ISSN 1755-5361

University of Essex

Department of Economics

Discussion Paper Series

No. 698 September 2011

Anatomy of Welfare Reform Evaluation: Announcement and Implementation Effects

Richard Blundell, Marco Francesconi and Wilbert van der Klaauw

Anatomy of Welfare Reform Evaluation: Announcement and Implementation Effects*

Richard Blundell
University College London, and
Institute for Fiscal Studies

Marco Francesconi
University of Essex, and
Institute for Fiscal Studies

Wilbert van der Klaauw
Federal Reserve Bank of New York

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 implementation effects. We explore several mechanisms through which women can respond to the announcement of a reform that increases in-work benefits, including sources of intertemporal substitution, human capital accumulation, and labor market frictions. Using the model's insights and information of the precise timing of the announcement and implementation of a major UK in-work benefit reform, we estimate its effects on single mothers' behavior. We find large and positive announcement effects on employment decisions. We show that this finding is consistent with the presence of frictions in the labor market. The impact evaluations of this reform which ignore such effects produce implementation effect estimates that are biased downwards by 15 to 35 percent.

Keywords: Policy evaluation; In-work benefit; Anticipation effects; Labor market frictions; Intertemporal substitution

JEL Classifications: D10; J13; J22

September 2011

* We would like to thank Orley Ashenfelter, Gary Becker, Martin Browning, David Card, Julie Cullen, Jeff Grogger, John Ham, Lars Hansen, James Heckman, David Jaeger, Patrick Kline, Shelly Lundberg, Bruce Meyer, Enrico Moretti, Michele Pellizzari, Craig Riddell, John Rust, Bernard Salanié, Chris Taber, Michèle Tertilt, Gerard van den Berg, Ken Wolpin, and seminar participants at Aarhus, Alicante, Bergen, Berkeley, Chicago, Columbia, Essex, Federal Reserve Bank of New York, Georgetown, IZA (Bonn), Linz, Mannheim, Maryland, Nuremberg, Ohio State, Oxford, Turin, and UCL for helpful comments. Blundell and Francesconi thank the ESRC Centre for the Microeconomic Analysis of Public Policy at IFS, and Francesconi also thanks the ESRC Centre for Micro-social Change at Essex. The views expressed in this paper are those of the authors and do not necessarily reflect those of the Federal Reserve Bank of New York or the Federal Reserve System as a whole.

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 has been extensive work documenting how economic agents adjust their behavior in anticipation of a variety of alterations to their environment, there has been relatively little research on anticipatory responses by individuals to welfare reforms. The common approach in the empirical evaluation literature instead has been to assume either that the implementation of a reform comes as a complete surprise, or that there is little or no scope or incentive for agents to respond to information or beliefs about a possible reform in advance of its implementation. The goal of this paper is to analyze the potential nature of such anticipatory responses, in the context of a change in working tax credits. Under what conditions and how would women adjust their labor supply behavior in anticipation of, and in response to, announcements about welfare reform? How would anticipation and announcement effects influence the evaluation of the impacts of welfare reform?

To help answer these questions, we formulate a simple model of female labor force participation decisions which embeds a basic in-work benefit reform and explicitly allows for announcement and anticipation effects. We describe several mechanisms through which women's work behavior can respond to the announcement of an in-work benefit reform that permanently increases their earnings provided that they work. On the one hand, intertemporal substitution effects through preferences and saving would lead to a labor supply reduction between the announcement and the implementation of the reform. For example, an increasing disutility of working would cause forward-looking women who anticipate the introduction of the reform to prefer a withdrawal from the labor market today and a later entry into the market when they can reap the monetary benefits offered by the reform. On the other hand, labor market frictions, human capital formation and habit persistence could lead women to increase their labor supply in response to the announcement of a future increase in earnings tax credits. For instance, in the presence of labor market frictions where job availability is not guaranteed, women have an incentive to enter or remain in the labor market after the announcement, so that they are in a position to collect the in-work benefit when the reform is implemented.

We next apply this analysis to examine whether and how a specific targeted group of individuals (single mothers) may have changed their labor market choices in anticipation of the introduction of a major tax reform in the UK. More specifically, we study the effects of the announcement and the implementation of the Working Families' Tax Credit (WFTC) reform on single mothers' employment. 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. In addition to a large implementation effect, we find evidence of a significant positive announcement effect on labor supply, measured between the time WFTC was announced and its actual implementation. We are able to document the timing of the announcement quite precisely according to the Budget speech and media coverage. Both the announcement effect and the implementation effect emerge for several labor market outcomes, and are especially strong for mothers of children of preschool and primary school age for whom the tax credit increase was in fact particularly large. We interpret the large anticipatory employment increase as a response to significant labor market frictions.

We also find strong announcement effects along the informal (unpaid) child care utilization margin, and not along the paid child care 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 child care services were relatively expensive and the pre-WFTC in-work support was not particularly generous towards child care 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 found temporary child care arrangements for their children before placing them in formal daycare centers (or other formal child care arrangements) after gaining eligibility to WFTC's substantial child care tax credit top-up.

The paper continues with a brief discussion of the relevant literature on anticipatory economic behavior, with a focus on tax and welfare reforms. In Section II a labor supply model is formulated and simulated, and its key econometric implications for public policy program evaluation are discussed. Section III describes the Working Families' Tax Credit reform, the data used in estimation as well as the identification strategy for recovering the announcement and implementation effect parameters. Section IV presents the main empirical results on labor market outcomes and provides an economic interpretation based on the model of Section II. Section V shows the reform's impact on child care utilization, examines transitions along labor market and child care use margins, and discusses a number of sensitivity tests. Section VI concludes with a discussion of broader implications for the evaluation of policy reforms and highlights the need for collecting new data on agents' knowledge and subjective expectations regarding the likelihood and nature of future social interventions.

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, 2008). Mertens and Ravn (2010) provide empirical evidence on the aggregate effects of anticipated and unanticipated U.S. tax policy shocks. They find that both types of shocks have contributed to the business cycle, and associate anticipated tax cuts with significant pre-implementation changes in output, investment, and hours worked. Examples of tax reform studies at the micro level include evaluations of the impact of announced changes in corporate income taxes on firms' dividend and investment policies (Kari, Karikallio, and Pirttilä, 2008) and the effect of tax rebate announcements on consumer spending (Heim, 2007).[1] Relatively few studies have investigated anticipation and announcement effects associated with welfare and tax credit reforms. As a consequence, little is known about whether and how forward-looking individuals alter their labor supply behavior in anticipation of new policies.

