Anatomy of Policy Reform Evaluation: Announcement and Implementation Effects

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Abstract

This paper formulates a simple model of female labor force decisions which embeds an in-work benefit reform and explicitly allows for announcement and anticipation effects. We stress two mechanisms through which women can respond to the announcement of a reform that increases in-work benefits, namely, intertemporal substitution and labor market frictions. Simulations show that those two mechanisms lead to opposite changes in female labor force participation before the implementation of the reform. If labor market frictions dominate, we expect a positive pre-reform effect on labor supply; while if intertemporal substitutability dominates, then we expect to observe a reduction in labor market participation between the announcement and the implementation of the reform. We use this result to analyze the effects of a major UK in-work benefit reform, the Working Families’ Tax Credit (WFTC), on single mothers’ behavior. We find that there are large and positive announcement effects on eligible employment decisions (that is, working 16 or more hours per week), which suggests that women’s responses are consistent with a story based on labor market frictions rather than intertemporal substitution. Treatment effect evaluations based on pre-post reform outcome comparisons that ignore such announcement effects produce estimates that are biased downwards by 15 to 35 percent. These results emerge also in the case of other labor supply margins, such as full time employment, labor market participation, hours worked, and labor market transitions.

Keywords: Difference in differences; In-work benefit; Anticipation effects; Labor market frictions; Intertemporal substitution

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Expectations are central to human life and economic analysis. Economists have long developed models in which individuals and firms are postulated to be forward looking and to respond to changes in the environment in which they make their decisions even before such changes actually occur. But while there is extensive work documenting how economic agents adjust their behavior in anticipation of shocks that are expected to affect their environment, there is relatively little work on the corresponding responses of individuals to welfare reforms. The goal of this paper is to analyze precisely such responses in the specific case of female labor supply. We formulate a simple model of female labor force decisions which embeds a basic in-work benefit reform and explicitly allows for announcement and anticipation effects. We stress two mechanisms through which women can respond to the announcement of the reform, namely, intertemporal substitution and labor market frictions.

Our analysis shows that those two mechanisms lead to opposite changes in female labor force participation before the implementation of the in-work benefit reform, a reform that implies a permanent increase in women’s earnings provided that they work. On the one hand, in a frictionless world, labor supply declines between the announcement and the implementation of the reform if behavior is dominated by intertemporal substitution effects. This is because women, who are forward looking and anticipate the introduction of the reform, prefer to withdraw from the labor market today and get a job with certainty (since there are no frictions) tomorrow when they can reap the monetary benefits offered by the reform. On the other hand, in a world with labor market frictions and no intertemporal substitution effects, women’s response to the announcement of the reform is to increase their labor supply today in anticipation of the greater benefits they can obtain from working when the reform is implemented tomorrow. Because there are frictions in the labor market, nonemployed women do not have a guarantee to get a job whenever they wish. Therefore, they enter (or decide to remain in) the labor market after the announcement, to be in a position to collect the in-work benefit when the reform is introduced.

We apply this analysis to study the effects of the Working Families’ Tax Credit (WFTC) reform on single mothers’ behavior. The WFTC reform was initially presented in the UK Parliament in November 1997, officially announced in the Budget speech in March 1998, and finally implemented eighteen months later in October 1999, offering ample room for anticipatory behavior. We find that the anticipated changes in single mothers’ labor supply
are consistent with the existence of adjustment frictions that dominate any intertemporal substitution effects. In other words, the estimated WFTC announcement effect on labor supply, measured between the time WFTC was announced and its actual implementation, is always positive. This result emerges for a number of labor market outcomes, and it is especially strong for mothers of children of preschool and primary school age for whom the tax credit increase was in fact particularly large. There are also strong announcement effects along the informal (unpaid) childcare utilization margin, and not along the paid childcare margin, for which instead we find sizeable implementation effects. These latter effects can be explained by the fact that, at the time of the WFTC reform, formal childcare services were relatively expensive and the pre-WFTC in-work support was not particularly generous towards childcare expenditures. Therefore, single mothers who wanted to take advantage of the benefits offered by the WFTC reform decided to enter the labor market as soon as they could and find temporary arrangements for their children before placing them in formal daycare centers (or other formal childcare arrangements) after the introduction of WFTC, when they would have been entitled to receive also a substantial childcare tax credit top-up.

The paper continues with a discussion of the relevant strands of literature about the impact of news announcements on economic behavior and about the analysis of public policy reforms. In Section II the labor supply model is formulated and simulated, and its key econometric implications are discussed. Section III describes the Working Families’ Tax Credit reform that allows us to illustrate the extent of anticipatory behavior in labor market decisions of British single mothers. It also describes the data used in estimation and the identification strategy for recovering the announcement and treatment effect parameters. Section IV presents the empirical results on labor market outcomes and childcare utilization, examines transitions along such margins, and discusses a number of sensitivity checks. Section V concludes.

I. Related Literature

The connection between news announcements or expectations of future events and individual responses by forward looking agents has a long history in economics. Recent examples include a wide range of economic behaviors, from the role of news and expectations as
drivers of business cycles and stock prices (Beaudry and Portier, 2006; Jaimovich and Rebelo, 2009) to the response of foreign exchange rate quotations to macroeconomic announcements (Andersen et al. 2003; Evans and Lyons, 2003), and from the impact of announced changes in corporate income taxes on firms’ dividend and investment policies (Kari, Karikallio, and Pirttilä 2008) to the effect of tax rebate announcements on consumer spending (Heim, 2007).

Relatively fewer studies instead analyze anticipation and announcement effects associated with welfare programs. Moffitt (1987) argues that the social security reforms that took place during the late 1960s and early 1970s were fully anticipated, and that their anticipation is likely to explain the absence of changes in post-reform labor supply behavior. Despite this, we still know little about the way in which forward-looking individuals make their labor supply decisions in anticipation of policy reforms.

Others have pointed to the role of expectations in explaining the increase in the exit rate out of unemployment in anticipation of unemployment benefit exhaustion (e.g., Moffitt, 1985; Meyer, 1990; Card and Hyslop, 2005), while Grogger and Michalopoulos (2003) find significant effects of time limits on welfare receipt on welfare participation, consistent with forward-looking behavior. More recently, Card, Chetty, and Weber (2007a) confirm the unemployment benefit results using administrative data from Austria. They argue, however, that time to next job, rather than the number of days an individual is registered as unemployed, is a better measure of search duration, because it is not affected by program parameters. Using this alternative measure, their empirical analysis shows that the hazard

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1A number of other behaviors have also been examined. For instance, Slemrod (1995), Auerbach and Siegel (2000), and Scholes, Wilson, and Wolfson (2000) provide evidence of shifting of taxable income by firms and high income individuals to future periods in anticipation of the 1986 Tax Act, which reduced corporate and individual tax rates. Goolsbee (2000) also documents an increased exercise of stock options by high income executives in anticipation of an increase in marginal income tax rates, while Pencavel (2001) emphasizes the importance of expectations about future reforms for understanding the impacts of a series of ‘one-time’ early retirement schemes offered to faculty at the University of California.

2A number of papers examine the extent to which workers respond to such reforms by intertemporally substituting work with leisure (e.g., Burtless and Greenberg, 1983; Ashenfelter, 1984). They typically find small and short-term intertemporal responses. Besides the fact that the intertemporal elasticity of substitution between leisure and work could be genuinely small, this result might also be driven by the transitory nature of the experiments under study (i.e., their lack of salience) or by the presence of labor market frictions in individuals’ search behavior. Much empirical work has long documented very small intertemporal elasticities in labor supply models (MaCurdy, 1981; Altonji, 1986; French, 2004), and Ham and Reilly (2002) report evidence that does not support intertemporal substitutability in the labor market.

3A study by the Council of Economic Advisers (1997) examines the effect of waiver activity in the early 1990s on welfare caseload, and finds that waivers made a substantial contribution to the reduction in caseload. Thus, knowledge that welfare policies were to become stricter deterred women from welfare participation even before waivers were implemented. See also Moffitt (1999).
of re-employment rises only slightly at the point of benefit exhaustion, implying that job seekers do not wait to return to work until their unemployment insurance benefits are exhausted. This is consistent with forward-looking behavior, whereby a large fraction of the unemployed leave the unemployment registry only once their benefits end and they are no longer required to maintain their eligibility for benefits. Such results however remain silent on whether this and the implied job seeking behavior are driven by intertemporal motives or other preference considerations rather than by liquidity constraints or labor market imperfections. Moreover, the same results do not directly inform us on how anticipation effects might shape individual behavior, because they do not rely on pre-announced welfare-to-work reforms or other policy changes in the environment in which the unemployed make their decisions.\(^4\)

Somewhat closer to our work is the study by Chetty et al. (2009), which tests the hypothesis that labor supply elasticities estimated from small quasi-experiments using microdata are attenuated by adjustment costs and institutional constraints on hours worked. Their estimation strategy takes advantage of bunching at kink points generated in the tax schedule by large tax changes and of changes in earnings around tax reforms. The results, based on administrative panel data from Denmark, support the notion that failing to account for search frictions can lead to considerably smaller estimates of the impact of taxes on labor supply. In our work, we too explore the possibility of attenuation by frictions in a setting other than that of income tax changes, namely, in the context of in-work benefit reform. In addition, we explore the competing role played by the intertemporal substitutability of leisure and work, while accounting for the effect of reform announcements on individuals’ anticipatory behavior.

Limited discussions of announcement and anticipation effects can be found in few other studies. For instance, Black et al. (2003) examine the impact on the exit rate out of unemployment of a threat of mandatory training and re-employment services required for continued unemployment benefit receipt. The threat in their context could be seen as a news announcement in our context, changing the information set of unemployment insurance.

\(^4\)Similar points can be extended to the analysis presented in Card, Chetty, and Weber (2007b), which looks at the effects of lump-sum severance payment and extended unemployment insurance benefits on labor market outcomes of job losers in Austria. This work, however, not only finds evidence of forward-looking behavior but also emphasizes the importance of credit constraints on job searchers, whereby agents — differently from what a simple permanent income hypothesis model with unrestricted borrowing and lending postulates — have limited capacity to smooth income fluctuations.
ance claimants. They find substantial reductions in both duration and level of benefits received and a large increase in subsequent earnings. The earnings gain appears to result primarily from earlier return to work of individuals in the treatment group soon after receiving notice of the mandatory training and re-employment services. This result is consistent with the presence of strong announcement effects, whereby job-ready claimants respond to the threat of the program and exit unemployment quickly. In their evaluation of Progresa — a major social program in Mexico that provides generous conditional cash transfers to parents of children who attend school and live in treatment villages — Attanasio, Meghir, and Santiago (2009), instead, find no evidence of anticipation effects. Two mechanisms have been proposed to explain this negative result. First, although the program was later extended to control villages, parents in the control group might have been unable to take advantage of future availability in the (likely) event they were liquidity constrained. Second, there was no explicit, public announcement about the future availability of the grants.

Recent work on dynamic treatment effect models highlights the importance of a no-anticipation assumption for identifying treatment effects (Abbring and van den Berg, 2003). The research summarized in Abbring and Heckman (2007) explicitly discusses sequential randomization and no-anticipation assumptions, which require potential outcomes to be unaffected by agent actions in response to different predictions of future treatments and outcomes. An important implication of this reduced form approach is that valid inference requires an ability to condition on agent information sets, including the likelihood of (as well as eligibility to) future policy reforms. With forward-looking behavior, the assumed pre-reform comparability of treatment and control groups underlying many widely used evaluation procedures requires both groups to have common expectations about future policy changes as well as common ability and motivation to act upon such knowledge. In specifying potential outcomes, then, one should not only consider the effects of actual program participation, but also the effects of the information available to agents about the program and policy.

Identification of reduced form models however typically rests on the unappealing assumption of myopic expectations (whereby a reform is entirely unexpected) or perfect foresight (whereby a reform is fully anticipated). An attractive feature of structural mod-

\[ \text{References} \]

\[ \text{Note:} \]

\[ ^5 \text{See also Heckman and Vytlacil (2007).} \]
els with forward looking behavior instead is that they require an explicit specification of agents’ information sets, including individual beliefs about the likelihood of a future policy reform. For example, Heckman and Navarro (2007) formulate a structural stopping time model in which individuals decide the age at which they stop schooling and can learn about observable and unobservable variables that affect expectations of future outcomes as well as revise their decisions sequentially. Their model is suitable for the analysis of outcomes associated with different times to treatment without imposing the no-anticipation condition invoked by Abbring and van den Berg (2003). Heckman and Navarro (2007) show that identification of anticipatory and treatment effects in their context requires neither parametric functional form assumptions nor arbitrary exclusion restrictions, but relies on curvature restrictions across index functions generating durations that can be motivated by economic theory.

