 for workers aged over 21 (aged 18–21). We account for these increases in our analysis.

[18] Before the introduction of the minimum wage, approximately 3 percent of men and 12 percent of women reported an hourly wage at or below the minimum wage. Among the WFTC eligible, this was higher (5 percent of men and 17 percent of women).

[19] An alternative (parametric) specification is the Tobit model. However, it imposes more assumptions on the distribution of the error term. In previous versions of the paper, we estimate equation (3) also using the Tobit and find that the results are similar to those of OLS and CLAD. In Table A.2 of the Online Supplemental Material, we present the regression-based analysis using Tobit for men and women, respectively.

6. Results

In this section, we start with a graphical representation of our main results. We then present the regression-based analysis using OLS and the censored LAD on log wages for men and women, respectively. The latter estimation adjusts the econometric specification (3) to take into account the left-censoring generated by the minimum wage. In addition, we look separately at the results for single men, married (or cohabiting) men, single women and married (or cohabiting) women.

The regression results in Tables 5 and 6 estimate the equations in Section 5.3. We report the marginal effects of WFTC and the spillover effect on the actual (log) wage between 2000 and 2003.

6.1 *Graphical representation*

We start by showing a simple graphical analysis that illustrates the identification strategy. The graphs are a simple representation of the regression-based analysis; however, they summarize well some of the key results of the policy impact.

In Figures 2a and 3a, we show the densities of the log difference between actual and predicted wages for WFTC-eligible workers and "similar" noneligible workers. These similar workers are the noneligible workers in industries and education groups with a

Table 5. Effect of WFTC on Ln(Wages) for men.

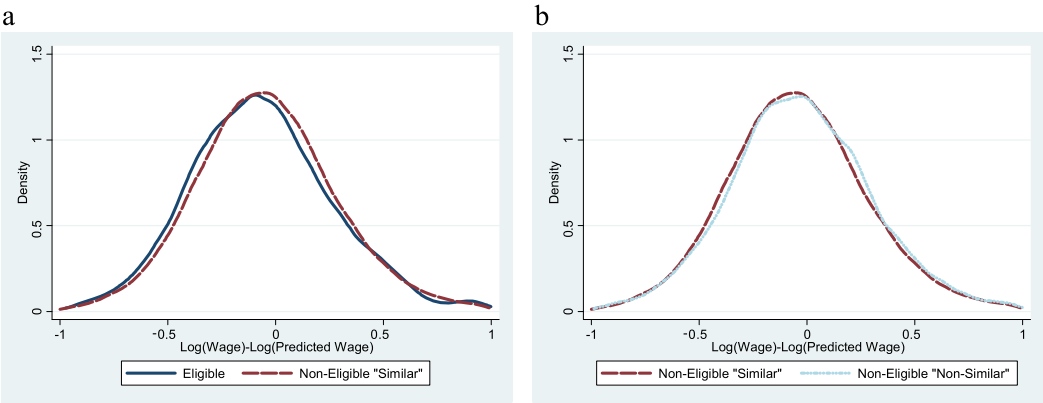
	Ln (Wages)					
	OLS	CLAD	OLS		CLAD	
	[1] ALL	[2] ALL	[3] Single	[4] Married	[5] Single	[6] Married
Predicted wage	0.991***	0.986***	0.959***	1.004***	0.951***	1.000***
	[0.00555]	[0.00564]	[0.00940]	[0.00618]	[0.00981]	[0.00737]
WFTC	−0.143***	−0.278***	−0.108*	−0.142***	−0.258***	−0.268***
	[0.0317]	[0.0489]	[0.0600]	[0.0398]	[0.0769]	[0.0606]
FC generosity	−0.0564	0.160	0.128	−0.201*	0.210	0.0352
	[0.0796]	[0.109]	[0.131]	[0.116]	[0.170]	[0.138]
Spillover (industry)	−0.0871*	−0.177***	−0.300***	0.0281	−0.397***	−0.0734
	[0.0486]	[0.0535]	[0.0769]	[0.0665]	[0.0858]	[0.0676]
Spillover (education)	−0.196***	−0.297***	0.394***	−0.539***	0.342***	−0.672***
	[0.0562]	[0.0596]	[0.0768]	[0.0694]	[0.0980]	[0.0770]
Constant	0.290***	0.305***	0.329***	0.277***	0.348***	0.293***
	[0.0149]	[0.0157]	[0.0236]	[0.0181]	[0.0259]	[0.0209]
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,245	76,147	25,661	50,584	25,634	50,534