Clearly, to understand observed changes in outcomes following a reform, it is important to know the extent to which the reform was expected. When comparing outcomes just before and after implementation of a policy change, a lack of a behavioral response does

[1]Other examples of anticipatory behaviors in response to tax and benefit changes include Auerbach and Siegel (2000), who 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.

not necessarily imply that the change was ineffective, as the policy change may have been fully anticipated. Moffitt (1987) attributes the absence of a jump in labor supply following social security reforms to the fact that they were fully anticipated.

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), and in response to a shorter benefit duration (Card, Chetty, and Weber, 2007), while Grogger and Michalopoulos (2003) find significant effects of time limits on welfare receipt on welfare participation. Attanasio and Rohwedder (2003) find some anticipatory impact of social security announcements on consumption behavior of workers.[2]

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 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.[3]

Despite having witnessed a massive introduction of welfare and tax reforms around the world during the past twenty years, it remains common practice in evaluating reform impacts to assume away anticipatory behavioral responses. Each new reform is treated as if entirely unanticipated (anticipated with zero probability), which, if untrue, may generate

[2] A study by the Council of Economic Advisers (1997) examined the effect of waiver activity in the early 1990s on welfare caseload, and found 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).

[3] In their evaluation of Progresa — a major social program in Mexico that provided generous conditional cash transfers to parents of children who attended school and lived in treatment villages — Attanasio, Meghir, and Santiago (forthcoming), instead, find no evidence of anticipation effects on villages not initially selected for program eligibility. Two mechanisms have been proposed to explain this result. First, although the program was scheduled to be later extended to control villages, parents in the control group might have been unable to take advantage of this knowledge because they were liquidity constrained. Second, families in control villages may in fact have been unaware or uncertain of future eligibility to the program, as there was no explicit, public announcement about the future availability of the grants.

biased impact estimates. In part, this practice may reflect the complications induced by anticipation effects for econometric analysis. 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, requiring potential outcomes to be unaffected by agent actions in response to different predictions of future treatments and outcomes (see also Heckman and Vytlacil, 2007).

An important implication of this work for reduced-form or treatment effects approaches is that valid inference requires an ability to condition on agent information sets, including the perceived likelihood of, and 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 comparable abilities and motivations 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. Crépon et al. (2010) reject the no-anticipation assumption in their study of French training programs for the unemployed and show that, with data on the date of information notification, the causal effects of notification and of the treatment on the outcome are identified.

An attractive feature of models with forward looking behavior 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 an optimal stopping model in which individuals sequentially decide the age at which to stop schooling and can learn about measured and unmeasured variables that affect expectations of future outcomes. Their model is suitable for the analysis of outcomes associated with different times to treatment (including both anticipatory and implementation effects) without imposing the no-anticipation condition invoked by Abbring and van den Berg (2003).

Another example that incorporates expectations of a future reform is the study by van der Klaauw and Wolpin (2008). They estimate the impact of social security reforms on savings and labor supply decisions, and to help identify the perceived risk of a reform they use self-reported subjective expectations data on future social security benefits individuals expect to receive.[4] 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. Simulation results 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.

Despite these important advances, it should be pointed out that while allowing for uncertainty about the occurrence and timing of future policy implementations, none of the current 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 lead to anticipatory behaviors. As the model in the next section illustrates, the interplay and timing of policy announcements, individuals' expectations, and the actual implementation of a reform are all key elements which jointly determine the eventual total impact of any reform.

II. A Model of Female Labor Supply with Welfare Reform and Pre-Implementation Effects

A. Setup

We illustrate our key insights regarding the role and implications of pre-implementation anticipation and announcement effects for welfare policy evaluation research using a simple model of female labor supply.

Consider a three-period economy in which each woman i chooses in each period t whether to work ($y_{it} = 1$) or not ($y_{it} = 0$) and how much to consume (c_{it}).[5] In each period $t = 1, 2, 3$, a woman's objective is to maximize the expected present value of her remaining lifetime utility

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

[4] See also Dominitz, Manski, and Heinz (2003) for evidence showing strong consumer expectations of a future decline in the generosity of the social security benefit program.

[5] Although the model could be easily extended to more periods, this extension would not add further insights, while the model's salient features can be fully shown in this three-period formulation.

with respect to y_{it} and c_{it}. In (1), X_{it-1} denotes the number of periods the woman has worked prior to period t (and, without loss of generality, X_{i0} is set equal to zero), δ is the subjective discount factor, $\mathrm{E}[\cdot]$ is the mathematical expectation operator, and Ω_{it} is the individual's information set at time t. The latter includes information the woman has regarding the possible implementation of a future policy reform, which we discuss in detail below. The law of motion for work experience, X_{it} is given by

$$X_{it} = X_{it-1} + y_{it}, \tag{2}$$

and end-of-period assets, A_{it}, evolve according to

$$A_{it} = (1+r)A_{it-1} + w_{it}y_{it} + N_{it} - c_{it}, \tag{3}$$

where r is the real interest rate, w_{it} represents woman i's potential earnings, and N_{it} is her exogenous nonlabor income. Choices are subject to a nonnegativity constraint on net assets requiring $A_{is} \geq 0$, $s = 1, 2, 3$, and a lifetime resource constraint

$$\sum_{t=1}^{3} \left(\frac{1}{1+r}\right)^{t} c_{it} = \sum_{t=1}^{3} \left(\frac{1}{1+r}\right)^{t} (w_{it}y_{it} + N_{it}), \tag{4}$$

for which we assume $A_0 = A_4 = 0$.[6]

Potential earnings are stochastic and depend on previous work experience. In particular,

$$\log(w_{it}) = w_0 + \alpha X_{it-1} + \beta d_t \mathrm{I}(t \geq 2) y_{it} + \epsilon_{it}, \tag{5}$$

where the parameter α measures the returns to work experience, $\mathrm{I}(z)$ is an indicator function that is equal to one if z occurs and zero otherwise, and ϵ_{it} is a technology shock which captures random fluctuations in earnings that are independent of the individual decision process. We assume that ϵ_{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 Ω_{it}, individuals form beliefs about the likelihood that the reform

[6]In the analysis below, we will also consider a model without saving, where the period-by-period budget constraint equals $c_{it} = w_{it}y_{it} + N_{it}$.

will be introduced 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 $\pi_{t3}(d_2) = \Pr(d_3 = 1|\Omega_t, d_2)$, where $\pi_{t3}(1) = 1$.