Another example is the study by van der Klaauw and Wolpin (2008), which structurally estimates the impact of social security reforms on savings and labor supply decisions. In the spirit of the argument put forward by Manski (2004) on elicited expectations, they identify the perceived risk of a reform using data on subjective expectations of future social security benefits that individuals expect to receive. Similarly, Keane and Wolpin (2002) develop a model in which individuals form expectations about future welfare program changes and specify a stochastic process for variation in benefit rule parameters. Simulations indicate that the effect of changes in welfare benefits on behavior depends critically on how individuals form expectations about future welfare benefits and whether these are perceived to be permanent or transitory.

It is worth noting that, crucially, none of the available structural models fully embeds the notion of policy change announcements. This is important because news announcements may directly affect agent expectations and information sets and thus facilitate identification of anticipatory behavior. As the model in the next section illustrates, the interplay and timing of policy announcements, individuals’ expectations, and reform implementation imply natural cross-equation restrictions that, together with the choice theoretic structure

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6See also Dominitz, Manski, and Heinz (2003) for evidence showing strong consumers’ expectations of a future decline in the generosity of the social security benefit program. The notion of elicited expectations is an alternative to another concept developed in Manski (1993) based on a synthetic cohort assumption, according to which choices and outcomes of one group can be observed and acted upon by a younger group. Heckman and Navarro (2007) critically discuss this assumption, and its reliance on stationary environments and on the exclusion of temporal dependence in unobservables.
of the model, are decisive for identification.

II. A Model of Female Labor Supply with Welfare Reform and Anticipation Effects

A. Setup

We illustrate our key insights regarding the complications of public policy reform evaluations using a simple model of female labor supply.

Consider a three-period economy in which each woman \( i \) chooses whether to work \((y_{it} = 1)\) or not \((y_{it} = 0)\).\(^7\) At any period \( t = 1, 2, 3 \), each woman’s objective is to maximize the expected present value of her remaining lifetime utility

\[
E \left[ \sum_{s=t}^{3} \delta^{s-t} U_{is}(c_{is}, y_{is}, X_{is-1}) \mid \Omega_{is} \right],
\]

with respect to \( y_{it} \). The other variables are defined as follows: \( c_{it} \) is the level of goods consumption for woman \( i \) at time \( t \); \( X_{it-1} \) denotes the number of periods woman \( i \) has worked prior to period \( t \) (and, without loss of generality, \( X_{i0} \) is set equal to zero); \( \delta \) is the subjective discount factor; \( E[\cdot] \) is the mathematical expectation operator and \( \Omega_{it} \) is the individual’s information set at time \( t \). The latter includes information the individual has regarding the possible implementation of a future policy reform, to be discussed below.

Work experience evolves according to

\[
X_{it} = X_{it-1} + y_{it}. \tag{2}
\]

Without saving or borrowing, women’s choices are resource constrained each period by

\[
c_{it} = w_{it}y_{it} + N_{it}, \tag{3}
\]

where \( w_{it} \) represents woman \( i \)’s potential earnings, and \( N_{it} \) is the woman’s exogenous non-labor income. Potential earnings are stochastic and depend on previous work experience. In particular,

\[
w_{it} = w_0 + \alpha X_{it-1} + \beta d_t I(t \geq 2)y_{it} + \epsilon_{it}, \tag{4}
\]

\(^7\)The choice of three periods is by no means restrictive. The model could be easily extended to more periods, but without adding further intuition. That is, its salient features can be fully fleshed out in this three-period formulation.
where the parameter $\alpha$ measures the return to work experience, $I(z)$ is an indicator function that is equal to one if $z$ occurs and zero otherwise, and $\epsilon_{it}$ is a technology shock which captures random fluctuations in earnings that are independent of the individual decision process. We assume that $\epsilon_{it}$ has an identical and independent over time logistic distribution.

The term $d_t$ is an indicator of the implementation of a one-time welfare reform that could occur either in period 2 or 3. That is, in periods 2 and 3, $d_t = 1$ if the reform is or already has been implemented and $d_t = 0$ if the reform has not been implemented. Based on acquired information $\Omega_{it}$ in a given period $t$, individuals form beliefs about the likelihood that the reform will be in place in future periods. We denote the beliefs in period 1 about a reform in period 2 by $\pi_{12} = \Pr(d_2 = 1|\Omega_1)$. Beliefs in period $t = 1, 2$ about a reform in period 3 are denoted by $\pi_{13}(d_2) = \Pr(d_3 = 1|\Omega_t, d_2)$, where $\pi_{13}(1) = 1$.

The parameter $\beta$ in (4) encapsulates the benefit of the reform. The reform gives each woman a permanent shift in earnings, $\beta$, provided that the woman works ($y_{it} = 1$). For simplicity, the earnings shift is independent of prior work experience.

Per period utility is linear and additive in consumption and takes the following specification:

$$U_{it} = c_{it} + \gamma_1 y_{it} + \gamma_2 X_{it-1} y_{it} + \gamma_3 c_{it} y_{it}. \quad (5)$$

In (5), $U_{it}$ is decreasing in $y_{it}$ (i.e., $\gamma_1 < 0$) reflecting disutility of work, and increasing in consumption, $c_{it}$. Letting the labor market decisions interact with prior experience implies that the utility function is not intertemporally separable, as long as $\gamma_2 \neq 0$: a positive value of $\gamma_2$ may be interpreted as habit formation in the labor market, whereas a negative value may reflect an increasing current disutility of work with previous work effort or increasing propensity to substitute nonmarket time in subsequent periods. Finally, the value of good consumption may be increased ($\gamma_3 > 0$) or decreased ($\gamma_3 < 0$) when the woman participates in the labor market.

Women take decisions in a labor market environment that may include frictions. Labor market imperfections are reflected in the choice set available to women. Specifically, $y_{it} \in J_i$, where $J_i$ denotes the work decision choice set available to woman $i$ in period $t$, and this is equal to $\{0\}$ (that is, no job is available) with probability $(1 - \lambda_i)$ and to $\{0, 1\}$ (that is, the choice set includes both ‘not working’ and ‘working’) with probability $\lambda_i$. 
We assume that there is no current labor market friction for a woman who worked in the previous period, that is, \( \lambda_t(y_{it-1}) = 1 \) if \( y_{it-1} = 1 \), while the arrival rate if currently not working \( \lambda_t(0) \) may be less than one.

B. Simulations

As an illustration of the possible effects of welfare reform on labor supply, we solve the model of the previous section and use its solution to simulate choice decisions of 1 million women under a number of different alternative specifications.\(^8\) In all cases, the following parameter values are used: \( \delta = 0.9, w_0 = 1, \alpha = 0, \beta = 1, \gamma_1 = \gamma_3 = 0 \). In the baseline case there are no labor market search frictions and utility is time separable, which correspond to setting \( \gamma_2 = 0 \) and \( \lambda(0) = 1 \). To capture the role of labor market frictions and intertemporal substitution, we also consider a case where the job offer arrival rate \( \lambda(0) = 0.5 \), as well as a case in which the disutility of working is dependent of past work decisions \( (\gamma_2 = -1.5) \), implying that utility is not time separable.

For simplicity we will assume that in period 1 individuals assign an equal probability to the implementation of a reform in periods 2 and 3, such that \( \pi_{12} \equiv \Pr(d_2 = 1|\Omega_1) = \pi_{13}(0) \equiv \Pr(d_3 = 1|d_2 = 0, \Omega_1) = \pi_1 \). Beliefs in period 2 about the likelihood of a reform in the last period is denoted by \( \pi_{23}(0) = \Pr(d_3 = 1|d_2 = 0, \Omega_2) = \pi_2 \). In the baseline scenario in which no additional information is received about the likelihood of reform in the final period, \( \pi_1 = \pi_2 \). However, an alternative scenario we will consider is one in which there is an unanticipated announcement in period 2, which will be part of the individual’s information set at \( t = 2 \), that may increase individuals’ beliefs about the likelihood of a reform at \( t = 3 \), such that \( \pi_2 > \pi_1 \).

For each exercise, we then consider five different scenarios, distinguished by the nature of the welfare reform. First, the baseline scenario, which imposes \( \pi_1 = \pi_2 = 0 \), is the case in which there is no reform, nor is there a possibility of a reform envisaged by women. In scenarios 1 to 3, a reform is actually implemented in period 3. But the extent to which the reform is anticipated and/or announced differs across the different scenarios. In scenario 1, we assume \( \pi_1 = \pi_2 = 0 \): thus, there is no pre-implementation announcement and the reform is completely unanticipated. In scenario 2, with \( \pi_1 = 0 \) and \( \pi_2 = 1 \), individuals rule out the possibility of a future reform in period 1, but a reform that will take place in period

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\(^8\)The model solution is presented in Appendix A.
3 is announced in period 2. In scenario 3, in which we assume $\pi_1 = 0.5$ and $\pi_2 = 1$, women in period 1 assign a 50 percent chance that a reform will be introduced in period 2 as well as a 50 percent chance that a reform will be implemented in period 3 if it was not already implemented in period 2, and the implementation of the reform in period 3 is announced in period 2. Finally, in alternative scenario 4, we have an announcement of a completely unanticipated next-period reform in period 2, but the reform fails to materialize.

Starting with the simulations for the no friction-and-time-separable utility case, Figure 1 (maybe Table 1?) shows that the employment rate is constant across the three periods in the scenarios without a reform, while there is a large increase in period 3 with the actual introduction of the reform. Even in case of some anticipation of a future reform or an announcement in period 2, there is no incentive to change optimal behavior in absence of labor market frictions or scope for intertemporal substitution. In this environment, therefore, individuals act the same as if they were myopic.\(^9\)

In the case with search frictions, Figure 2 shows that overall employment rates are lower, reflecting the lower job arrival rate in the first as well as in subsequent periods. Even in the baseline scenario, there are now gains from working in the first two periods (albeit not in the third) as it guarantees the option to work in the subsequent period. Both the anticipation of a possible reform as well as an announcement in period 2 lead to an increase in employment rates in the pre-implementation periods. In case of scenario 4, this pre-implementation knowledge can lead to a reduction in employment rates when such a reform does not materialize. It is important to realize that in case of any anticipation effects, both the eventual implementation of a reform and the absence of a reform can affect behavior.

When utility is not time-separable, there is scope for intertemporal substitution in employment decisions which can generate employment patterns quite different from those in the case of search frictions. By allowing the disutility of work to increase with work experience, the model can generate employment rates that are slightly declining over time in the baseline scenario, as shown in Figure 3. In period 2, the announcement of an unanticipated reform to be implemented in period 3 now causes the employment rate in period 2 to fall, in anticipation of the higher earnings and employment rate in period

\(^9\)Note that in our simulations there are no wage returns to work experience ($\alpha = 0$) and we do not allow for savings.
3. Similarly, the anticipation in period 1 of a possible future reform leads to a lower employment rate in that period.

C. Econometric Implications

As illustrated in the figures, when a reform is announced or when there is some anticipation of its possible implementation, this can affect behavior even before the reform’s actual introduction. Depending on what exact effects one is interested in evaluating, these behavioral responses would generally be considered part of the program’s overall causal effect. For example, in the case of an announcement of an entirely unanticipated reform (scenario 2), the employment rates in periods 2 and 3 could be compared to those in the baseline scenario to obtain estimates of the announcement and implementation effects of the reform, which together capture its overall impact. For the special baseline scenario in which employment rates are constant across the three periods, a simple before-after comparison of rates in periods 2 and 1 would measure the announcement effect, while a comparison of rates in periods 3 and 2 would represent the implementation effect. The overall effect would be measured by the difference in employment rates between periods 3 and 1. Clearly, in this case a simple pre- and post-implementation comparison of employment rates (contrasting period 3 with either period 2, or periods 1 and 2 combined) will generally lead to inaccurate inferences regarding the reform’s overall effect. Forward looking behavior in a world with search frictions could then lead to underestimation of the reform’s impact, while with time-non separable utility it could lead to overestimation of the true overall causal effect.