Note: The coefficients and standard errors (in parenthesis) are reported. *** represents significance at the 1% level, ** represents significance at the 5% level, and * represents significance at the 10% level. Standard errors are bootstrapped with 200 replications. We include a full set of time dummies (each quarter of each year) in all the regressions. The regression is based on equation (3), where *Predicted wage* ($\hat{W}^c$) is the predicted wage, which has been corrected for changes in average earnings and general inflation using quarterly indexes, t. *WFTC* (TC^d), calculated as the weekly WFTC divided by households' weekly (predicted) income, measures the direct effect of WFTC on eligible workers. *FC Generosity* (ΔTC) is the change in households' entitlement from Family Credit to WFTC divided by households' weekly (predicted) income. *Spillover* (TC^s) is the average *WFTC* (weighted by the fraction of eligible) in each industry group (*Industry*) and education group (*Education*), respectively.

TABLE 6. Effect of WFTC on Ln(Wages) for women.

	Ln (Wages)					
	OLS	CLAD	OLS		CLAD	
	[1] ALL	[2] ALL	[3] Single	[4] Married	[5] Single	[6] Married
Predicted wage	0.992***	1.015***	0.961***	1.008***	0.977***	1.038***
	[0.00387]	[0.00411]	[0.00676]	[0.00475]	[0.00693]	[0.00504]
WFTC	0.0429***	0.000308	0.0341**	0.0433***	−0.0167	0.00339
	[0.0103]	[0.00946]	[0.0152]	[0.0133]	[0.0135]	[0.0132]
FC generosity	−0.0700**	−0.0109	−0.0890	−0.0710	−0.0259	−0.0147
	[0.0353]	[0.0314]	[0.0559]	[0.0444]	[0.0470]	[0.0419]
Spillover (industry)	−0.138***	−0.0927***	−0.120***	−0.149***	−0.0765***	−0.0935***
	[0.0146]	[0.0153]	[0.0221]	[0.0215]	[0.0250]	[0.0192]
Spillover (education)	−0.0325**	−0.0254*	0.132***	−0.130***	0.126***	−0.109***
	[0.0141]	[0.0144]	[0.0220]	[0.0178]	[0.0235]	[0.0180]
Constant	0.264***	0.203***	0.296***	0.252***	0.249***	0.175***
	[0.00975]	[0.0108]	[0.0171]	[0.0124]	[0.0179]	[0.0133]
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80,878	80,432	28,604	52,274	28,451	51,965

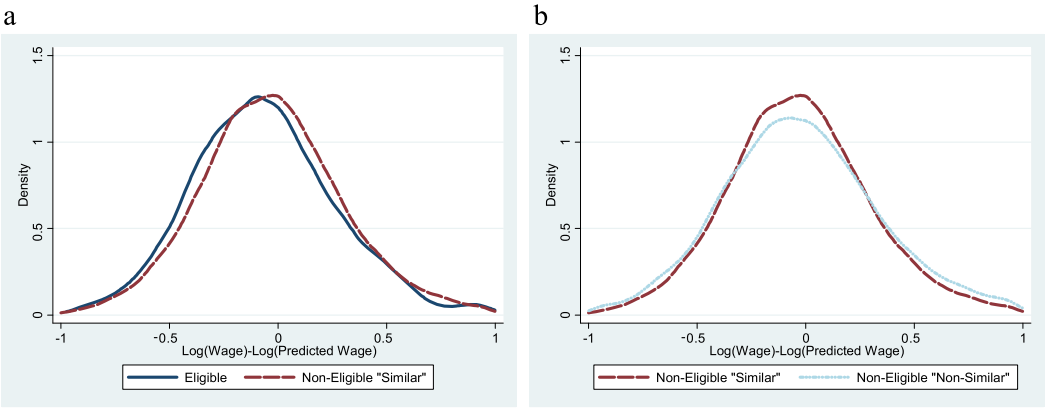
Note: The coefficients and standard errors (in parenthesis) are reported. *** represents significance at the 1% level, ** represents significance at the 5% level, and * represents significance at the 10% level. Standard errors are bootstrapped with 200 replications. We include a full set of time dummies (each quarter of each year) in all the regressions. For definitions of variables, see Table 5.