The parameter β in (5) encapsulates the benefit of the reform. The reform gives each woman a permanent shift in log-earnings, β, provided that the woman works ($y_{it} = 1$). For simplicity, the log-earnings shift is independent of prior work experience and does not depend on a minimum number of weekly hours worked. Both such features could be added to the model, but they would not change its main insights.

Per period utility derived from consumption and work effort is specified as follows:

$$U_{it} = (1 + \gamma_3 y_{it}) \log(c_{it}) + (\gamma_1 + \gamma_2 X_{it-1}) y_{it}. \tag{6}$$

In (6), 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 γ_2 may be interpreted as habit formation in the labor market, whereas a negative value would capture 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.

Finally, women take decisions in a labor market environment that may include frictions. Labor market imperfections are reflected in the choice set available. Specifically, $y_{it} \in J_{it}$, where J_{it} denotes the work choice set available to woman i in period t, and this is equal to $\{0\}$ (that is, no job is available) with probability $(1 - \lambda_t)$ and to $\{0, 1\}$ (that is, the choice set includes both 'not working' and 'working') with probability λ_t. 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 job 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 and use its solution to simulate choice decisions of women under a number of different model specifications.[7] In the benchmark case, the following parameter values are used: $\delta = 0.95, r = 0.05, N_{it} = 0.5, w_0 = 0.2$, $\alpha = 0$, $\beta = 0.45$, $\gamma_1 = -1.4, \gamma_2 = \gamma_3 = 0$ and $\lambda_t(0) = 1$. In this model, therefore, there are no job search frictions ($\lambda_t(0) = 1$), utility is time separable ($\gamma_2 = 0$), and there is no return to human capital ($\alpha = 0$). Moreover, in this benchmark model we assume no saving, with individuals in each period facing the period-by-period budget constraint $c_{it} = w_{it}y_{it} + N_{it}$.

We then analyze a set of alternative model specifications, changing one feature of the benchmark case separately each time. First, to capture the role of labor market frictions, we consider a case where the job offer arrival rate when previously unemployed is less than unity, $\lambda_t(0) = 0.5$. Second, we analyze a model with human capital accumulation, with wages depending on work experience ($\alpha = 0.25$). Third, we assess the role of non-time-separable preferences by looking at a case of habit persistence, where $\gamma_2 = 0.25$. Fourth, we consider the role of intertemporal substitution with disutility of working increasing in past work experience ($\gamma_2 = -1.5$). Fifth, we consider a version of the benchmark model that allows for saving behavior, whereby women face the lifetime resource constraint given by (4).

For each of these alternative model specifications, we then assess the impact of several reform scenarios. To ease interpretation, all impacts on employment choices will be shown relative to a baseline scenario in which there is no reform and in which the possibility of a reform is never envisaged by women. Instead, in all but one reform scenarios we consider, a reform is actually implemented and/or anticipated or announced. For simplicity, we will assume in these scenarios that in period 1 women 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 are denoted by $\pi_{23}(0) = \Pr(d_3 = 1|d_2 = 0, \Omega_2) = \pi_2$.

As in the baseline scenario, in reform scenario (i), we assume $\pi_1 = \pi_2 = 0$. Thus, no reform is anticipated or announced, but unlike the baseline, the reform is actually implemented in period 3 ($d_3 = 1$). This is the case of an unannounced and unanticipated reform, the sort of ideal scenario analysts have in mind in reform evaluations. In scenario (ii), individuals again rule out the possibility of a future reform in period 1, i.e., $\pi_1 = 0$. But an announcement in period 2 that the reform will be introduced in period 3 changes

[7] The model solution is presented in Appendix A.

women's beliefs entirely, implying $\pi_2 = 1$. This is, therefore, a case where an unanticipated announcement in period 2 will be part of the individual's information set at $t = 2$. In scenario (iii), in which we assume $\pi_1 = 0.5$ and $\pi_2 = 1$, women in period 1 assign a 50 percent chance that the reform will be introduced in period 2 as well as a 50 percent chance that the reform will be implemented in period 3 if it was not already implemented in period 2; while the implementation of the reform in period 3 is announced in period 2 (hence, the updated belief π_2 is greater than the prior π_1). Finally, in scenario (iv), we have an announcement — as in scenario (ii) — of a completely unanticipated next-period reform in period 2, but in period 3 the reform fails to materialize.

Starting with the simulations for the benchmark model with no search frictions, time-separable utility, no saving and no human capital accumulation, Figure 1 shows that the only predicted employment change occurs in period 3 and coincides with the implementation of the reform in that period. Pre-reform information is immaterial: neither anticipation nor announcement of the reform affects employment choices in earlier periods, indicating that there is no incentive to change behavior in the benchmark model. In this environment, individuals act the same as if they were myopic.

In the case with search frictions, Figure 2 shows that while we again see large employment increases in the third period coinciding with the implementation of the reform, there are now also increased gains from working in the first two periods as doing so guarantees the option to work in a subsequent period. Both the anticipation of a possible future reform in period 1 as well as an announcement in period 2 lead to increases in the employment rate in pre-implementation periods. These increases in turn contribute to a greater overall employment increase in period 3 (relative to scenario (i)) when the reform is implemented. In case of scenario (iv), this pre-implementation knowledge actually leads to a (small) employment increase in period 3 even though no actual reform materializes. As the figures make clear, in the presence of anticipation effects, both the eventual implementation of the reform *and* its absence can affect behavior.[8]

The presence of human capital effects (Figure 3) generates qualitatively very similar changes in employment. The anticipation or announcement of a future reform causes

[8]In an environment in which policy makers engage in repeated interactions with economic agents, it is important to assess the credibility of reform announcements that are systematically unfulfilled. The development of the political economy considerations associated with this issue, however, is beyond the scope of our paper. For a related discussion on modeling the political economy of policy choice, see Besley and Case (2000)).

individuals to increase their participation in the labor market prior to the actual reform because the latter increases the expected future wage return to work experience. These gains are especially large in the first period, as reflected in the large employment increase in that period for scenario (iii). Moreover, pre-reform increases in employment actually contribute to a larger overall employment increase (compared to the unanticipated reform in scenario (i)) in the implementation period 3.

In Figures 4 and 5 we explore two kinds of time non-separable preferences: one in which the disutility of working declines with work experience, a form of habit persistence, and one in which the disutility of working instead increases with work experience. As shown in Figure 4, in the presence of habit persistence, agents who anticipate or learn about the future implementation of a reform that increases net wages start working more in earlier periods, as doing so will increase the utility received from working once the reform has been implemented. As was the case for the model with human capital accumulation, the gains from, and the resulting increase in, employment in period 1 are especially large.