In analyses that instead use differences-in-differences methods, employment rates in the baseline scenario are usually estimated using the employment rates of a comparison group, consisting of otherwise similar individuals who are not eligible for and unaffected by the reform. In this case, comparing the period 2 versus period 1 difference in employment rates for the treatment group with the same difference for the control group will provide an estimate of the announcement effect, while similar comparisons for the period 3 versus period 2 differences and period 3 versus period 1 differences will instead estimate the implementation and total program impacts, respectively. Using either period 2 or periods 1 and 2 combined as pre-program period will again lead to estimation biases. Correct evaluation therefore depends crucially on knowledge by the evaluator of the extent to which
individuals may have anticipated or learned about the reform prior to its implementation. Were there discussions and/or formal announcements of possible reforms in the periods leading up to its actual implementation? Not only is this important for valid inference, but it is also key to understanding the overall effect of an intervention. As illustrated by our simulations, the overall size of the employment effect depends on whether the reform was announced prior to implementation. That is, the extent to which individuals can and will act in advance of a subsequent reform can affect its ultimate effect. For example, in the case of labor market frictions, advance knowledge allows more people to take advantage of the in-work benefit, by staying in or entering the labor market before the reform is implemented. Thus the effectiveness of a given reform can crucially depend on the way it is implemented: on when it was proposed, passed and implemented.

While in many cases it would be reasonable to assume that a reform was unanticipated before it was announced or implemented, or at least that \( \pi_1 = 0 \) was very small, in other cases this seems less reasonable. For example, while there may be uncertainty about when a reform is implemented, as well as about the details of reform, many individuals report that they anticipate a reform that will reduce their future social security retirement benefits (Dominitz, Manski, and Heinz, 2003). In presence of anticipation effects, such as described in scenarios 3 and 4, evaluating the impact of an intervention is more complex. First, one needs to refine the question of what effect one hopes to estimate. In a world in which a new reform occurs or is announced totally unexpectedly it seems appropriate to define the counterfactual outcome to be those that would have occurred in the same world had the reform never occurred. However, in a world in which people anticipate the possibility of a future reform, instead of considering a world in which reforms never occur, a more reasonable counterfactual would be a world in which a reform had not been announced or implemented yet. That is, the counterfactual outcome is the outcome that would have occurred without an announcement in period 2 or without a previously unannounced implementation in period 3. As is clear in scenarios 3 and 4, the non-occurrence of a reform is an event that directly affects behavior itself. The use of a simple before-after comparison of outcomes for the treatment group therefore seems even less appropriate, as the observed outcomes in periods 1 and 2 may not be suitable proxies for what outcomes would have been without the announcement and/or implementation of the reform.
Similarly, in a world in which individuals consider the possibility of future reforms, the requirements for a valid control group in differences-in-differences type evaluations become more stringent. Not only do control and treatment group members have to have comparable characteristics and backgrounds, they also need to have had comparable knowledge, beliefs and expectations about future reforms and about future decision environments more generally. In scenarios 3 and 4, the valid counterfactual for estimating the announcement effect is a world where people had similar expectations but no announcement of a reform was made. For estimating the overall effect of the announcement and subsequent implementation, the counterfactual is a world in which neither occurred. The corresponding control group should have had comparable characteristics and expectations but were not eligible or subjected to the reform or its announcement.\footnote{Note that this requires an absence of general equilibrium and spillover effects.}

III. Application: The WFTC Reform

A. Overview of the Reform and Its Announcement

Our application investigates the introduction of the Working Families’ Tax Credit, a major in-work benefit reform introduced in Britain in October 1999. We focus on single mothers, one of the explicit target groups of the reform. Our goal is to use the insights from the model of the previous section to guide our interpretation of single mothers’ labor market and child care utilization decisions in anticipation of, and response to, the introduction of the WFTC reform.

A number of previous studies have already provided comprehensive descriptions of this reform and its impact on a wide set of outcomes (see, among others, Blundell and Hoynes, 2004; Francesconi and van der Klaauw, 2007; Brewer et al., 2009). Appendix B describes the details of the policy change. In what follows, we stress three special features, namely, the economic climate within which WFTC was introduced, its formal announcement with the long time gap leading to implementation, and the economic salience of the reform.

\textit{Economic Context}

The WFTC reform was introduced on October 5th, 1999. By that year, the UK economy had recovered from the recession of the early 1990s, with the unemployment rate reaching
5 percent (about half of what it was in 1992), and GDP growth being stable at around 3 percent (as opposed to the negative growth experienced in 1991 and 1992). At the same time, the balance of payments was in good standing, and positive in 1997 and 1998, and inflation was low (less than 2 percent), with the base rate of interest being independently set by the Bank of England since 1997. Public finances were also healthy, following a period of continued decline in government net borrowing which moved into surplus in 1998/99. The pound was strong and consumer confidence high. The growth rate in household consumption expenditures more than doubled between 1995 and 1998, from less than 2 percent to over 5 percent. Part of this increased confidence could be seen in the housing market, which in 1997 experienced positive growth in house prices for the first time since 1989. When the WFTC reform was implemented in October 1999, therefore, the British economy was in a strong position, with a positive outlook for a balanced and stable growth.

**Announcement and Media Coverage**

Prior to the reform in October 1999, another work-conditioned transfer called Family Credit (FC) had been in operation since April 1988. The Pre-Budget Statement in November 1997 of the newly elected Labour government (the Labour Party won the elections in May of that same year) announced an in-work benefit reform as a crucial instrument of the government’s strategy to make work pay for low-income families. The Budget in March 1998 formally announced the new tax credit and set out the time of its official introduction (approximately 18 months later), which did not have to be further approved by other Parliamentary commissions or governmental bodies. + new TABLE 2 on the new parameters announced and actually changed

WFTC dominated the Budget speech in the Commons. It represented the key policy in the government’s strategy to ‘make work pay’ and, together with the New Deals for the unemployed, a prominent feature of the new welfare-to-work architecture. Other important benefits supporting families with young children, which were also expected to change, such as Income Support — the primary cash transfer to low-income nonworking individuals (in many respects similar to TANF in the US) — and Child Benefit, were barely mentioned.

The 1998 Budget received a phenomenal media coverage. A content analysis of just two major newspapers (The Times and The Daily Telegraph) and the BBC’s Online News Service shows at least 80 stories on the announcement of the new tax credit reform pub-
lished in 1998. About three-quarters of these came out between February and April 1998. Similarly, the government’s dissemination effort was intense, as revealed by the considerable number of press releases issued by the Treasury on March 17th, 1998 (the day of the speech) and by the emphasis of post-Budget press releases issued by the Department of Social Security, which was then responsible for the administration of Family Credit.\textsuperscript{11}

\textit{Salience}

Like its predecessor, eligibility to the WFTC tax credit was restricted to low-income parents working at least 16 hours per week. However, the new WFTC transfer program was more generous than FC in three important ways: it had higher credits, particularly those for young children; families could earn more before the benefit began to be withdrawn; and it had a lower withdrawal/taper rate. Overall, the reform increased the attractiveness of working 16 or more hours a week compared to working fewer hours. But the last of the three aspects of the reform meant that the biggest income gains were expected to be experienced by families just at the end of the FC taper (i.e., families whose earnings had reduced their entitlement to FC to zero), who tended to be working full-time (Blundell, Brewer, and Francesconi, 2008; Brewer et al. 2009).

\textbf{TO COMPLETE}:

- Table 2 @@@ + IS and HB (to add) ...

- Figure 1 (award + budget )

Therefore, both news announcements and salience of the reform tended to foster an already favorable economic climate, which in turn further encouraged work and self-sufficiency among people in low-income families and with traditionally weak labor market attachment, such as single mothers.

As a caveat, it is also important to stress that the WFTC reform was also accompanied, preceded and followed by changes in key parameters of other existing schemes, such as Income Support and Child Benefit, and by the introduction of new programs, such as the National Minimum Wage and the various New Deal schemes.\textsuperscript{12} There are, therefore, a number of possible interactions between WFTC and other policy initiatives. While


\textsuperscript{12}For a thorough description of such initiatives, see Card et al. (2004).
disentangling the effect of each individual policy is beyond the scope of this paper, in the empirical analysis we will attempt to isolate, to the extent possible, the impact of WFTC.

B. Data

We use samples from two data sources, each with advantages and disadvantages. The first is drawn from the first twelve waves of the British Household Panel Survey (BHPS) collected over the period 1991–2002. Since the Fall of 1991, the BHPS has annually interviewed a representative random stratified sample of the population of Great Britain with about 5,500 households comprising more than 10,000 individuals. The survey’s fieldwork is typically between September and December of each year. Our estimating sample includes employed unmarried non-cohabiting females (separated, divorced, widowed and never married) who are at least 16 years old and were born after 1941 (thus aged at most 60 in 2002). We exclude any female who was long-term ill or disabled, or in school full time in a given year. The sample includes 3,474 women, of whom 1,606 are lone mothers and the remaining 1,868 are childless. Although only 8 percent of the women are observed in the same marital state for all 12 years of the panel, approximately 30 percent of them are observed for at least seven years in the same state. The resulting sample size, after pooling the 12 years for both groups of women, is 15,260 observations (5,616 lone mothers and 9,644 on childless women).

The second data source is the Family Resources Survey (FRS), for the period 1995–2002. The advantage of the FRS over the BHPS is that it is a larger data set, collecting information on over 20,000 households each year. Its disadvantage, is that it is not a longitudinal survey but a repeated cross-sectional survey, so the same individuals are not followed over time. Observed changes in labor force behavior over time will therefore partly reflect changes in sample composition. Our FRS sample consists of unmarried non-cohabiting women who are between 16 and 59 years old at the time of interview, and excludes women with disabilities or in full-time education. The sample has 76,886 women, 13 Detailed information on the BHPS is presented in Lynn et al. (2006) and can be obtained at <http://www.iser.essex.ac.uk/ulsc/bhps/doc/>. 14 The FRS fieldwork dates coincide with the fiscal year, covering the period April to March of the following year. Because the WFTC reform was introduced in October 1999, that is, in the middle of the fieldwork of the 1998–1999 sweep, we re-timed each FRS data from October to September of the following year. This makes the interpretation of the estimates easier and allows for a more direct comparison to the BHPS results. Information on the FRS can be found at <http://research.dwp.gov.uk/asd/frs/>.
of whom 28,468 are single mothers and 48,418 are single childless women.

Appendix Table A1 presents summary statistics of the outcomes as well as background characteristics of the two groups of women. Although there are some small discrepancies between the BHPS and the FRS figures, the similarities across samples are quite striking. Both samples reveal some noticeable differences in characteristics between the two groups of women. Those who have children tend on average to be younger (especially in the BHPS), less educated (or more likely to have left school at age 18 in the FRS), more likely to be nonwhite, and more likely to be in social housing or less likely to be house owners. In addition, there appear to be systematic differences in employment behavior of both groups of women. Compared to unmarried childless women, single mothers are substantially less likely to be in any form of employment, whether eligible employment (working 16 hours per week or more), or full time employment (working 30 hours per week or more), or working any positive number of hours. Among those working, mothers also work fewer hours. The other outcome (paid childcare utilization) is analyzed only for single mothers.