NOTES: "Eligible" are workers eligible for WFTC. "Noneligible ("Similar")" in Figure 2a are workers not eligible for WFTC in the Agriculture and Fishing, Energy and Water, and Construction industry categories, which have the highest density of WFTC-eligible workers. "Noneligible ("Nonsimilar")" in Figure 2b are workers not eligible for WFTC in the Manufacturing, Distribution, Hotels and Restaurants, and Public Admin, Education and Health (University education) categories, which have the lowest density of WFTC-eligible workers. See Table 1a for % of WFTC-eligible workers by industry category.

FIGURE 2. Kernel density of WFTC effect on wage for men (Industry comparison).

high density of eligible workers, respectively. We observe that the wage distribution of the treated workers is to the left of the wage distribution observed for the noneligible workers. This shows that the policy had an effect and that this effect was negative, re-



NOTES: "Eligible" are workers eligible for WFTC. "Noneligible ("Similar")" in Figure 3a are workers not eligible for WFTC in the High School_16 education category, which has the highest density of WFTC-eligible workers. "Noneligible ("Nonsimilar")" in Figure 3b are workers not eligible for WFTC in the University education category, which has the lowest density of WFTC-eligible workers. See Table 1b for % of WFTC-eligible workers by education category.

FIGURE 3. Kernel density of WFTC effect on wage for men (Education comparison).

sulting in lower than predicted wages after the policy was implemented compared with the noneligible workers. Moreover, we see that the treatment affects all parts of the distribution. The results are unchanged when we look at the different groups.

Given the possibility of a treatment spillover onto the noneligible similar workers, we further compare these workers with an alternative group that has a low eligibility probability. The differences between these two groups illustrate the existence of spillover effects to workers who are noneligible but have similar characteristics to those eligible. From Figures 2b and 3b, we see that while the spillover effect is less striking than the direct effect, when using the industry-group variation, the wage distribution of the eligible-similar workers is to the left of the wage distribution observed for the noneligible-less similar workers.

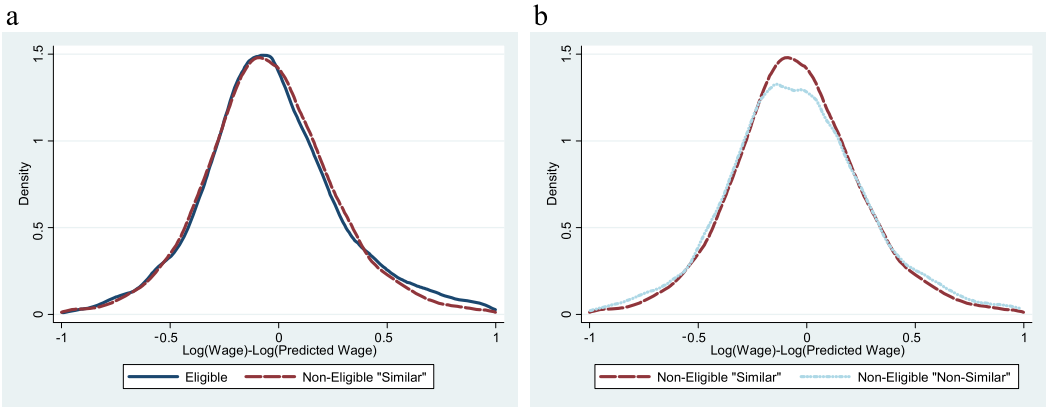
In Figures 4a, 4b, 5a, and 5b, we replicate the above analysis for women. Interestingly, we see that while the direct effects are small, the spillover effects are more notable. In particular, from Figure 5b, we see that while there is a negative effect on the top half of the distribution, there is a positive effect on the bottom half. This may be a consequence of the minimum wage, which binds for the bottom of the distribution.