While Figures 2–4 exhibit anticipatory behavior leading to increases in pre-reform employment, Figures 5 and 6 instead feature responses from models that generate pre-reform employment reductions. The case of intertemporal substitution due to non-separability of preferences where the disutility of working increases with work experience is shown in Figure 5. 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 3. Similarly, the anticipation in period 1 of a possible future reform leads to a lower employment rate in that period. As was the case for the previous model specifications, pre-reform employment responses contribute to an overall larger employment increase (relative to the unanticipated reform of scenario (i)) in period 3. In case of scenario (iv), pre-reform knowledge still generates a (small) employment increase in period 3 even though the reform is not actually implemented.

Finally, Figure 6 considers the same no-search-frictions, time-separable utility and no-human-capital-accumulation model associated with Figure 1, while now allowing for saving behavior (but no borrowing). Saving generates employment responses in the various reform scenarios that are qualitatively very similar to those shown in Figure 5. Intertemporal substitution of leisure again causes agents to reduce their pre-reform employment in response to an anticipated future increase in labor supply in period 3 (when their wages are higher).

This anticipatory behavior is further associated with a greater eventual employment increase in the period the reform is implemented.

C. Econometric Implications and Identification Issues

As illustrated in Figures 1–6, when a reform is announced or when there is some anticipation of its possible implementation, individual behavior can be affected 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 (ii)), the employment rates in periods 2 and 3 could be compared to those in the baseline scenario (i.e., the differences shown in Figures 1–6) to obtain estimates of the announcement effect and the implementation effect of the pre-announced reform, which together characterize its overall impact.[9]

In analyses using differences-in-differences (DD) methods, the baseline (counterfactual) scenario is typically approximated by the experiences of a control group. Thus, employment rates in the baseline scenario are estimated using the employment rates of a comparison group, consisting of otherwise similar individuals who are not eligible for, or 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 the similar comparison for the period 3 versus period 1 differences will instead estimate the pre-announced program's implementation effect. These comparisons will serve as estimates of the differences shown in Figures 1–6. Clearly, DD analyses based on pre- and post-implementation comparisons of employment rates that contrast period 3 with either just period 2, or with periods 1 and 2 combined will generally lead to inaccurate inferences regarding the reform's overall effect. Forward looking behavior in a world with, for example, search frictions could then lead to underestimation of the reform's impact, while with saving or non-time-separable utility could lead to overestimation of the true overall causal effect.

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

[9]Note that this implementation effect of the pre-announced reform corresponds to the change in the employment rate in period 3 relative to the baseline scenario in which no reform is actually implemented, anticipated or announced. It measures the combined effect of the announcement and implementation.

implementation. Were there discussions and/or formal announcements of possible reforms in the periods leading up to their actual implementation? Was there scope and a potential benefit for agents to act on this information? 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 overall impact. 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, and especially on *when* it was proposed, passed, and implemented.

While in many cases it may be reasonable to assume that a reform was unanticipated before it was announced or implemented, or at least that π_1 was very small, in other cases this seems less reasonable. For instance, as mentioned earlier, while there may be uncertainty about the exact *timing* and *specifics* of a benefit reform, many individuals report in surveys that they anticipate a reform that will reduce their future social security retirement benefits (Dominitz, Manski, and Heinz, 2003). In the presence of anticipation effects, such as described in scenarios (iii) and (iv), 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 is unexpectedly implemented or unexpectedly announced, it seems appropriate to define the counterfactual outcomes to be those that would have occurred in the same world had the reform or announcement never occurred. However, in a world in which people anticipate the possibility of a future reform, instead of considering an environment in which reforms never occur, a more reasonable counterfactual would be a world in which a reform may occur in the future, but has not been announced or implemented yet. That is, the counterfactual outcomes are the outcomes that would have occurred without an announcement in period 2 and without the implementation in period 3. As it is clear from comparing scenarios (iii) and (iv), the non-occurrence of a pre-announced reform in such a world can be an event that directly affects behavior itself.

Second, in a world in which individuals consider the possibility of future reforms, the requirements for a valid control group in DD type evaluations become more stringent. Not only do control and treatment group members must have comparable characteristics and backgrounds, but they also need to have had comparable knowledge, beliefs, and expectations about future reforms and about future decision environments more generally. In scenarios (iii) and (iv), then, 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. For the observed outcomes in periods 1 and 2 of control group members to be suitable proxies for what outcomes would have been without the announcement and/or implementation of the reform, the corresponding control group should have had comparable characteristics and expectations, but were not eligible and/or subjected to the reform or its announcement.[10]

III. Application: The WFTC Reform

A. Overview of the Reform and Its Announcement

Our application investigates the introduction of the Working Families' Tax Credit (WFTC), a major in-work benefit reform introduced in Britain in October 1999. We focus on its impact on single mothers, a primary target group of the reform.[11] 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.

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

[10] Note that this also requires an absence of general equilibrium and spillover effects.

[11] While some working married couples with children also benefitted from the WFTC reform, Francesconi, Rainer and van der Klaauw (2009) show that the tax credit and its corresponding labor supply effects were more modest for this sub-population.

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 the first significant positive growth in house prices 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 on the 18th of 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.

WFTC dominated the Budget speech in the Commons and, together with the New Deals for the unemployed, it represented a prominent feature of the new welfare-to-work architecture. Other benefits supporting families with young children, which were also scheduled 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.

Importantly, the March 1998 announcement was truthful. Table 1 shows how the key parameters of the tax credit actually changed between the baseline FC year (i.e., 1998) and the new WFTC regime (1999) and how they differed from the time they were announced in the 1998 Budget speech and the time WFTC was implemented in October 1999. Of the eight parameters listed in the table, six exhibit either no or a negligible nominal difference between implementation and announcement. The differences virtually disappear if the announced values are corrected to account for inflation (see the values in square brackets in column (4)). For the two parameters with a more sizeable variation (i.e., the basic rate and the credit for children aged 0–10), the gaps between implementation and announcement cannot be attributed to inflation adjustments only, with the actual values being slightly larger (more generous) than those announced 18 months earlier.

The 1998 Budget received phenomenal media coverage. 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.[12] A content analysis of four major tabloid newspapers (The Sun, The Daily Mirror, The Daily Express, and The Daily Star), two main broadsheet papers (The Times and The Daily Telegraph) and the BBC's Online News Service shows at least 250 stories on the announcement of the new tax credit reform published during the course of 1998. Almost three-quarters of these came out between February and April 1998. According to data from the British Household Panel Survey (BHPS), approximately 52 percent of single mothers read a newspaper regularly and, after controlling for a standard set of socio-demographic variables, single mothers' likelihood of reading a daily newspaper was not statistically different from all other women's (including single childless women).[13] This evidence provides only an indication that single mothers received payoff-relevant information around the time of the announcement of the reform, much before its introduction. Clearly, they could have relied not just on newspapers but also on television and radio as well as on social interactions with relatives, friends and neighbors, for which reliable data are not available.