Figure 4 plots the time trends of eligible employment over the sample period using the BHPS data, which give us a longer time span than the FRS data. The trends based on the FRS sample are qualitatively similar. Panel (a) shows the trends for single women with and without children, while panel (b) disaggregates the single mothers’ patterns into three groups stratified by the age of the youngest dependent child (ages 0-4, 5-10, and 11-18). The data reveal that single childless women had very stable labor market participation patterns over the whole sample period. The participation rates of single mothers too were stable with a small positive trend up to 1997, when they rose from about 40 to 43 percent in 1998 and further up, to nearly 48 percent, in 1999. Figure 4(b) suggests that the strongest growth was experienced by women with children in the youngest age group (0-4 years), who increased their participation rate from approximately 30 percent during the 1991-1997 period, to 45 percent in the 1999-2002 period. Interestingly, in 1998, the year preceding the introduction of the reform, the eligible employment rates of mothers of pre-school and school age children (0-4 and 5-10 years, respectively) increased quite substantially by about 5 percentage points.
C. Methods

To relate our analysis to existing evaluation studies of in-work benefit reform, let \( \ell_{it} \) denote a dummy variable that is equal to 1 if individual \( i \) is a lone mother and 0 otherwise, and let \( s \) be the time period in which the reform occurs (i.e., \( s = 1999 \)). We model the outcome variable as being determined by the following specification

\[
y_{it} = a_1 + a_2 \ell_{it} + (a_{31} + a_{32} \ell_{it})t + [a_{41} + a_{42}(t - s)]I(1999 \leq t \leq 2001) + b \ell_{it}I(1999 \leq t \leq 2001) + b_0 \ell_{it}I(t = 1998) + W'_{it} \vartheta + \mu_i + \epsilon_{it},
\]

where \( t \) varies from 1991 to 2002 for the BHPS sample and from 1995 to 2002 for the FRS sample. The term \( I(z) \) is a function indicating that the event \( z \) occurs (value 1) or not (value zero), \( W_{it} \) is a vector of individual characteristics, \( \mu_i \) represents unobserved time-invariant fixed effects (only included in the BHPS sample), and \( \epsilon_{it} \) is an i.i.d. error term, with \( E(\epsilon_{it}|W_{it}, \ell_{it}, \mu_i) = 0 \), where \( E(\cdot) \) is the mathematical expectation operator.

Equation (6) allows for different intercepts (when \( a_2 \neq 0 \)) and different pre-reform linear trends (when \( a_{32} \neq 0 \)) for control (single women without children) and treatment groups (single mothers). The parameters \( a_{41} \) and \( a_{42} \) measure possible shifts in the intercept and slope of the process generating \( y \) following the reform. In our application, they capture the effects of all other (non-WFTC) policy changes that occurred at \( s \) (e.g., the introduction of the minimum wage). While our control group of single women without children was ineligible for FC and WFTC benefits and therefore not directly affected by the in-work benefit reform, both groups were potentially affected by the other policy initiatives that took place in that year. By assuming that lone parents would have responded in the same way to these other reforms, we net out the separate impact of WFTC, which is captured in the equation by \( b \). Finally, as discussed earlier in section II.C, to avoid evaluation biases from ignoring a potential announcement effect associated with the introduction of the WFTC reform, we explicitly allow for such an effect, which is captured in (6) by \( b_0 \).
IV. Evidence

A. Eligible Employment

Table 1 shows the estimated effects of the WFTC reform on eligible employment for both the BHPS and the FRS samples. These are least squares estimates (OLS) based on linear probability models with group-specific pre-program trends and, in the case of the BHPS sample, with individual fixed effects (FE). Marginal effect estimates from probit regressions on both samples and from Chamberlain fixed-effects logit models on the BHPS sample were very similar. Column (1) reports baseline results without announcement effect, while column (2) shows both the treatment effect and the announcement effect estimates.

The results in column (1) align remarkably well with the treatment effect estimates reported in earlier studies (e.g., Brewer et al., 2006; Francesconi and Van der Klaauw, 2007; Gregg, Harkness, and Smith, 2009). The BHPS estimates are around 5 percentage points and are a little larger than those found with the FRS sample. In fact, the latter are closer to those reported in Blundell and Hoynes (2004) and Blundell et al. (2004), which were also obtained using FRS data. Accounting for an announcement effect leads to substantially larger treatment effect estimates. The OLS results in both samples show an increase by about 20 percent, raising the rate at which lone mothers worked 16 or more hours per week up to 6 and 4 percentage points in the BHPS and FRS samples, respectively.

The announcement effect itself is positive and large, about half the size of the total treatment effect in each sample (thus, representing a 3 percentage point increase in the BHPS and a 2 percentage point increase in the FRS). The effect is statistically significant in the BHPS sample (albeit only at the 10 percent level in the case of the FE regressions), but it is not in the FRS sample. Together these results provide evidence of a strong announcement effect. According to the theoretical analysis of Section II, this evidence is consistent with a story based on the presence of labor market frictions rather than with a story based on intertemporal substitutability.

results are qualitatively similar to those presented below. Redefining the announcement period in the FRS sample as the period between March 1998 and September 1999 (that is, from the month of the Budget speech to the month before the actual introduction of WFTC) leads to estimates of $b_0$ that are close to those shown in the next section and, usually, with smaller standard errors.
B. Other Labor Market Outcomes

Earlier studies have suggested that the positive labor supply response of single mothers was predominantly driven by an increase in full-time employment, that is, working 30 hours per week or more (Blundell and Hoyes, 2004; Francesconi and Van der Klaauw, 2007). Column (1) in the top panel of Table 2 confirms this evidence for both BHPS and FRS samples. When we allow for announcement effects (column 2), the findings of Table 1 emerge again. Depending on the sample, the rate at which lone mothers worked full time increased by between 4 and 5 percentage points over the post-reform period, while the announcement effect estimates vary between nearly 2 and 2.6 percentage points. The upper bound of such estimate is found in the BHPS sample with the OLS regression, while the lower bound emerges in the FE model applied to the BHPS sample and in the FRS sample. Notice, however, that all such announcement effect estimates are statistically significant and positive. As before, this is consistent with labor market imperfections causing an increase in employment by women so as to be in a position to benefit from the announced reform the following year.17

Another way to assess how the WFTC announcement influenced single mothers’ behavior is to analyze hours worked. If all women are considered (that is, including those with zero hours of work), the estimates in panel C of Table 2 indicate that, when announcement effects are accounted for it increases the estimated impact of WFTC on hours worked by almost 40 percent in the BHPS sample regardless of the estimation method and by 20 percent in the FRS sample. In either case, the impact of the announcement is statistically significant and large, ranging between 2 and 2.5 extra hours of work per week and representing 50 percent of the overall treatment effect. Restricting the focus only to women with positive hours of work, however, changes our results (panel D): the increase in the estimated treatment effect is more modest (especially in the BHPS sample), while the announcement effect is small and always insignificant. These results suggest that both the WFTC announcement and its introduction had a strong effect on previously nonworking lone mothers, whereas on already working women there is evidence of a slightly smaller effect and no announcement response. Again, the absence of a drop in labor supply fol-

17Similar evidence is revealed when we look at the rate at which single mothers were in paid employment (panel B), although the estimated announcement effects are never statistically significant at conventional levels either across samples or across estimating models.
ollowing the announcement and prior to the implementation of the reform is consistent with relatively small or zero intertemporal substitution effects, or with labor market frictions that dominate these.

There is also evidence that WFTC had a stronger employment impact on mothers with one young child than on mothers with multiple older children (Francesconi and Van der Klaauw, 2007; Gregg, Harkness, and Smith, 2009). A stronger impact of the reform for this subgroup of mothers is consistent with the fact that the increase in the total tax credit (including a larger childcare tax credit) under WFTC was especially large for mothers of young children. Column (1) in Table 3 upholds this result across samples and estimation techniques, although in the FRS sample we also find some significant employment response amongst mothers with two or more children and the youngest child aged 0–4. Allowing for announcement effects again raises the overall impact of the significant treatment effect estimates by about 20 percent (column 2). For example, a lone mother with one child aged 0–4 increased her probability of being in eligible employment by 8.3 percentage points in the FRS sample (a 25 percent increase), and by 9.6 percentage points in the BHPS sample (FE model, a 13 percent increase). Again, in both samples and irrespective of the estimation method, the WFTC announcement leads to a significant increase in the eligible employment rate of mothers of children aged 0–10. This increase is large (ranging between 3 and 5 percentage points) and represents approximately 40 percent of the corresponding total WFTC effect estimate. The larger anticipation effects is consistent with the larger overall effect of the WFTC reform for this subgroup of mothers, and these findings are consistent with the previous interpretation of a limited role of intertemporal substitution and a more dominant role played by labor market imperfections.

C. Paid and Unpaid Childcare Utilization

One of the drivers of the effects of the WFTC reform has been identified in the increase in the tax credit provided to cover childcare costs (Blundell and Hoynes, 2004; Francesconi and Van der Klaauw, 2007; Brewer et al., 2009). The estimates in the first column of Table 4 replicate earlier findings (panel A): the introduction of WFTC led to an increase in the use of paid childcare services of about 2-3 percentage points in both samples and regardless of the estimating method. A similar response emerges also when announcement effects are accounted for. But the 1998 announcement of the reform was not followed by
an immediate change in formal childcare utilization. The announcement effect estimates are always very small and highly insignificant.

This result is confirmed when we disaggregate treatment and announcement effects by age of the youngest child and number of children (panel B). Lone mother with only one child aged 0–4 experienced the greatest increase in the probability of using paid childcare services of approximately 4 percentage points, compared to an increase of 3 percentage points for those with one child aged 5–10. Accounting for announcement effects, however, does not alter any of the treatment effect estimates, and the announcement impact estimates are small and never (individually or jointly) significant.

In panels C and D, where we focus on unpaid childcare usage, however, we find exactly the opposite results. That is, there is evidence of strong announcement effects and of no implementation effect along this margin. The announcement effects emerge especially in the case of mothers of one child of pre-school or school age, i.e., those women who showed sizeable implementation effects in formal childcare utilization.

The switch from informal to formal childcare utilization at the time of the WFTC reform can be explained by the fact that, up to that time, paid childcare services were relatively expensive. In the years preceding WFTC, Family Credit was not generous towards child care expenditures, since it only allowed a small fraction of eligible childcare costs to be disregarded from the calculation of net family income, rather than adding those costs to the maximum credit as done under WFTC. Therefore, forward-looking single mothers who wanted to take advantage of the benefits offered by the WFTC reform had an incentive to enter the labor market prior to its implementation because of frictions in the labor market. Meanwhile they had to find (temporary) arrangements for their children — typically with relatives, neighbors, and unregistered childminders — before placing them in daycare centers or other formal child care arrangements after the introduction of WFTC, when they would have been entitled to receive a substantial childcare tax credit top-up.

D. Transitions in the Labor Market and Childcare Usage

We take advantage of the panel nature of the BHPS sample and examine year-to-year employment transitions. That is, we estimate announcement and implementation effects both on the probability of staying in eligible employment (i.e., conditioning on $y_{it-1} = 1$), and on the probability of starting a job with 16 or more hours of work per week (i.e.,
conditioning on \( y_{it-1} = 0 \). We refer to the former as the persistence probability and the latter as the entry probability. The results are reported in Table 5.

The introduction of WFTC increased single mothers’ persistence probability by almost 6 percentage points. When announcement effects are accounted for (column 2), this effect rises by 20 percent to 7 percentage points, while the probability of staying in eligible employment between the Fall of 1997 and the Fall of 1998 (roughly six months after the official announcement in Parliament) went up as well, by almost 2.5 percentage points. Entry rates into WFTC-eligible jobs show similar patterns, although the magnitude of the effects is generally slightly smaller. These results emphasize again the importance of labor supply decisions that were taken between the announcement of the reform and its implementation. They also confirm the finding that such decisions are consistent with the presence of labor market imperfections.

Another way of documenting the extent of labor market dynamics is to analyze changes in hours worked between two successive years. Blundell, Brewer, and Francesconi (2008) show that the introduction of the WFTC reform led to a substantial increase in single mothers’ hours of work. They find that this adjustment primarily occurred through job changes rather than labor supply adjustments within a job, but did not examine the presence of anticipation effects. We repeat their analysis allowing for such effects but without differentiating by whether there was a job change. As in their study, we use the BHPS data and restrict our sample to single women who have been observed working at least two consecutive times during the sample period. Without announcement effects, the results (not shown for convenience) are close to those reported in Blundell, Brewer and Francesconi (2008), with a significant increase of 1.73 (s.e.=0.67) hours per week. Accounting for announcement effects increases the treatment effect estimate to 2.09 (s.e.=0.74) hours per week (approximately a 20 percent rise), and between 1997 and 1998 the change in hours worked was also positive and significant amounting to almost 0.5 (s.e.=0.18) additional weekly hours (about a quarter of the treatment effect). These figures tie in well with the hours estimates shown in Table 2 and with our understanding of anticipatory behavior in the presence of labor market frictions.