In the analysis that follows, we will quantify these results and test their robustness.

6.2 *Regression analysis*

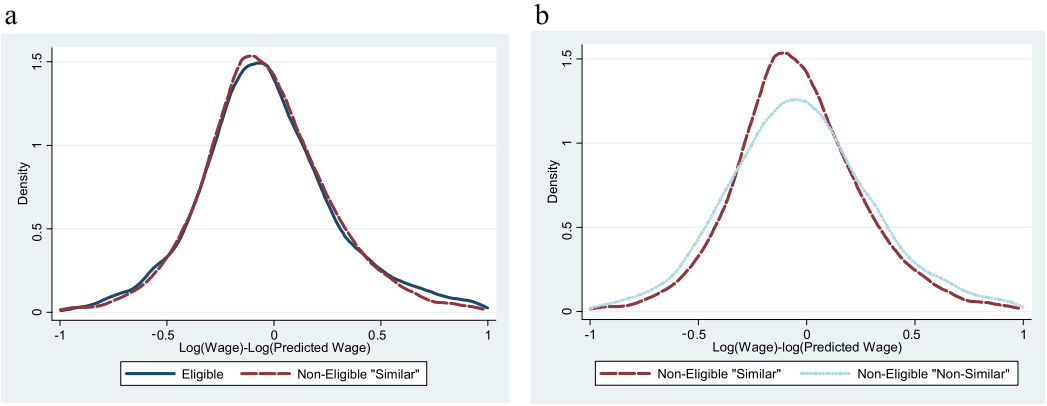
The regressions are first performed for men, with the output displayed in Table 5. The results are shown for the OLS and the CLAD estimation.[20] Columns (1) and (2) show the

[20]The number of observations is very slightly lower for CLAD because the estimation technique used for the CLAD estimator is an iterative linear programming algorithm for which observations are dropped if the predicted value is less than the censoring value when the left tail of the distribution is censored, or they are dropped if the predicted value is greater than the censoring value when the right tail of the distribution is censored.



NOTES: "Eligible" are workers eligible for WFTC. "Noneligible ("Similar")" in Figure 4a are workers not eligible for WFTC in the Agriculture and Fishing, Energy and Water, and Construction industry categories, which have the highest density of WFTC-eligible workers. "Noneligible ("Nonsimilar")" in Figure 4b are workers not eligible for WFTC in the Manufacturing, Distribution, Hotels and Restaurants, and Public Admin, Education and Health (University education) categories, which have the lowest density of WFTC-eligible workers. See Table 1a for % of WFTC-eligible workers by industry category.

FIGURE 4. Kernel density of WFTC effect on wage for women (Industry comparison).



NOTES: "Eligible" are workers eligible for WFTC. "Nonigiblee ("Similar")" in Figure 5a are workers not eligible for WFTC in the High School_16 education category, which has the highest density of WFTC-eligible workers. "Noneligible ("Nonsimilar")" in Figure 5b are workers not eligible for WFTC in the University education category, which has the lowest density of WFTC-eligible workers. See Table 1b for % of WFTC-eligible workers by education category.