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

[12]See <http://archive.treasury.gov.uk/budget/1998/newsindx.htm> and <http://www.dwp.gov.uk/publications/electronic-archive/press-releases/>.

[13]Over the pre-reform period, the BHPS collected information on newspaper readership only in the first two waves (1991 and 1992) and in waves 6 and 7 (1996 and 1997). The results mentioned here use data from the 1997 wave only, but are robust to inclusion of the other three sweeps of data.

program was more generous than FC in four important ways (see columns (1) and (3) in Table 1): it had higher credits, particularly those for young children; it reduced net child care costs; 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 four 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).

Comparing the implementation parameters in column (3) of Table 1 to the corresponding baseline parameters in column (1) gives an indication of the increased generosity of the WFTC regime. Figure 7 provides another illustration of this greater generosity.[14] In absence of child care subsidies, we observe a gradual increase in benefits with higher hours of work levels. If the mother received Housing Benefit (a rent subsidy), the rate of increase was somewhat slower than that shown in the figure, due to the fact that the tax credit was treated as income in other means-tested programs. But the main features remain the same, with the greatest increases in benefits falling to those in full-time employment, many of whom would not have been eligible for a tax credit before the reform. Depending on the amount of child care expenditures, the child care component of the tax credit could have represented a considerable increase in generosity of the in-work benefit program, beyond that associated with the reduced earnings tax rate (taper rate) and increased earnings disregard (threshold). This is illustrated in panel (a), which also shows the benefit schedule under WFTC in the case the child care component had been computed as it was under FC, that is, as an earnings disregard.

To assess the overall work incentives associated with the reform, Figure 7(b) shows the mother's budget constraint in the case where she used paid child care.[15] The reform unambiguously improved the financial incentive to take on eligible employment, and especially full-time employment. The effect on hours of work for those already in eligible employment was ambiguous, depending on the relative magnitude of income and substitution effects

[14]Since the increased credit for children aged 0–10 was accompanied by a equivalent increase for mothers who worked fewer than 16 hours (or did not work at all) and received Income Support, Figure 7 ignores this component to focus on the main work incentive effects of the program.

[15]As in Figure 7(b), FC benefits and income at hours below 16 were calculated based on the higher basic child credit rate under WFTC.

for this group. Similarly, the child care tax credit, receipt of which was conditional on eligible employment, had an unambiguous positive effect on labor force participation and an ambiguous effect on hours for those in eligible work.

In sum, both news announcements and salience of the WFTC 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.

It is important to stress again that the WFTC reform was 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.[16] As emphasized in Francesconi and van der Klaauw (2007), these other reforms were relevant to *all* women, and not just single mothers. But, even though none targeted only single mothers, a number of possible interactions between WFTC and other policy initiatives might have occurred. While 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. To do this, we use single childless women (who were not eligible for WFTC benefits) as our control group.

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.[17] 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 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 single women, of whom 1,606 are lone mothers at any point during

[16] For a thorough description of such initiatives, see Card, Blundell, and Freeman (2004) and Brewer et al. (2009).

[17] 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/>.

the survey period 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.[18] 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 pooled sample has 76,886 women, 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 single women with and without children. 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 are 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 child care utilization) is only relevant for single mothers.

Figure 8 plots the time trends of eligible employment over the sample period using the

[18]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/>.

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 8(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 as well as to the model simulations and econometric implications discussed in Section II,[19] let ℓ_{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

$$\begin{aligned} y_{it} &= \psi_1 + \psi_2 \ell_{it} + (\psi_{31} + \psi_{32} \ell_{it})t + [\psi_{41} + \psi_{42}(t-s)]\,\mathrm{I}(t \geq 1999) \\ &\quad + \phi_1\ \ell_{it} \mathrm{I}(t \geq 1999) + \phi_0 \ell_{i\tau} \mathrm{I}(t = 1998) + \mathbf{W}'_{it}\vartheta + \mu_i + \varepsilon_{it}, \end{aligned} \tag{7}$$

where t varies from 1991 to 2002 for the BHPS sample and from 1995 to 2002 for the FRS sample, $\mathbf{W}_{it}$ is a vector of individual characteristics, μ_i represents unobserved time-invariant fixed effects (only included in the BHPS sample), and ε_{it} is an i.i.d. error term, with $\mathrm{E}(\varepsilon_{it}|\mathbf{W}_{it}, \ell_{it}, \mu_i) = 0$.

Equation (7) allows for different intercepts (when $\psi_2 \neq 0$) and different pre-reform linear trends (when $\psi_{32} \neq 0$) for control (single women without children) and treatment groups

[19] While an important topic for future research, estimating a full life-cycle version of the model in Section II that also incorporates the tax structure and benefit schedules facing single women during the period of study is beyond the scope of the current paper.

(single mothers). The parameters ψ_{41} and ψ_{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 influenced 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 ϕ_1. Finally, 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. This *announcement effect* is captured in (7) by ϕ_0. Consequently, ϕ_1 represents the *implementation effect* of the pre-announced reform, as previously discussed in Section II.C.[20]

Note that while allowing explicitly for announcement effects, our analysis assumes that in years prior to 1998 single mothers had similar expectations about a future reform as single childless women. If single mothers assigned a greater likelihood to a future reform in prior years, then our results in Section II imply that estimates based on (7) may yield biased estimates. Our earlier simulations suggest that such anticipatory effects would generally be in the same direction as the announcement effect. Thus, there is a slight potential for the estimated impacts presented below to be biased towards zero.

IV. Evidence

A. Labor Market Outcomes

Table 2 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

[20] It is worth noting that, because of the different fieldwork coverage of the two surveys, the announcement period in the BHPS is different from that in the FRS. In the former survey, it covers the period September–December 1998, while in the latter, it refers to the period between October 1998 and September 1999. Restricting the measurement of the announcement effect to the September–December 1998 period in the FRS sample (thus dropping all the individuals whose information was collected between January and September 1999) leads to a smaller sample size and larger standard errors around ϕ_0. But the 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 ϕ_0 that are close to those shown in the next section and, usually, with smaller standard errors.

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 (i.e., ϕ_0 is set equal to zero), while column (2) shows both implementation effect and announcement effect estimates.