We build on the previous results on childcare usage and examine labor market transitions jointly with childcare utilization decisions. Four processes are estimated using the BHPS sample, the results of which are in Table 6. We begin with the transition from
nonwork (including non-eligible employment) to eligible employment with paid childcare services. The estimates (in the first row of Table 6) reveal a strong positive implementation effect of 3.9 percentage points. That is, almost three-quarters of the mothers who entered eligible employment as a result of the reform did so by also choosing to use paid childcare services. This effect, however, was not preceded by any similar change in the previous year. The previous year instead witnessed an increase of 2.1 percentage points in the probability of entering eligible employment (from nonwork) with reliance on unpaid childcare services (second row). These results strongly uphold our earlier story based on forward looking behavior and labor market imperfections. Single mothers anticipated the introduction of the 1999 reform by entering eligible employment while using informal childcare, when Family Credit did not provide a generous support to childcare expenditures. With the introduction of WFTC, they stayed in eligible employment but took advantage of the more generous support to childcare costs and switched to paid childcare services.

The next two transition processes shown in Table 6 confirm this interpretation. About two-thirds of the increase in the persistence probability in eligible employment (measured in Table 5) are driven by mothers who moved to paid childcare services after the introduction of the reform (third row). Conversely, the whole anticipation effect in the same persistence probability can be attributed to women who did not rely on formal childcare arrangements.

E. Sensitivity Analysis

To demonstrate the robustness of the results, we performed a number of sensitivity checks. First, because on average single mothers achieve lower educational levels, we restricted the control group to single childless women with educational qualifications below A level and repeated the analysis using the BHPS sample. This restriction marginally reduces both treatment and announcement effects, but the estimates (not shown) are still significant and not statistically different from those reported in Tables 1 and 2. Second, a broadly similar conclusion emerged when the control group consisted of mothers who were married to, or cohabited with, partners whose pre-reform earnings were in the bottom quartile of the male earnings distribution. In the case of the FRS sample, however, the treatment

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18The same exercise cannot be repeated on the FRS sample. As shown in Appendix Table A1, the FRS does not contain detailed information on educational qualifications, but only on whether an individual left full time education at age 18.

19In a study of the effect of WFTC on married women’s behavior, Francesconi, Rainer, and van der Klaauw (2009) distinguish a wide range of couples on the basis of wife’s education and husband’s earnings
effect estimate for eligible time employment and the announcement effect estimate for full
time employment lost statistical significance.

Third, to model the pre-reform trend more flexibly, we repeated our analysis including
quadratic (rather than linear) pre-program trends. Irrespective of the estimating sample,
this alternative specification did not alter any of the key findings of the study. Fourth,
similar results also emerged when we restricted both samples to single women aged 55 or
less, a restriction that was motivated by the observation that single mothers tend to be
more concentrated in the middle of the age distribution. Fifth, to account for the fact that
the FRS interviews individuals over the entire year rather than the September-December
period (as typically done in the BHPS), we included indicators for the season (quarter) of
interview. The inclusion of such indicators does not affect any of the results.

Another survey-specific concern is related to the different temporal length of the two
surveys under analysis. This may have an effect on how pre-program trends can influence
the evaluation analysis. To have an insight into this issue, we re-estimated the BHPS
sample using data only from the 1995 wave onwards, covering the same time period as the
FRS. The results for the labor market outcomes are reported in Table 7. Both implemen-
tation and announcement effect estimates are generally smaller than those found in the
full BHPS sample. Importantly, the announcement effects along full time employment and
hours of work remain positive and statistically significant, while those along eligible em-
ployment and the work/nowork margin lose statistical significance, as it is the case when
these were estimated using the FRS sample. Similar patterns are revealed by the employ-
ment transition estimates, with smaller (but positive and still significant) implementation
effects, and smaller and insignificant announcement effects in both cases. These results,
therefore, reveal that some of the discrepancies between the BHPS and FRS results, and
especially those related to the magnitude of the implementation effects, might be driven
by the different time length of the two surveys.

A different concern is that the labor market changes between the announcement and
the implementation of the reform do not capture genuine anticipation effects. Rather,
they might reflect the fact that single mothers are in low-pay jobs and such jobs are
typically short lived (Card and Blank, 1990; Stewart 2007). If this were the case, then, we
and labor supply. They concluded that the responses of women married to men in the bottom quartile
of the male earnings to the introduction of WFTC were similar to single mothers’ responses along several
margins.
would expect a substantial fraction of the jobs started after the WFTC announcement be terminated by the time the reform was introduced.\footnote{Recent U.S. evidence documents that welfare recipients have fairly low turnover rates and high job retention rates as compared to other (non-welfare recipient) employees in comparable jobs (Holzer, Stoll, and Wissoker, 2004). In addition, Farber (2008) shows that, although long-term employment relationships have become much less common for men in the private sector, women have seen no systematic change in job durations over the last 30 years. Similar results for women have also emerged for Canada (Heisz, 2005). For British single mothers over the years in our sample period, Yeo (2007) reports a median duration of 30 months for a full-time job and of 18 months for a job in eligible employment.}

To assess this possibility, we use the wave-on-wave job history information collected by the BHPS and identify the months in which a woman started and stopped working. We do this for all the women who reported to be in a paid job at the time of interview in the 1997–2000 period, so that the entire announcement and part of the post-reform periods are covered. Figure 4 shows the monthly rates of entry into all jobs for single mothers and single childless women. On average, 6 percent of all employed single women (whether with children or not) entered into a new job each month between March 1997 and the month prior to the introduction of WFTC (September 1999). Although single childless women had a greater entry rate before the announcement, both groups of women displayed similar entry behavior after March 1998, with single mothers having a slightly greater probability of starting a job than single childless women. We use such data to compute job durations for the two groups of women. Table 8 reports Kaplan-Meier estimates of job duration, including both completed and uncompleted spells, where job tenure is defined as months in the same job with the same employer. The estimate show comparable job exit behavior among women in the two groups, which have a median job tenure of about 36 months. For robustness, we repeated the same exercise using a sample of unpartnered women drawn from the 1998-2000 Labour Force Surveys (LFS). The LFS results are reported at the bottom of Table 8. While the estimates for single mothers are essentially identical to those found with the BHPS data, those for single childless women suggest slightly greater exit rates, with a median job tenure of about 26 months. With this evidence, we argue that the estimated employment effects of WFTC are not likely to be the result of high labor market churning among single mothers.
V. Conclusion

The aim of this paper has been to document whether and how economic agents adjust their labor supply behavior in anticipation of in-work benefit reform. We started by formulating a simple model of female labor force decisions which embeds an in-work benefit reform and explicitly allows for announcement and anticipation effects. The model emphasizes two mechanisms through which women can respond to the announcement of the reform, namely, intertemporal substitution and labor market frictions. The model predicts that, if labor market frictions have a dominant role, a greater fraction of women should be at work between the announcement and the actual introduction of the reform in order to increase the chance that they could benefit from the new tax credit after its introduction. If instead intertemporal substitution effects dominate, then we expect to observe a decline in labor market participation between the announcement and the implementation of the reform.

Our empirical analysis looks at single mothers’ labor supply behavior in relation to the Working Families’ Tax Credit, a major in-work benefit reform introduced in Britain in October 1999 and formally announced by the UK government 18 months earlier in March 1998. The analysis uncovers strong evidence of announcement effects of WFTC on single mothers’ labor supply. These effects turn out to be positive, and thus in line with a story based on labor market frictions rather than intertemporal substitutability. The magnitude of the estimated announcement effects is large, typically half the size of the corresponding estimated treatment effects. In addition, treatment effect estimates that ignore announcement effects are biased downward, in the order of 15 to 35 percent. These results are robust to different labor market outcome measures and to a number of sensitivity checks.

In the case of paid (formal) childcare utilization, we find sizeable implementation effects, but no announcement effect. Conversely, there are large announcement effects and no implementation effect in the case of unpaid (informal) childcare utilization. This again reveals cogent evidence of forward looking behavior. Women in fact had to pay for formal childcare services without receiving benefits directly to cover such costs before the introduction of WFTC. Instead, they would have received a generous childcare tax credit after its implementation. Results based on transitions of childcare decisions confirm this interpretation.
In a world in which agents are forward looking and anticipation effects are allowed, individual behavior can be affected not only by the implementation of a public policy reform but also by its absence. If a reform is anticipated or announced in advance of its introduction, this anticipatory behavior may influence the selection of the appropriate econometric evaluation procedure, such as the choice of the pre-program period in a difference-in-difference design. It may also affect the size of the estimated impacts. The role and consequences of forward looking behavior, where people may act in anticipation or in response to announcement effect prior to policy reforms, as well as its effect on how such reforms should be evaluated represent important and relatively understudied topics for future research.
Appendix A

Solution to the Dynamic Program

The standard solution method for finite horizon dynamic programming problems is backward recursion (Eckstein and Wolpin, 1989). Letting $V_t^d(X_{it-1}, \epsilon_{it})$ be the maximum of expected discounted remaining lifetime utility given $X_{it-1}$ prior periods of employment, implementation of welfare reform $d$ and a wage draw of $\epsilon_{it}$, then

$$V_t(X_{it-1}, d_t, \epsilon_{it}) = \max[V_t^1(X_{it-1}, d_t, \epsilon_{it}), V_t^0(X_{it-1}, d_t, \epsilon_{it})],$$

where $V_t^1(\cdot)$ and $V_t^0(\cdot)$ denote the expected discounted (remaining) lifetime utilities if the woman $i$ works in current period $t$ ($y_{it} = 1$) and does not work ($y_{it} = 0$) respectively. At the terminal period ($t = 3$), the value functions when $J_3 = \{0, 1\}$ are:

$$V_{i3}^1(X_{i2}, d_3, \epsilon_{i3}) = \begin{cases} (1 + \gamma_3)(w_0 + \alpha X_{i2} + \beta d_3 + \epsilon_{i3} + N_{i3}) + \gamma_1 + \gamma_2 X_{i2} & (7) \\ N_{i3} & (8) \end{cases}$$

The woman works if $V_t^1(\cdot)$ is greater than $V_t^0(\cdot)$. The explicit decision rule governing the participation decision at $t = 3$ is then given by

$$y_{i3} = 1 \quad \text{iff} \quad \epsilon_{i3} \geq -w_0 - \alpha X_{i2} - \beta d_3 - \frac{(\gamma_3 N_{i3} + \gamma_1 + \gamma_2 X_{i2})}{1 + \gamma_3} = \epsilon_{i3}^*(X_{i2}, d_3)$$

$$y_{i3} = 0 \quad \text{otherwise.}$$

Thus, the expected value in period 3 for a woman who does not face labor market frictions is

$$EV_{i3}^{(0,1)}(X_{i2}, d_3) = \Pr(\epsilon_{i3} > \epsilon_{i3}^*(X_{i2}, d_3)) \left\{ (1 + \gamma_3)\left[w_0 + \alpha X_{i2} + \beta d_3 \right. \\ +E(\epsilon_{i3} | \epsilon_{i3} > \epsilon_{i3}^*(X_{i2}, d_3)) \left. \right] + N_{i3} + \gamma_1 + \gamma_2 X_{i2} \right\} + \left[1 - \Pr(\epsilon_{i3} > \epsilon_{i3}^*(X_{i2}, d_3))\right]N_{i3} \quad (9)$$