FIGURE 5. Kernel density of WFTC effect on wage for women. (Education comparison).

results for all men for the OLS and the CLAD estimation, respectively. Columns (3) to (6) show the results separately for single and married men for each estimation strategy, respectively. As an alternative specification, we report the main results using a WFTC eligibility variable instead of the tax credit amount. The results, shown in Table A.3, are similar to those shown for men and women in Tables 5 and 6. Although this is a more straightforward approach to computing the tax credit amounts (i.e., closer to the more standard approach of using eligible or not), the coefficient on eligibility may also capture

the intensive margin (generosity). Using the continuous measure allows us to include a measure that take into account the tax credit amount change from Family Credit and WFTC.

From Columns (1) and (2) of Table 5, three striking results emerge from the analysis of men. The first is that a WFTC-eligible worker has a decline in his gross wage relative to a similarly skilled noneligible worker who has the same predicted wage. The coefficients are larger in the censored specification than in the linear specification (0.278 versus 0.143). From column (2), we see that as the rate of WFTC increases for the eligible worker, the gross wage falls relative to that of a similarly skilled noneligible worker. Calculated at the average WFTC rate, which is 0.24, this is equivalent to a decrease in gross wages of approximately 7 percent. When we evaluate this at the average weekly wage of £216 and the average weekly WFTC of £45, this implies that there is a 30 percent shift in incidence from the eligible worker to the employer.

The second important result is that there is a strong and negative spillover effect on noneligible workers when we look by industry and by education group. The results from both specifications are similar and imply that for every pound of the tax credit, there is a negative spillover of WFTC on the wages of noneligible workers in a given education group. From column (2), the coefficients of 0.29 implies that, when evaluated at the average fraction of WFTC workers in an education group, wages of noneligible workers decrease by 1.7 percent while the fraction of men eligible for WFTC increases by one percent in an education group. Similarly, when looking at spillovers within given industry groups, we find that wages fall for all workers. Evaluated at the average fraction of WFTC workers in an industry, wages for all similar workers decrease by 0.9 percent. This is of critical policy importance, as we can no longer assume that a partial analysis of the tax credit is sufficient when trying to understand all the consequences of the policy. Studies of the effect of taxes and benefits on other groups often ignore this group. Moreover, because the spillover affects the wages of eligible workers as well, this is equivalent to an 8 percent shift in tax credit incidence to the employer, over and above the shift from the direct effect.

Finally, a change in tax credit generosity from Family Credit to WFTC does not have a significant effect. This is the case for both specifications (columns (1) and (2)).[21] This implies that the effect on gross wages is a result of the change in payment method (i.e., payment through the employer). This is important to our understanding of the mechanism through which the incidence of the tax credit can be shared. The result implies that even in the absence of an increase in the tax credit, increased salience of the tax credit to the employer can lead to a shift in the incidence of the tax credit. When looking separately at single and married men in columns (3) to (6) in Table 5, we find similar patterns across the groups and specifications. The results, however, become less precise when we look at single men, which likely reflects the small number of single fathers claiming WFTC. The distinction by marital status is more relevant when looking at women because the proportion of single mothers is relatively larger than that of single fathers.

[21]The effect, however, does not take into account potential changes in the generosity from childcare expenses.

As noted earlier, it is traditional to focus on women when looking at the participation effects of tax credits. However, when looking at wage changes, the reasoning for this is less obvious because men are at least as, if not more, likely to claim the tax credit through their employer. In particular, in a coupled household, because the male partner is more likely to be the household member in the labor force, he is also more likely to be the claimant. The results in Table 6, which present the results for women, offer interesting insights and confirm this hypothesis.

Table 6 presents the results for women from the OLS and CLAD estimation. From columns (1) and (2), we see that the direct effect is small and positive when using the linear specification but is close to zero (and insignificant) when we take into account the left-censoring that is generated by the minimum wage (column 2). Moreover, from columns (3) to (6), when separating by marital status, the results suggest that the direct effect of WFTC is negative only for single mothers and positive for married women. However, these results are not very stable across the different specifications. One explanation for why the direct effect is stronger for men than for women may be that women have a lower average wage than men and work, on average, fewer hours, so the potential incidences from WFTC are smaller. Alternatively, it may be that women are more likely to be "protected" from a wage (growth) cut because of the minimum-wage barrier.