The implementation effect 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.[21] 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 implementation 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, 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 sizeable announcement effect.

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 Hoynes, 2004; Francesconi and van der Klaauw, 2007). Column (1) in the top panel of Table 3 confirms this evidence for both BHPS and FRS samples. When we allow for announcement effects (column (2)), the findings of Table 2 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,

[21]Findings reported later in Section V provide a possible explanation for the size difference between the BHPS- and FRS-based estimates.

while the lower bound emerges in the FE model applied to the BHPS sample and in the FRS sample. Notice further that all such announcement effect estimates are statistically significant and positive.[22]

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 3 indicate that accounting for announcement effects 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 estimate of ϕ_0 is statistically significant and large, ranging between 2 and 2.5 extra hours of work per week and representing 50 percent of the implementation effect. Restricting the focus only to women with positive hours of work, however, changes our results (panel D): the increase in the estimated implementation effect is more modest (especially in the BHPS sample), while the announcement effect is small and always insignificant, which is consistent with the theoretically ambiguous effect predicted for this group of women.

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 child care tax credit) under WFTC was especially large for mothers of young children. The estimates in column (1) of Table 4 uphold 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 implementation 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 statistically significant increase in the eligible employment rate of mothers of children aged 0–10. This

[22]Similar 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.

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 single mothers with pre-school children, who indeed were expected to benefit the most from the reform.

B. Interpretation

According to the model of Section II, the direction and magnitude of anticipation and announcement effects on employment of single mothers depend on the relative importance of search frictions, human capital accumulation, habit persistence, and intertemporal substitution due to preferences and saving. Our empirical results reveal a large positive announcement effect on the employment of single mothers, especially those with young children. This finding implies that the factors contributing to a positive effect (i.e., search frictions, returns to human capital, and habit persistence) dominated those that would have led to a negative impact (i.e., intertemporal substitution and saving).

Micro-based empirical work has long documented, and provided evidence that is consistent with, relatively small intertemporal labor supply elasticities (e.g., MaCurdy, 1981; Ashenfelter, 1984; Altonji, 1986; Blundell and MaCurdy (1999), Ham and Reilly, 2002; French, 2004; and the meta-analysis by Chetty et al., 2011a).[23] Even were the intertemporal elasticity to be moderately sized, as argued in Lee (2001) and Ziliak and Kniesner (2005), using savings to finance future increases in labor supply is unlikely to play a very important role in this reform. Data from the first twelve waves of the BHPS show that just over one-third of lone mothers reported saving from current income. In addition, they reported saving relatively small amounts of money. The mean monthly amount saved among all lone mothers was around £31 (corresponding to less than 8 percent of gross monthly earnings), while the average amount among savers was £89. Given this evidence, it is therefore not very surprising that we did not find a decline in employment rates in anticipation of the implementation of WFTC.

The question is then to identify the factor(s) which may be responsible for the strong labor supply increase prior to the reform. Among the mechanisms that could explain

[23]These studies do not allow for frictions or human capital effects. Imai and Keane (2004) find larger intertemporal substitution elasticities after accounting for human capital accumulation effects in a lifecycle labor supply model. In our framework, human capital effects, as well as labor market frictions, can encourage responses to announcement effects since experience increases the value of future labor supply.

a positive announcement effect, human capital accumulation is unlikely to have been an important pathway. This is because of the relatively small additional expected wage return (on top of the WFTC induced net wage increase) from one additional year of experience. Wage experience profiles have been found to be flatter for women, and especially for lower educated and unskilled workers, than for other workers (Dustmann and Meghir, 2005; Connolly and Gottschalk, 2006). In their study of single parents in the Canadian Self-Sufficiency Program, Card and Hyslop (2005) find little evidence of long term impact on wages of those who have experienced higher levels of labor supply. There is also little empirical evidence that supports a significant role of habit persistence in women's work decisions. Estimates from life cycle models of female labor supply typically find no evidence of habit persistence (Eckstein and Wolpin, 1989; Francesconi, 2002).

Based on the existing evidence, therefore, our findings point to short-run labor market frictions as the main explanation for the observed employment increase, with women entering or remaining in the labor market to be in a position to benefit from the announced reform the following year.[24] There is evidence that American single mothers are likely to be employed in short-lived jobs (Card and Blank, 2000).[25] If this were to be true also for British lone mothers, then the motivation for early entry into (or delayed exit out of) employment between the WFTC announcement and its implementation based on search frictions may be weak. In this case, we would expect many of the jobs that were started following the announcement to have terminated by the time the reform was introduced. There are two reasons for why we do not believe this argument to hold for the single mothers in our study. First, even if jobs held by single mothers were to be short-lived, if having a job (versus not working) makes it easier to move to another job and to remain employed, the prospects of WFTC benefits would still provide an incentive to enter and remain in the labor force.[26] Second, it actually turns out that lone mothers who started

[24]In line with this interpretations, Chetty et al. (2011b) use Danish tax records to document that labor market frictions play a significant role in shaping labor supply responses to tax changes.

[25]Recent U.S. evidence, however, documents that welfare recipients, many of which are single mothers, 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).

[26]As pointed out by Pissarides and Wadsworth (1994), in Britain twice as many workers chose to look for work whilst employed, rather than quit into full-time search unemployment, suggesting that searching on the job is an important mechanism for labor market transitions.

new jobs in our sample during the 1997–2000 period on average kept jobs with a median tenure of 36 months.[27] For these reasons we see labor market frictions as the most credible explanation for the strong, positive anticipation effects.

As a further, more direct piece of evidence, we checked whether single mothers who lived in areas with greater labor market frictions responded differently from those who lived in areas with weaker frictions. To ascertain this, we matched labor market information on 306 travel-to-work areas to our BHPS sample. A woman was defined to have faced high (low) labor market frictions if she lived in a travel-to-work area with an above (below) average stock of unemployment.[28] We then repeated the analysis of Table 3 after interacting the friction indicator with the announcement and implementation effect variables as well as the single mother group and trend measures. The results indicate that, although there is no significant difference in implementation effects between women in high friction and low friction areas, announcement effects are almost 50 percent larger for women in high friction areas (with the difference being statistically significant at the 10 percent level). This evidence lines up well with our friction story, according to which single mothers in slacker labor markets had an incentive to be in eligible employment even before the introduction of the reform.