In the case in which $J_3 = \{0\}$, that is, when the woman has no job available because of labor market frictions, the expected value is

$$EV_{i3}^{(0)}(X_{i2}, d_3) = N_{i3}. \quad (10)$$

Combining (9) and (10) yields the expected remaining lifetime utility value to each woman for period 3, namely

$$EV_{i3}(X_{i2}, y_{i2}, d_3) = \lambda_3(y_{i2})EV_{i3}^{(0,1)}(X_{i2}, d_3) + (1 - \lambda_3(y_{i2}))EV_{i3}^{(0)}(X_{i2}, d_3),$$

with $\lambda_3(1) = 1$. At time $t = 2$, the value functions corresponding to (7) and (8) will have to account for beliefs regarding the possible implementation of a reform in period 3.
Therefore, the value functions when woman \( i \) works and when she does not work are given respectively by

\[
V_{i2}^1(X_{i1}, d_2; \epsilon_{i2}) = (1 + \gamma_3)(w_0 + \alpha X_{i1} + \beta d_2 + \epsilon_{i2} + N_{i2}) + \gamma_1 + \gamma_2 X_{i1} + \delta \pi_{23}(d_2)EV_{i3}(X_{i1} + 1, 1, 1) + \delta(1 - \pi_{23}(d_2))EV_{i3}(X_{i1} + 1, 1, 0)
\]

\[
V_{i2}^0(X_{i1}, d_2) = N_{i2} + \delta \pi_{23}(d_2)EV_{i3}(X_{i1}, 0, 1) + \delta(1 - \pi_{23}(d_2))EV_{i3}(X_{i1}, 0, 0).
\]

where it is important to recall again that \( \pi_{23}(1) = 1 \). Then, the decision rule at \( t = 2 \) is

\[
y_{i2} = 1 \text{ iff } \epsilon_{i2} \geq -w_0 - \alpha X_{i1} - \beta d_2 - \frac{(\gamma_3 N_{i2} + \gamma_1 + \gamma_2 X_{i1})}{1 + \gamma_3} - \delta \pi_{23}(d_2)\left[EV_{i3}(X_{i1} + 1, 1, 1) - EV_{i3}(X_{i1}, 0, 1)\right] - \delta(1 - \pi_{23}(d_2))\left[EV_{i3}(X_{i1} + 1, 1, 0) - EV_{i3}(X_{i1}, 0, 0)\right],
\]

\[
y_{i2} = 0 \text{ otherwise.}
\]

As in the case of the last period, the expected value to each woman is a weighted sum of the value which is expected when jobs are available and the value expected when there is no job available. Therefore, for any given beliefs about the likelihood of a reform at \( t = 3, \pi_{23} \), we have

\[
EV_{i2}(X_{i1}, y_{i1}, d_2|\pi_{23}) = \lambda_2(y_{i1})EV_{i2}^{[0,1]}(X_{i1}, d_2|\pi_{23}) + (1 - \lambda_2(y_{i1}))EV_{i2}^{[0]}(X_{i1}, d_2|\pi_{23}),
\]

(11)

where the choice-specific expected value functions in (11) are given by

\[
EV_{i2}^{[0,1]}(X_{i1}, d_2|\pi_{23}) = \Pr(\epsilon_{i2} > \epsilon_{i2}^*(X_{i1}, d_2))\left\{ (1 + \gamma_3)\left[w_0 + \alpha X_{i1} + \beta d_2 + E(\epsilon_{i2}|\epsilon_{i2} > \epsilon_{i2}^*(X_{i1}, d_2))\right] + N_{i2} + \gamma_1 + \gamma_2 X_{i1} + \delta \pi_{23}EV_{i3}(X_{i1} + 1, 1, 1) + \delta(1 - \pi_{23})EV_{i3}(X_{i1} + 1, 1, 0) \right\}
\]

\[
+ \left[ 1 - \Pr(\epsilon_{i2} > \epsilon_{i2}^*(X_{i1}, d_2)) \right] N_{i2} + \delta \pi_{23}EV_{i3}(X_{i1}, 0, 1) + \delta(1 - \pi_{23})EV_{i3}(X_{i1}, 0, 0)
\]

\[
EV_{i2}^{[0]}(X_{i1}|\pi_{23}) = N_{i2} + \delta \pi_{23}EV_{i3}(X_{i1}, 0, 1) + \delta(1 - \pi_{23})EV_{i3}(X_{i1}, 0, 0).
\]

Finally, in period \( t = 1 \), in which \( X_{i0} \) is given (and set equal to zero for simplicity), the value functions depending on whether woman \( i \) works or does not work are given respectively by

\[
V_{i1}^1(X_{i0}, \epsilon_{i1}) = (1 + \gamma_3)(w_0 + \epsilon_{i1} + N_{i1}) + \gamma_1 + \delta \pi_{12}EV_{i2}(X_{i0} + 1, 1, 1|\pi_{13}) + \delta(1 - \pi_{12})EV_{i2}(X_{i0} + 1, 1, 0|\pi_{13})
\]

\[
V_{i1}^0(X_{i0}, \epsilon_{i1}) = N_{i1} + \delta \pi_{12}EV_{i2}(X_{i0}, 0, 1|\pi_{13}) + \delta(1 - \pi_{12})EV_{i2}(X_{i0}, 0, 0|\pi_{13}),
\]

and the decision rule in a frictionless world (when \( J_{i1} = \{0, 1\} \)) is

\[
y_{i1} = 1 \text{ iff } \epsilon_{i1} \geq -w_0 - \frac{(\gamma_3 N_{i1} + \gamma_1)}{1 + \gamma_3} - \delta \pi_{12}\left[EV_{i2}(X_{i0} + 1, 1, 1|\pi_{13}) - EV_{i2}(X_{i0}, 0, 1|\pi_{13})\right] - \delta(1 - \pi_{12})\left[EV_{i2}(X_{i0} + 1, 1, 0|\pi_{13}) - EV_{i2}(X_{i0}, 0, 0|\pi_{13})\right] = \epsilon_{i1}^*(X_{i0})
\]

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\[ y_{i1} = 0 \quad \text{otherwise}, \]

while, regardless of the realization of \( \epsilon_{i1} \), \( y_{i1} \) is always equal to 0 in an economy with job market frictions, that is, when \( J_{i1} = \{0\} \).
Appendix B

A Description of the WFTC Program

Up to April 2003, the main in-work support program in the UK has been the Working Families’ Tax Credit (WFTC), which replaced Family Credit (FC) on October 5, 1999.\textsuperscript{21} Along with other active labor market programs (such as the various welfare-to-work “New Deal” schemes) and the introduction of the National Minimum Wage, WFTC had a crucial part in the central government’s antipoverty strategy. By the end of 2002, it reached almost 2.7 million children in 1.4 million families (a 70-percent increase from November 1998 when FC was still in place), and at a cost of around £6 billion a year (approximately 10 billion in current US dollars). To put these figures into perspective, in February 2003, a total of 2.5 million children were living in families claiming at least one of the other key means-tested welfare benefits, including Income Support and Housing Benefits, while the government spending on, say, Income Support alone — the primary cash transfer to low-income nonworking individuals (in many respects similar to AFDC or TANF in the United States) — was around £13 billion a year.

A family needs to meet three basic requirements in order to be eligible for WFTC. First, at least one adult in the family (or the lone parent in a single-parent family) must work 16 hours or more per week.\textsuperscript{22} Second, the family must have at least one dependent child. A dependent child is a child, grandchild, stepchild or foster child of the family who is under the age of 16 (or under 19 if in full-time non-university education). Third, family savings and capital must be below a given amount (which, in current prices, was set at 8,000 over our entire sample period) and net family income must be sufficiently low. In fact, families with incomes below a specified “threshold” or “applicable amount” (which increased from 62.25 per week in 1991 to 92.90 per week in 2001) receive maximum credit; when incomes are greater than the threshold, the maximum credit is reduced by a proportion (known as “taper rate”) of the difference between net family income and threshold. Besides net family income and hours worked, the amount of the weekly credit to which a family is entitled depends on the number and ages of children and childcare costs, in the form of a basic child credit and a childcare credit.

There are a few parameters through which the WFTC reform potentially increased the generosity of in-work support relative to FC. First, the WFTC system substantially increased the credit for younger children in the age group 0-10. The nominal increment of £5 per week represents a 34 percent increase between 1998 and 1999 (while the annual increase of the credit for children in the same age group between 1991 and 1998 was, on average, 6.6 percent only). Second, the income threshold grew by 14 percent from £79 to £90 per week, whereas its average annual growth in each of the previous eight years had been 3.4 percent. Third, the taper rate at which earnings above the threshold are taxed was lowered from 70 percent to 55 percent. Fourth, WFTC is more generous with

\textsuperscript{21}More detailed descriptions of the program are in Blundell and Hoynes (2004) and Francesconi and van der Klaauw (2007). It should be noted that, in April 2003 and thus after the end of our sample period, WFTC was itself replaced by two new tax credits: the Child Tax Credit (CTC) and the Working Tax Credit (WTC).

\textsuperscript{22}At its introduction in 1988, Family Credit set the minimum hour cutoff at 24 hours per week, which was reduced to 16 in 1992. In addition, from July 1995, FC was modified to provide an extra £10 credit for those working 30 hours per week or more. This feature was retained by the WFTC reform, with the additional credit set at 11.15 in October 1999.
eligible childcare costs.\textsuperscript{23} From October 1994, FC allowed eligible childcare costs (up to a maximum amount, which was £60 per week just before WFTC was introduced) to be disregarded from the calculation of net family income.\textsuperscript{24} This meant that the maximum childcare support was £70 per week for a family with one child, and £105 per week for a family with two or more children.

To get an idea of the impact of the reform, consider a single mother with one child aged 6, who works more than 16 hours per week (but less than 30), has net earnings of £150 per week and pays 60 per week for childcare. In 1999 under the WFTC regime, she would receive a credit of £81.15 per week. In 1998 under the FC regime, the same woman with the same characteristics would receive a credit of 56.80 per week (in constant 1999 prices), that is 43 percent less than in 1999. If the woman had net earnings of £200 per week, her credit in both years would be lower, but in 1999 she would receive 2.5 times more than she would in 1998 (£53.65 versus £21.25 per week). For the empirical earnings and childcare expenditures distributions of mothers working 16 or more hours per week in 1998, and assuming no behavioral responses, the reform implied an average credit increase of about 20 percent (from an average of £42 to almost £50 per week), with a quarter of those mothers seeing an increase of at least 50 percent.

Unlike FC, WFTC was not administered by the Benefits Agency but by Inland Revenue. In line with the government’s effort to reduce the potential stigma associated with claiming in-work benefits, this administrative feature and the receipt of the credit through the wage packet directly from the employer were intended to emphasize that WFTC was indeed a tax credit rather than a welfare benefit (H.M. Treasury, 1998). Therefore, while in many respects WFTC was similar to EITC in the United States, it differed from it in that WFTC had no phase-in rate but instead a minimum hours requirement of 16 per week, it had a higher phase-out rate (taper rate), included a generous childcare tax credit, and it was administered and paid out differently.

\textsuperscript{23}To be “eligible” (or “relevant”), childcare services must be provided by registered childminders, day nurseries, and after-school clubs, or certain other special schools or establishments that are exempt from registration. Relevant childcare can be for any child in the family up to age 11 until May 1998, or up to age 12 from June 1998 to May 2000, or up to age 15 from June 2000 onward.

\textsuperscript{24}This was the disregard for families with one child. In 1998 a disregard of £100 was introduced for families with two or more children.
References


Auerbach and Siegel, 2000.