We do, however, find that for women there is a strong and negative spillover effect by both industry and education groups. This result implies that as the fraction of eligible women increases in the workplace, there is a larger wage drop for everyone in the same skill group (i.e., those with the same predicted wage). This is consistent with the idea that the shift in the burden of the tax credit increases with the fraction of eligible workers. When comparing the results from the different estimation techniques, the results remain consistent, but the order of magnitude of the coefficients changes. From column (2), the education (industry) spillover coefficient of 3 (9) percent implies that, when evaluated at the average fraction of WFTC workers in an education (industry) group, wages of non-eligible workers fall by 0.4 percent (1.0 percent) while the fraction of women eligible for WFTC increases by 1 percent in an education (industry) group.

We check how sensitive the results are to alternative measures of hours worked, to removing the proxy respondents, to removing those who report a wage below or at the national minimum after its introduction, to restrict the analysis to those for whom the change in generosity from Family Credit to WFTC was small, and to restricting the counterfactual group to those with children of school age. These results are summarized in Table A.4 and described below.

Although eligibility is based on discrete hours cut-offs, as a robustness check, we use actual hours worked (a continuous measure). In Table A.4, columns (1) and (2), we show that if we predict wages using hours worked, the results for are similar for men and women: the (negative) direct effect is stronger but still much smaller in magnitude than for men and is only marginally significant. Second, we might be concerned that proxy respondents for eligible workers are more (or less) likely to misreport wages than for similar noneligible workers. We exclude proxy respondents from the analysis, and

from columns (3) and (4) of Table A.4, we find that the main results hold. For both men and women, the coefficients remain mostly unchanged, suggesting that the results are not driven by differentials in the response by proxy respondents. To address another potential issue of misreporting, we investigate the effect of reporting below the national minimum wage after its introduction on the main results. After the introduction of the national minimum wage, 1 percent of men and 3 percent of women in the sample report a wage below the national minimum. From columns (5) and (6), we see that when we exclude these workers, as well as those who report a wage at the national minimum, from the sample, the results do not change with respect to the baseline results. In columns (7) and (8), we report the results for those who were previously eligible for Family Credit, which was paid directly to them, and are now eligible for WFTC, but for whom the change in the amount they receive is small (i.e., less than £10 extra per week). We find that there is still a negative effect on wages, further suggesting that the method of payment is important. Finally, we restrict the analysis to workers with children under the age of 16. By construction, those receiving WFTC have children, however, among the control group, some may not have children or have children over the age of 16. From columns (9) and (10), we see that our results are robust within this subsample of workers.

7. Extensions and robustness

In this section, we conduct some robustness checks and broaden our analysis to investigate some interesting questions. First, we conduct a falsification test using our two-stage estimation on prepolicy years. Second, we consider whether the size of the firm has any impact on the share in incidence between workers and firms.

7.1 *Test of the common trends assumption*

The identification strategy we use relies on a common trend assumption. The β_1 in equation (3) is flexible, such that it adjusts to pick-up parallel changes in common trends. However, here we provide further evidence through a falsification test that the common trend assumption holds in a sample period before WFTC. To ensure that the observed differential effects on wages between eligible and ineligible workers did not exist prior to the introduction of WFTC, we replicate our analysis by performing placebo estimation on data before 1997.[22] Policies to help families with children have been used in the UK since the 1970s. In particular, given that WFTC's predecessor was similar to WFTC, we can only measure relative changes from the previous policies and not the absolute effect of tax credit policies.

Using data from 1994 to 1995, inclusive, to perform the first stage of our analysis and data from 1996 to 1997, inclusive, for the second stage, we report our results in Table 7. It can be seen that during this period there was no differential effect. Additionally, to account for potential local labor market differences and the geographic variability in

[22]This is the period prior to the changes by the Labour Government that was elected in May 1997.

TABLE 7. Falsification tests—Placebo treatment before WFTC introduced.