V. Other Outcomes and Sensitivity Analysis

A. Paid and Unpaid Child Care 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 child care costs (Blundell and Hoynes, 2004; Francesconi and van der Klaauw, 2007; Brewer et al., 2009). The estimates in the first column of Table 5 (panel A) indicate that the introduction of WFTC led to an increase in the use of paid child care services of about 2-3 percentage points in both samples and regardless of

[27] For this analysis, we used the wave-on-wave job history information collected by the BHPS and considered job durations of all the women who entered paid job at the time of interview in the 1997–2000 period. For robustness purposes, we also computed job durations using a sample of unpartnered women drawn from the 1998-2000 Labour Force Surveys (LFS) and found a very similar median job tenure. Moreover, 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.

[28] Since 2000, job centers stopped providing information on vacancies and labor force stock. Thus, standard measures of labor market tightness and unemployment/vacancy ratios could not be computed at the travel-to-work area over the whole BHPS sample. As an additional robustness exercise, we re-defined the presence of labor market frictions on the basis of unemployment inflows and found results that are similar to those discussed in the text.

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 child care utilization. The announcement effect estimates are always very small and highly insignificant.

This result is confirmed when we disaggregate implementation 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 child care 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 implementation 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 child care 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 child care utilization.

The switch from informal to formal child care utilization at the time of the WFTC reform can be explained by the fact that, up to that time, paid child care 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 child care 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 child care tax credit top-up.

B. Transitions in the Labor Market and Child Care 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 6.

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. While the magnitude of the effects is generally slightly smaller, the increase applies to a larger base of single mothers not in eligible employment prior to 1998. These results point again to the importance of labor supply decisions that were taken between the announcement of the reform and its implementation, which are consistent with the presence of labor market imperfections.

We build on the previous results on child care usage and examine labor market transitions jointly with child care utilization decisions. Four processes are estimated using the BHPS sample, the results of which are in Table 7. We begin with the transition from nonwork (including non-eligible employment) to eligible employment with paid child care services. The estimates (in the first row of the table) 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 child care 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 child care 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 child care, when Family Credit did not provide a generous support to child care expenditures. With the introduction of WFTC, they stayed in eligible employment but took advantage of the more generous support to child care costs and switched to paid child care services.

The next two transition processes shown in Table 7 confirm this interpretation. About two-thirds of the increase in the persistence probability in eligible employment (measured in

Table 6) are driven by mothers who moved to paid child care 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 child care arrangements.

As mentioned earlier, press coverage around the time of the Budget speech in March 1998 when WFTC was formally announced, was considerable, especially during the February-April period. The existence and timing of anticipation effects is based on information diffusion, similar to the way that news announcements matter in financial markets (Andersen et al., 2003). We therefore examine whether or not our results match with the timing of that announcement. In particular, if the employment changes estimated for 1998 were concentrated after March of that year (seasonal effects apart), they could be seen more credibly as a behavioral adjustment in response to the announcement in 1998 of the 1999 policy reform. On the other hand, if such changes were equally spread over time before and after the Budget speech, that interpretation would be harder to defend. Figure 9(a) shows monthly rates in eligible employment for single women without children and for lone mothers between September 1997 and December 1998. While the employment rate of single childless women increased by less than 1 percentage point over that period, the employment rate of single mothers increased by 3.5 percentage points (from 38 to 41.5 percent). Interestingly, 82 percent (about 2.9 percentage points) of that increase occurred during the April-July period. This result is consistent with the presence of an announcement effect.

Another way of documenting the importance of the WFTC announcement is to look at the distribution of dates (months) in which women who were in employment at the time of the 1998 interview started a job since the previous September. The distributions for single childless women and lone mothers, which include both job-to-job transitions and new labor market entries, are plotted in Figure 9(b). Of the new jobs that lone mothers started between September 1997 and December 1998, 62 percent commenced after March 1998, whereas only 51 percent of the new jobs started by unmarried women without children began after the Budget speech.

C. 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.[29] This restriction marginally reduces both implementation and announcement effects, but the estimates (not shown) are still significant and not statistically different from those reported in Tables 2 and 3. Second, 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.

Third, 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. Fourth, 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.

A further sensitivity test relates to the difference in surveying periods covered by the two surveys under analysis. This may have an effect on how pre-program trends can influence the evaluation analysis. To gain 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 8. Both implementation 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 employment and the work/nowork margin lose statistical significance, as was the case when these were estimated using the FRS sample. Similar patterns are revealed by the employment 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 difference in the magnitude of the estimated effects based on the BHPS and FRS data, and especially the implementation effects, might be driven by the different years covered by the two surveys and the associated length of the pre-reform period.

[29]The 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.

VI. Conclusion

In a world in which agents are forward looking, behavior may respond not only to the actual implementation of public policy reforms but also in anticipation of such reforms. The potential presence of anticipation and announcement effects has important implications for policy evaluation. If a reform is anticipated or announced in advance of its introduction, this anticipatory behavior may affect the ultimate size of the overall impact of the reform. Moreover, an anticipated reform that is not implemented can also generate behavioral responses.

In this paper we consider the implications of such anticipatory behavior for the evaluation of welfare reform impacts as commonly conducted. We illustrate these issues by analyzing potential labor supply responses to a simple in-work benefit reform that increases a worker's net earnings. Using a simple dynamic model of female labor force decisions, we discuss various channels and mechanisms through which the anticipation or announcement of a reform could generate pre-reform employment responses. We also discuss how such anticipatory behavior affects the interpretation and estimation of program impacts, for example when adopting standard differences in differences methods.

We then provide an empirical application which 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. By documenting the precise timing of announcement and implementation, our analysis uncovers strong evidence of announcement effects of WFTC on single mothers' labor supply. These effects turn out to be positive, and consistent with the presence of frictions in the labor market that cause women to work between the announcement and the actual introduction of the reform in order to increase the chance that they could benefit from the new, more generous, tax credit after its introduction. The magnitude of the estimated announcement effect is large, typically half the size of the corresponding estimated implementation effects. In addition, implementation 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) child care 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) child care utilization. This again reveals cogent evidence of forward looking behavior. Women in fact had to pay for formal child care services without receiving benefits directly to cover such costs before the introduction of WFTC. Instead, they would have received a generous child care tax credit after its implementation. Results based on transitions of child care decisions confirm this interpretation.

The role and consequences of anticipatory behavior for the evaluation of welfare reforms represents an important and relatively understudied topic for future research. Our analysis in this paper suggests that such behavior can significantly affect estimates of the reform's overall effectiveness. It also highlights the importance of identifying and measuring announcements, information and beliefs regarding future reforms. As pointed out by Chetty, Looney, Kroft (2009) and Chetty and Saez (2009), the effects of policies will vary substantially depending on their information and salience characteristics.