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Meyer, Bruce. 1990. *Econometrica*


Fig 1. Trends in Employment Rates
No frictions and no intertemporal substitution

\[ \delta = 0.9, \alpha = 0, \beta = 1, w_0 = 1, \gamma_1 = \gamma_3 = 0, \lambda(0) = 1, \gamma_2 = 0. \] Baseline: \( \pi_1 = \pi_2 = 0 \) & no reform;
scenario 1: \( \pi_1 = \pi_2 = 0 \) & reform in period 3; scenario 2: \( \pi_1 = 0, \pi_2 = 1 \) & reform in period 3;
scenario 3: \( \pi_1 = 0.5, \pi_2 = 1 \) & reform in period 3; scenario 4: \( \pi_1 = 0, \pi_2 = 1 \) & no reform in period 3;
Fig 2. Trends in Employment Rates

Frictions - no intertemporal substitution

\[ \delta = 0.9, \alpha = 0, \beta = 1, w_0 = 1, \gamma_1 = \gamma_3 = 0, \lambda(0) = 0.5, y_2 = 0. \]
Baseline: \( \pi_1 = \pi_2 = 0 \) & no reform;
scenario 1: \( \pi_1 = \pi_2 = 0 \) & reform in period 3;
scenario 2: \( \pi_1 = 0, \pi_2 = 1 \) & reform in period 3;
scenario 3: \( \pi_1 = 0.5, \pi_2 = 1 \) & reform in period 3;
scenario 4: \( \pi_1 = 0, \pi_2 = 1 \) & no reform in period 3;
Fig 3. Trends in Employment Rates
No frictions - intertemporal substitution

\[ \delta = 0.9, \alpha = 0, \beta = 1, w_0 = 1, \gamma_1 = \gamma_3 = 0, \lambda(0) = 1, \gamma_2 = -1.5. \] Baseline: \( \pi_1 = \pi_2 = 0 \) & no reform;
scenario 1: \( \pi_1 = \pi_2 = 0 \) & reform in period 3; scenario 2: \( \pi_1 = 0, \pi_2 = 1 \) & reform in period 3;
scenario 3: \( \pi_1 = 0.5, \pi_2 = 1 \) & reform in period 3; scenario 4: \( \pi_1 = 0, \pi_2 = 1 \) & no reform in period 3;
(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week

(a) FC and WFTC schedules, weekly awards

With childcare benefit

In-work benefit (£/week)

Hours per week
(b) Budget constraints under FC and WFTC

With childcare benefit

Disposable income (£/week)

Hours per week

FC WFTC
WFTC (childcare benefit as under FC)
Table 1: Key Parameters of the Tax Credits at Baseline (FS regime), at Announcement, and at WFTC regime

<table>
<thead>
<tr>
<th></th>
<th>1998 (FS regime)</th>
<th>1998 Budget (Announcement)</th>
<th>1999 (WFTC regime)</th>
<th>Difference (3)–(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Basic rate</td>
<td>£48.80</td>
<td>£48.80</td>
<td>£52.30</td>
<td>£3.50</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(£49.78)</td>
<td>(+7.1%)</td>
</tr>
<tr>
<td>Additional credit for working 30+ hours</td>
<td>£10.80</td>
<td>£10.80</td>
<td>£11.15</td>
<td>£0.35</td>
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<tr>
<td></td>
<td></td>
<td></td>
<td>(£11.02)</td>
<td>(+3.2%)</td>
</tr>
<tr>
<td>Credit for child aged:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0–10</td>
<td>£14.85</td>
<td>£17.35</td>
<td>£19.85</td>
<td>£2.50</td>
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<tr>
<td></td>
<td></td>
<td>(£17.70)</td>
<td></td>
<td>(+14.4%)</td>
</tr>
<tr>
<td>11–15</td>
<td>£20.45</td>
<td>£20.45</td>
<td>£20.90</td>
<td>£0.45</td>
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<tr>
<td></td>
<td></td>
<td>(£20.86)</td>
<td></td>
<td>(+0.02%)</td>
</tr>
<tr>
<td>16–18</td>
<td>£25.40</td>
<td>£25.40</td>
<td>£25.95</td>
<td>£0.55</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(£25.91)</td>
<td></td>
<td>(+0.02%)</td>
</tr>
<tr>
<td>Taper rate</td>
<td>70%</td>
<td>55%</td>
<td>55%</td>
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<tr>
<td>Threshold</td>
<td>£79.00</td>
<td>£90.00</td>
<td>£90.00</td>
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<tr>
<td>Childcare tax credit</td>
<td>Costs deducted</td>
<td>70% of up to:</td>
<td>70% of up to:</td>
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<tr>
<td></td>
<td>from earnings</td>
<td>£100 (1 kid)</td>
<td>£100 (1 kid)</td>
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</tr>
<tr>
<td></td>
<td></td>
<td>£150 (2+ kids)</td>
<td>£150 (2+ kids)</td>
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Table 2: WFTC Treatment and Announcement Effects — Eligible Employment

<table>
<thead>
<tr>
<th></th>
<th>BHPS (N=15,260)</th>
<th>FRS (N=76,886)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>OLS</strong></td>
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<td></td>
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<tr>
<td>Treatment (b)</td>
<td>0.051 (0.016)</td>
<td>0.060 (0.018)</td>
</tr>
<tr>
<td>Announcement (b0)</td>
<td>0.029 (0.014)</td>
<td>0.018 (0.016)</td>
</tr>
<tr>
<td><strong>FE</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment (b)</td>
<td>0.049 (0.018)</td>
<td>0.059 (0.019)</td>
</tr>
<tr>
<td>Announcement (b0)</td>
<td>0.027 (0.015)</td>
<td></td>
</tr>
</tbody>
</table>


*Notes:* Standard errors are shown in parentheses. Estimates in bold are significant at the 5 percent level. \( N \) denotes the number of observations. The dependent variable is equal to one if a woman works 16 or more hours per week, and zero otherwise. ‘Announcement’ stands for announcement effect. It is measured over the period that goes from March 1998 to September 1999 in the FRS sample; whereas in the BHPS sample it is a dummy variable that is equal to one if the interview wave is 1998. ‘Treatment’ stands for treatment effect. It is measured over the period that goes from October 1999 to December 2001 in the FRS sample; while in the BHPS sample it is a dummy variable that is equal to one if the interview waves are 1999 (after October 5), 2000 or 2001. The other variables used in estimation are a quartic polynomial in age, number of children by age group (3 groups: ages 0-4, ages 5-10, and ages 11-18), dummy variables for ethnic origin (4 dummies; white is the base category), highest educational level in the BHPS sample (5; no qualification) and whether left full-time education at age 18 in the FRS sample, housing tenure (2; owner), region of residence (16; Greater London), interaction terms of a woman’s age with number of children by age group and with her education, interaction terms of a woman’s education with number of children by age group, and group specific linear trends.

*Estimates are obtained from linear probability models without and with individual fixed effects (OLS and FE, respectively).*

**b** *Estimates are obtained from linear probability models (OLS only).*
Table 3: WFTC Treatment and Announcement Effects — Other Employment Outcomes

<table>
<thead>
<tr>
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<th>BHPS</th>
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<th>FRS</th>
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</thead>
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<td>(2)</td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td></td>
<td>(b)</td>
<td>(b)</td>
<td>(b₀)</td>
<td>(b)</td>
</tr>
<tr>
<td>(b)</td>
<td>(b)</td>
<td>(b₀)</td>
<td>(b)</td>
<td>(b)</td>
</tr>
<tr>
<td>Panel A: Full time employment(^a)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td>0.045</td>
<td>0.054</td>
<td>0.026</td>
<td>0.030</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.017)</td>
<td>(0.013)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>FE</td>
<td>0.042</td>
<td>0.049</td>
<td>0.020</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.021)</td>
<td>(0.009)</td>
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<tr>
<td>Panel B: Employment(^b)</td>
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<tr>
<td>OLS</td>
<td>0.056</td>
<td>0.061</td>
<td>0.017</td>
<td>0.052</td>
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<td></td>
<td>(0.017)</td>
<td>(0.020)</td>
<td>(0.014)</td>
<td>(0.020)</td>
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<tr>
<td>FE</td>
<td>0.052</td>
<td>0.055</td>
<td>0.019</td>
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<tr>
<td></td>
<td>(0.017)</td>
<td>(0.019)</td>
<td>(0.014)</td>
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<tr>
<td>Panel C: Hours worked(^c)</td>
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<tr>
<td>OLS</td>
<td>3.32</td>
<td>4.60</td>
<td>2.41</td>
<td>3.58</td>
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<tr>
<td></td>
<td>(0.73)</td>
<td>(0.93)</td>
<td>(0.75)</td>
<td>(0.77)</td>
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<tr>
<td>FE</td>
<td>2.95</td>
<td>4.12</td>
<td>1.96</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.68)</td>
<td>(0.63)</td>
<td></td>
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<tr>
<td>Panel D: Hours worked(^d)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td>2.45</td>
<td>2.51</td>
<td>0.11</td>
<td>2.30</td>
</tr>
<tr>
<td></td>
<td>(0.78)</td>
<td>(1.03)</td>
<td>(0.82)</td>
<td>(0.63)</td>
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<tr>
<td>FE</td>
<td>2.30</td>
<td>2.37</td>
<td>0.06</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.58)</td>
<td>(0.72)</td>
<td>(0.38)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors are shown in parentheses. Estimates in bold are significant at the 5 percent level. See the notes to Table 2 for other details, including sample sizes, unless differently noted.

\(^a\) Equals to 1 if a woman works 30 or more hours per week, and 0 otherwise.

\(^b\) Equals to 1 if a woman works any positive number of hours per week, and 0 otherwise.

\(^c\) Includes women with zero hours of work.

\(^d\) Excludes women with zero hours of work. The number of observations is 10446 (of which, 3493 on single mothers and 6953 on single childless women) and 52,483 (of which, 17,138 on single mothers and 35,345 on single childless women) in the BHPS and the FRS samples, respectively.
Table 4: WFTC Treatment and Announcement Effects — Eligible Employment by Age of Youngest Child and Number of Children

<table>
<thead>
<tr>
<th></th>
<th>BHPS</th>
<th>FRS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (b)</td>
<td>(2) (b)</td>
</tr>
<tr>
<td>One child aged 0–4</td>
<td><strong>0.097</strong> (0.021)</td>
<td><strong>0.121</strong> (0.029)</td>
</tr>
<tr>
<td>One child aged 5–10</td>
<td><strong>0.078</strong> (0.023)</td>
<td><strong>0.094</strong> (0.035)</td>
</tr>
<tr>
<td>One child aged 11–18</td>
<td>0.031 (0.026)</td>
<td>0.033 (0.028)</td>
</tr>
<tr>
<td>Two children or more, youngest aged 0–4</td>
<td>0.044 (0.032)</td>
<td><strong>0.047</strong> (0.023)</td>
</tr>
<tr>
<td>Two children or more, youngest aged 5–10</td>
<td>0.003 (0.028)</td>
<td>0.003 (0.030)</td>
</tr>
<tr>
<td>Two children or more, youngest aged 11–18</td>
<td>0.002 (0.031)</td>
<td>0.005 (0.033)</td>
</tr>
</tbody>
</table>

**FE**

<table>
<thead>
<tr>
<th></th>
<th>BHPS</th>
<th>FRS</th>
</tr>
</thead>
<tbody>
<tr>
<td>One child aged 0–4</td>
<td><strong>0.085</strong> (0.025)</td>
<td><strong>0.096</strong> (0.026)</td>
</tr>
<tr>
<td>One child aged 5–10</td>
<td><strong>0.070</strong> (0.031)</td>
<td><strong>0.084</strong> (0.024)</td>
</tr>
<tr>
<td>One child aged 11–18</td>
<td>0.032 (0.022)</td>
<td>0.028 (0.023)</td>
</tr>
<tr>
<td>Two children or more, youngest aged 0–4</td>
<td>0.038 (0.021)</td>
<td><strong>0.043</strong> (0.020)</td>
</tr>
<tr>
<td>Two children or more, youngest aged 5–10</td>
<td>0.020 (0.024)</td>
<td>0.019 (0.024)</td>
</tr>
<tr>
<td>Two children or more, youngest aged 11–18</td>
<td>0.009 (0.033)</td>
<td>0.011 (0.032)</td>
</tr>
</tbody>
</table>

**Notes:** Standard errors are shown in parentheses. Estimates in bold are significant at the five percent level. Definitions, number of observations, and list of variables used in estimation are in the notes to Table 2.
Table 5: Paid Child Care Utilization, Overall and by Age of Youngest Child and Number of Children

<table>
<thead>
<tr>
<th></th>
<th>BHPS (N = 5,616)</th>
<th>FRS (N = 35,469)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td></td>
<td>(b)</td>
<td>(b)</td>
</tr>
<tr>
<td></td>
<td>(b)</td>
<td>(b)</td>
</tr>
<tr>
<td>Panel A: Paid child care utilization&lt;sup&gt;a&lt;/sup&gt;</td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td>0.031 (0.010)</td>
<td>0.031 (0.011)</td>
</tr>
<tr>
<td>FE</td>
<td>0.028 (0.013)</td>
<td>0.029 (0.012)</td>
</tr>
</tbody>
</table>

Panel B: Paid child care utilization by child’s age and number of children<sup>a</sup>

<p>| | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>One child aged 0–4</td>
<td>0.047 (0.010)</td>
<td>0.044 (0.013)</td>
<td>0.032 (0.008)</td>
<td>0.035 (0.010)</td>
</tr>
<tr>
<td>One child aged 5–10</td>
<td>0.038 (0.009)</td>
<td>0.041 (0.016)</td>
<td>0.028 (0.007)</td>
<td>0.028 (0.009)</td>
</tr>
<tr>
<td>Two children or more,</td>
<td>0.013 (0.019)</td>
<td>0.011 (0.017)</td>
<td>0.003 (0.006)</td>
<td>0.005 (0.012)</td>
</tr>
<tr>
<td>youngest aged 0–4</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>FE</td>
<td>0.040 (0.014)</td>
<td>0.043 (0.019)</td>
<td>0.023 (0.009)</td>
<td></td>
</tr>
<tr>
<td>One child aged 0–4</td>
<td>0.031 (0.012)</td>
<td>0.030 (0.014)</td>
<td>0.000 (0.014)</td>
<td></td>
</tr>
<tr>
<td>One child aged 5–10</td>
<td>0.011 (0.023)</td>
<td>0.006 (0.025)</td>
<td>0.008 (0.009)</td>
<td></td>
</tr>
</tbody>
</table>