	Ln (Wages)			
	Men		Women	
	1996	1996–1997	1996	1996–1997
	[1]	[2]	[3]	[4]
Predicted wage	1.014***	1.013***	1.042***	1.044***
	[0.0116]	[0.00860]	[0.0102]	[0.00678]
WFTC	−0.171*	−0.0654	−0.00288	0.00593
	[0.0933]	[0.120]	[0.0191]	[0.0138]
FC generosity	0.367	−0.0549	−0.00650	−0.0294
	[0.236]	[0.262]	[0.0966]	[0.0657]
Spillover (industry)	0.0188	0.0190	−0.0145**	−0.0199***
	[0.0300]	[0.0208]	[0.00648]	[0.00427]
Spillover (education)	0.0236	−0.0227	0.0402	0.0657***
	[0.145]	[0.106]	[0.0294]	[0.0208]
Constant	−0.0275	0.00971	−0.0590***	−0.0423***
	[0.0298]	[0.0224]	[0.0216]	[0.0156]
Time dummies	Yes	Yes	Yes	Yes
Observations	11,719	24,742	11,893	25,238

Note: Data from UK Quarterly Labour Force Survey (1994–1997). The coefficients and standard errors (in parenthesis) are reported. *** represents significance at the 1% level, ** represents significance at the 5% level, and * represents significance at the 10% level. Standard errors are bootstrapped with 200 replications. We include a full set of time dummies (each quarter of each year) in all the regressions. For definitions of variables, see Table 5.

productivity over time, we interact region with industry when constructing the counterfactual wage. The results are presented in columns (1) and (2) Table A.5 for men and women, respectively. Similarly in columns (3) and (4) of Table A.5, we interact industry with trends when constructing the counterfactual wage. This should allow for a differential growth by industry and also for differential industry inflation rates. While the main result holds, the (industry) spillover effect increases slightly.

7.2 *Policy anticipation effect*

The introduction of the Working Families' Tax Credit was announced in March 1998 before its actual introduction in October 1999. Although not all the features were publicized, the government announced that this tax credit would replace Family Credit. Francesconi and van der Klaauw (2007), and in a more recent paper, Blundell, Francesconi, and Van der Klaauw (2011), argued that the anticipation effects of WFTC are important in the labor market and other behavioral responses. As a robustness check, we adjust the analysis to take into account that employers and workers may have adjusted their behaviors before the actual introduction of WFTC. We do this by excluding from the analysis the period between the announcement and the implementation. From Table 8, it can be seen that the main findings remain largely unchanged.

TABLE 8. Exclude WFTC announcement and implementation periods.

	Ln (Wages)	
	Men	Women
	[1]	[2]
Predicted wage	0.983***	1.012***
	[0.00565]	[0.00411]
WFTC	−0.288***	−0.00859
	[0.0479]	[0.00936]
FC Generosity	0.181*	0.00853
	[0.106]	[0.0312]
Spillover (industry)	−0.173***	−0.101***
	[0.0544]	[0.0152]
Spillover (education)	−0.371***	−0.0433***
	[0.0598]	[0.0142]
Constant	0.336***	0.231***
	[0.0156]	[0.0107]
Time dummies	Yes	Yes
Observations	76,145	80,421

Note: The coefficients and standard errors (in parenthesis) are reported. *** represents significance at the 1% level, ** represents significance at the 5% level, and * represents significance at the 10% level. Standard errors are bootstrapped with 200 replications. We include a full set of time dummies (each quarter of each year) in all the regressions. For definitions of variables, see Table 5.

7.3 *Firm size*

There is a large body of literature relating the size of the firm to the wage level in that firm. Brown and Medoff (1989) concluded that one of the main reasons wages are higher in larger firms is that they hire better-quality workers. In a competitive model, we would expect the hourly wages to be the same for similar workers. In it quite likely that in larger firms, there will be multiple workers with the same job-title. By applying the methodology used in Section 5, we compare: (1) small-sized firms (employing 1–19 workers); (2) medium-sized firms (20–49 workers); and (3) large-sized firms (more than 50 workers).