Panel C: Unpaid child care utilization<sup>b</sup>

<p>| | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td>0.002 (0.019)</td>
<td>–0.001 (0.014)</td>
<td>0.029 (0.009)</td>
<td>–0.003 (0.009)</td>
</tr>
<tr>
<td>FE</td>
<td>0.008 (0.023)</td>
<td>0.004 (0.026)</td>
<td>0.018 (0.009)</td>
<td></td>
</tr>
</tbody>
</table>

Panel D: Unpaid child care utilization by child’s age and number of children<sup>b</sup>

<p>| | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>One child aged 0–4</td>
<td>0.003 (0.008)</td>
<td>0.005 (0.014)</td>
<td>0.034 (0.013)</td>
<td>0.002 (0.013)</td>
</tr>
<tr>
<td>One child aged 5–10</td>
<td>0.001 (0.011)</td>
<td>–0.002 (0.015)</td>
<td>0.026 (0.027)</td>
<td>–0.04 (0.027)</td>
</tr>
<tr>
<td>Two children or more,</td>
<td>–0.003 (0.018)</td>
<td>–0.004 (0.023)</td>
<td>0.008 (0.023)</td>
<td>–0.001 (0.014)</td>
</tr>
<tr>
<td>youngest aged 0–4</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>FE</td>
<td>0.009 (0.018)</td>
<td>0.009 (0.022)</td>
<td>0.023 (0.022)</td>
<td></td>
</tr>
<tr>
<td>One child aged 0–4</td>
<td>0.006 (0.022)</td>
<td>–0.001 (0.024)</td>
<td>0.019 (0.024)</td>
<td></td>
</tr>
<tr>
<td>Two children or more,</td>
<td>0.008 (0.025)</td>
<td>0.004 (0.018)</td>
<td>0.007 (0.018)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors are shown in parentheses. Estimates in bold are significant at the five percent level. All estimates are obtained from linear probability models on the subsamples of single mothers.

<sup>a</sup> The dependent variable takes value one if the mother works, has at least one child aged 12 or less, and pays for child care arrangements, and zero otherwise.

<sup>b</sup> The dependent variable takes value one if the mother works, has at least one child aged 12 or less, and does not pay for child care arrangements, and zero otherwise.
Table 6: Eligible Employment Transitions (BHPS)

<table>
<thead>
<tr>
<th>Persistence probability(^a)</th>
<th>(1)</th>
<th>(2)</th>
<th>(b)</th>
<th>(b(_0))</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.058</td>
<td>0.070</td>
<td>0.024</td>
<td>6,478</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.033)</td>
<td>(0.008)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Entry probability(^b)</td>
<td>0.035</td>
<td>0.054</td>
<td>0.022</td>
<td>5,429</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.023)</td>
<td>(0.010)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors are shown in parentheses. Estimates in bold are significant at the five percent level. \(N\) denotes the number of wave-on-wave state-specific transitions.

\(^a\) Obtained from linear probability models of transitions in labor market states on the sample of single childless women and lone mothers. Conditional on being in eligible employment in \(t-1\).

\(^b\) Obtained from linear probability models of transitions in labor market states on the sample of single childless women and lone mothers. Conditional on not being in eligible employment in \(t-1\).
Table 7: Transitions in Eligible Employment and Child Care Utilization (BHPS)

<table>
<thead>
<tr>
<th>Type of transition</th>
<th>Treatment $(b)$</th>
<th>Annunciation $(b_0)$</th>
<th>$N$</th>
</tr>
</thead>
<tbody>
<tr>
<td>From nonwork to eligible employment with paid child care$^a$</td>
<td><strong>0.039</strong></td>
<td>−0.001</td>
<td>1,871</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.010)</td>
<td></td>
</tr>
<tr>
<td>From nonwork to eligible employment without paid child care$^a$</td>
<td>0.013</td>
<td><strong>0.021</strong></td>
<td>1,871</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.010)</td>
<td></td>
</tr>
<tr>
<td>From eligible employment without paid child care to eligible employment with paid child care$^b$</td>
<td><strong>0.047</strong></td>
<td>−0.003</td>
<td>2,093</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.023)</td>
<td></td>
</tr>
<tr>
<td>Persistence in eligible employment without paid child care$^b$</td>
<td>0.020</td>
<td><strong>0.025</strong></td>
<td>2,093</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.011)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors are shown in parentheses. Estimates in bold are significant at the five percent level. Estimates are obtained from linear probability models of transitions in labor market states on the subsample of lone mothers with children aged 12 or less. $N$ denotes the number of wave-on-wave state-specific transitions.

$^a$ The nonwork (origin) state includes women who work less than 16 hours per week and (if working) do not use paid child care.

$^b$ Conditions on working 16 or more hours per week in both origin and destination states.
Table 8: Sensitivity Checks: Restricting the BHPS Sample to 1995–2002

<table>
<thead>
<tr>
<th>Selected labor market outcomes</th>
<th>Treatment ($b$)</th>
<th>Announcement ($b_0$)</th>
<th>$N$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Eligible employment</td>
<td><strong>0.038</strong></td>
<td>0.012</td>
<td>10,224</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.032)</td>
<td></td>
</tr>
<tr>
<td>Full time employment</td>
<td><strong>0.033</strong></td>
<td><strong>0.016</strong></td>
<td>10,224</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.008)</td>
<td></td>
</tr>
<tr>
<td>Employment</td>
<td><strong>0.042</strong></td>
<td>0.008</td>
<td>10,224</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.021)</td>
<td></td>
</tr>
<tr>
<td>Hours worked (including zero hours)</td>
<td><strong>3.58</strong></td>
<td><strong>1.88</strong></td>
<td>10,224</td>
</tr>
<tr>
<td></td>
<td>(1.73)</td>
<td>(0.67)</td>
<td></td>
</tr>
<tr>
<td>Persistence probability in eligible employment</td>
<td><strong>0.055</strong></td>
<td>0.016</td>
<td>4,340</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.025)</td>
<td></td>
</tr>
<tr>
<td>Entry probability in eligible employment</td>
<td>0.050</td>
<td>0.019</td>
<td>3,637</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.015)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors are shown in parentheses. Estimates in bold are significant at the five percent level. For details, see Tables 2, 3 and 6 for details.
Table 9: Job Exit Rates for Single Mothers and Single Childless Women (Cumulative Percentage)

<table>
<thead>
<tr>
<th>Job tenure (months)</th>
<th>3</th>
<th>6</th>
<th>12</th>
<th>18</th>
<th>24</th>
<th>36</th>
<th>48</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>BHPS</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lone mothers</td>
<td>7</td>
<td>15</td>
<td>23</td>
<td>34</td>
<td>41</td>
<td>49</td>
<td>57</td>
<td>348</td>
</tr>
<tr>
<td>Single childless women</td>
<td>6</td>
<td>13</td>
<td>22</td>
<td>33</td>
<td>40</td>
<td>48</td>
<td>57</td>
<td>427</td>
</tr>
<tr>
<td>LFS</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lone mothers</td>
<td>8</td>
<td>14</td>
<td>23</td>
<td>32</td>
<td>38</td>
<td>47</td>
<td>55</td>
<td>5,145</td>
</tr>
<tr>
<td>Single childless women</td>
<td>11</td>
<td>19</td>
<td>30</td>
<td>41</td>
<td>47</td>
<td>58</td>
<td>63</td>
<td>16,426</td>
</tr>
</tbody>
</table>

*Notes: Figures are Kaplan-Meier estimates of job duration for jobs started in March 1997 and observed up to September 1999 in the case of the BHPS, and over the period between January 1997 and September 1999 in the case of the Labour Force Survey (LFS). N denotes the number of women.*
## Appendix Table A1: Summary Statistics by Sample

<table>
<thead>
<tr>
<th>Outcomes</th>
<th>BHPS</th>
<th>FRS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Unmarried women</td>
<td>Lone mothers</td>
</tr>
<tr>
<td>Eligible employment(^a)</td>
<td>0.635</td>
<td>0.438</td>
</tr>
<tr>
<td>Employment(^b)</td>
<td>0.721</td>
<td>0.622</td>
</tr>
<tr>
<td>Full time employment(^c)</td>
<td>0.526</td>
<td>0.270</td>
</tr>
<tr>
<td>Weekly hours worked</td>
<td>25.6</td>
<td>15.1</td>
</tr>
<tr>
<td>(including zeros)</td>
<td>(16.7)</td>
<td>(13.2)</td>
</tr>
<tr>
<td>Weekly hours worked(^d)</td>
<td>32.7</td>
<td>24.1</td>
</tr>
<tr>
<td>(conditional on work)</td>
<td>(16.7)</td>
<td>(13.2)</td>
</tr>
<tr>
<td>Transition probabilities in eligible employment:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Persistence probability</td>
<td>0.903</td>
<td>0.644</td>
</tr>
<tr>
<td>Entry probability</td>
<td>0.262</td>
<td>0.191</td>
</tr>
<tr>
<td>Paid child care utilization</td>
<td>0.134</td>
<td>0.142</td>
</tr>
</tbody>
</table>

### Main explanatory variables

| Age (years)                       | 33.096 (13.510)        | 30.728 (11.412)        | 34.339 (13.340)  | 33.951 (8.262) |
| Ethnic origin:                    |                        |                        |                 |              |
| White                             | 0.956                  | 0.914                  | 0.931           | 0.911        |
| Black                             | 0.022                  | 0.038                  | 0.032           | 0.050        |
| Indian                            | 0.007                  | 0.022                  | 0.010           | 0.008        |
| Pakistani/Bangladeshi             | 0.003                  | 0.014                  | 0.012           | 0.011        |
| Chinese or other                  | 0.012                  | 0.012                  | 0.015           | 0.020        |
| Education:                        |                        |                        |                 |              |
| No qualification                 | 0.172                  | 0.176                  |                 |              |
| Less than O level/GCSE            | 0.081                  | 0.120                  |                 |              |
| O level/GCSE (or equivalent)      | 0.213                  | 0.343                  |                 |              |
| A level (or equivalent)           | 0.194                  | 0.137                  |                 |              |
| Higher vocational qualification   | 0.192                  | 0.168                  |                 |              |
| University degree or more         | 0.149                  | 0.056                  | 0.700           | 0.893        |
| Left education at age 18          |                        |                        |                 |              |
| Number of children by age group:  |                        |                        |                 |              |
| 0–4                               | 0.389                  |                         | 0.524           |              |
|                                   | (0.512)                |                         | (0.672)         |              |
| 5–10                              | 0.587                  |                         | 0.624           |              |
|                                   | (0.754)                |                         | (0.757)         |              |
| 11–18                             | 0.771                  | 0.771                  | 0.598           |              |
|                                   | (0.754)                |                         | (0.790)         |              |
| Housing tenure:                   |                        |                        |                 |              |
| Owner                             | 0.597                  | 0.582                  | 0.624           | 0.468        |
| In social housing                 | 0.185                  | 0.345                  | 0.163           | 0.431        |
| In privately rented accommodation | 0.212                  | 0.073                  | 0.213           | 0.101        |
| Number of person-wave observations| 9,644                  | 5,616                  |                 |              |
| Number of women                   | 1,868                  | 1,606                  | 48,418          | 28,468       |

**Sources:** British Household Panel Survey (BHPS), 1991-2002; Family Resources Survey (FRS), 1995/96-2002/03.

**Notes:** Figures are sample means (standard deviations are shown in parentheses).

\(^a\) Working 16 or more hours per week.

\(^b\) Working 1 or more hours per week.

\(^c\) Working 30 or more hours per week.

\(^d\) Number of person-wave observations are 10,446 in the BHPS sample (6,953 for childless women and 3,493 for mothers) and 52,483 in the FRS sample (35,345 for childless women and 17,138 for single mothers).