We replicate our earlier analysis for each firm-size category and report the results in Table 9. We find that as the size of the firm increases, the degree of "spillover" by industry also increases. This seems quite reasonable, given that larger firms have more uniformity in wage contracts across workers (i.e., workers performing identical jobs and receiving the same hourly wage). Here, it would be more difficult for the employer to cut the gross wage of only those eligible for WFTC and to leave the wage of the noneligible workers unchanged.

8. CONCLUSION

The results presented in this paper show that when tax credits in the UK were paid directly through the employer rather than as a welfare benefit, there was a significant shift in the burden of tax credits. Firms adjusted wages downwards after observing directly the tax credits paid to workers. In equilibrium, the average wage of similar workers that

TABLE 9. Effect of WFTC by firm size.

	Ln (Wages)					
	MEN			WOMEN		
	Small	Medium	Large	Small	Medium	Large
	[1]	[2]	[3]	[4]	[5]	[6]
Predicted wage	0.870***	1.001***	1.050***	0.980***	1.030***	1.023***
	[0.0118]	[0.0122]	[0.00747]	[0.00828]	[0.00864]	[0.00580]
WFTC	−0.191***	−0.358***	−0.181	−0.0152	0.00896	0.0260
	[0.0595]	[0.114]	[0.112]	[0.0138]	[0.0202]	[0.0172]
FC generosity	−0.141	0.426*	0.146	0.0583	−0.0159	−0.0378
	[0.159]	[0.256]	[0.191]	[0.0483]	[0.0659]	[0.0532]
Spillover (industry)	−0.406***	0.136	0.387***	0.109***	0.0361	−0.215***
	[0.0901]	[0.103]	[0.0817]	[0.0294]	[0.0355]	[0.0206]
Spillover (education)	−0.519***	−0.0843	0.136*	−0.0486*	0.0841***	−0.0282
	[0.128]	[0.130]	[0.0759]	[0.0276]	[0.0313]	[0.0196]
Constant	0.487***	0.163***	0.180***	0.192***	0.0697***	0.256***
	[0.0322]	[0.0345]	[0.0207]	[0.0206]	[0.0234]	[0.0151]
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,064	13,256	45,866	23,115	16,615	40,739

Note: The coefficients and standard errors (in parenthesis) are reported. *** represents significance at the 1% level, ** represents significance at the 5% level, and * represents significance at the 10% level. Standard errors are bootstrapped with 200 replications. *Small* are firms with fewer than 25 workers. *Medium* are firms with 25 to 49 workers. *Large* are firms with more than 50 workers. We include a full set of time dummies (each quarter of each year) in all the regressions. For definitions of variables, see Table 5.

did not receive WFTC also adjusted downwards. The change in the visibility to the employer, and not the change in the amount of additional tax credit, explains this shift, and suggests that the individual wage bargaining between the firm and each given employee plays a role in tax incidence. This is the first paper to investigate the effects of the change in salience from the viewpoint of the employer (donor) rather than from that of the employee (recipient).

Another key finding in the paper is that the introduction of WFTC implied a (negative) spillover onto the wages of similarly skilled noneligible workers. This is of critical policy importance, as we can no longer assume that a partial analysis of the tax credit is sufficient when trying to understand all the consequences of the policy. Studies of the effect of taxes and benefits on other groups often ignore this group.

The UK government spent a great deal on this policy relative to previous policies, with the aim to "make-work-pay." By paying recipients through their employers rather than directly, they intended to reduce the stigma attached to receiving tax credits in the form of a welfare benefit. The literature has shown that WFTC increased the labor supply among single mothers by as much as 5 percent, with no labor supply effect on other groups, only an increased household income. While the success of the policy can be measured in these terms, one needs to measure its benefits in their entirety by incorporating the costs associated with the wage changes. This is important not only for tax

credits such as the WFTC but also for other taxes and benefits that are visible and/or can be extracted by another group in the economy.

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