Economists in recent years have begun to devote considerable effort to the collection and analysis of data on subjective expectations and belief (Manski, 2004). As shown in this paper, to explore the presence and importance of anticipatory behavior, it will be important for economists to collect more data on individual's beliefs and expectations regarding the likelihood and nature of future welfare reforms and of social interventions more generally.

Appendix A

Solution to the Dynamic Program

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

$$V_{it}(A_{it-1}, X_{it-1}, d_t, \epsilon_{it}) = \max[V^1_{it}(A_{it-1}, X_{it-1}, d_t, \epsilon_{it}), V^0_{it}(A_{it-1}, X_{it-1}, d_t)],$$

where $V^1_{it}(\cdot)$ and $V^0_{it}(\cdot)$ 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 $J_{i3} = \{0, 1\}$ are:

$$V^1_{i3}(A_{i2}, X_{i2}, d_3, \epsilon_{i3}) = U_{i3}(c_{i3} = (1 + r)A_{i2} + w_{i3} + N_{i3}, y_{i3} = 1) \tag{A.1}$$

$$V^0_{i3}(A_{i2}, X_{i2}, d_3) = U_{i3}(c_{i3} = (1 + r)A_{i2} + N_{i3}, y_{i3} = 0) \tag{A.2}$$

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

$$\begin{aligned} y^*_{i3}(A_{i2}, X_{i2}, d_3, \epsilon_{i3}) &= 1 \quad \text{iff} \quad V^1_{i3}(A_{i2}, X_{i2}, d_3, \epsilon_{i3}) > V^0_{i3}(A_{i2}, X_{i2}, d_3), \\ y^*_{i3}(A_{i2}, X_{i2}, d_3, \epsilon_{i3}) &= 0 \quad \text{otherwise.} \end{aligned}$$

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

$$\begin{aligned} \mathrm{E}V^{\{0,1\}}_{i3}(A_{i2}, X_{i2}, d_3) = \int & [y^*_{i3}(A_{i2}, X_{i2}, d_3, \epsilon_{i3})V^1_{i3}(A_{i2}, X_{i2}, d_3, \epsilon_{i3}) \\ & +(1 - y^*_{i3}(A_{i2}, X_{i2}, d_3, \epsilon_{i3}))V^0_{i3}(A_{i2}, X_{i2}, d_3)]f(\epsilon_{i3})d\epsilon_{i3} \end{aligned} \tag{A.3}$$

where $f(\cdot)$ represents the logistic density function.

In the case in which $J_{i3} = \{0\}$, that is, when the woman has no job available because of labor market frictions, the expected value is

$$\mathrm{E}V^{\{0\}}_{i3}(A_{i2}, X_{i2}, d_3) = V^0_{i3}(A_{i2}, X_{i2}, d_3). \tag{A.4}$$

Combining (A.3) and (A.4) yields the expected remaining lifetime utility value to each woman for period 3, namely

$$\mathrm{E}V_{i3}(A_{i2}, X_{i2}, y_{i2}, d_3) = \lambda_3(y_{i2})\mathrm{E}V^{\{0,1\}}_{i3}(A_{i2}, X_{i2}, d_3) + (1 - \lambda_3(y_{i2}))\mathrm{E}V^{\{0\}}_{i3}(A_{i2}, X_{i2}, d_3),$$

where the expected value function now also explicitly depends on the previous period's employment choice y_{it-1}, and with $\lambda_3(1) = 1$.

At time $t = 2$, the value functions corresponding to (A.1) and (A.2) will have to account for beliefs regarding the possible implementation of a reform in period 3. In addition, agents now optimally choose both their employment and consumption levels. The value functions when woman i works and when she does not work are given respectively by

$$\begin{aligned} V^1_{i2}(A_{i1}, X_{i1}, d_2, \epsilon_{i2}) &= \max_{0 \le c \le K(1)} \{U_{i2}(c, y_{i2} = 1) + \delta\pi_{23}(d_2)\mathrm{E}V_{i3}[K(1) - c, X_{i1} + 1, 1, 1] \\ &\qquad + \delta(1 - \pi_{23}(d_2))EV_{i3}[K(1) - c, X_{i1} + 1, 1, 0]\} \\ V^0_{i2}(A_{i1}, X_{i1}, d_2) &= \max_{0 \le c \le K(0)} \{U_{i2}(c, y_{i2} = 0) + \delta\pi_{23}(d_2)\mathrm{E}V_{i3}[K(0) - c, X_{i1}, 0, 1] \\ &\qquad + \delta(1 - \pi_{23}(d_2))EV_{i3}[K(0) - c, X_{i1}, 0, 0]\}. \end{aligned}$$

where $K(y_{i2}) = (1+r)A_{i1} + w_{i2}y_{i2} + N_{i2}$ and where it is important to recall again that $\pi_{23}(1)=1$. Then, the decision rule at $t=2$ when in receipt of a job offer is

$$y^*_{i2}(A_{i1}, X_{i1}, d_2, \epsilon_{i2}) = 1 \quad \text{iff} \quad V^1_{i2}(A_{i1}, X_{i1}, d_2, \epsilon_{i2}) > V^0_{i2}(A_{i1}, X_{i2}, d_2),$$

$$y^*_{i2}(A_{i1}, X_{i1}, d_2, \epsilon_{i2}) = 0 \quad \text{otherwise.}$$

while, if no job offer was received (when $J_{i1}=\{0\}$), then $y^*_{i2}=0$.

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 $t=3$, π_{23}, we have

$$\begin{aligned} \mathrm{E}V_{i2}(A_{i1}, X_{i1}, y_{i1}, d_2|\pi_{23}) = \lambda_2(y_{i1})\mathrm{E}V^{\{0,1\}}_{i2}(A_{i1}, X_{i1}, d_2|\pi_{23}) \\ +(1-\lambda_2(y_{i1}))\mathrm{E}V^{\{0\}}_{i2}(A_{i1}, X_{i1}, d_2|\pi_{23}), \end{aligned} \tag{A.5}$$

where the choice-specific expected value functions in (A.5) are given by

$$\begin{aligned} \mathrm{E}V^{\{0,1\}}_{i2}(A_{i1}, X_{i1}, d_2|\pi_{23}) &= \int [y^*_{i2}(A_{i1}, X_{i1}, d_2, \epsilon_{i2})V^1_{i2}(A_{i1}, X_{i1}, d_2, \epsilon_{i2}) \\ &\quad +[1-y^*_{i2}(A_{i1}, X_{i1}, d_2, \epsilon_{i2})]V^0_{i2}(A_{i1}, X_{i1}, d_2)]f(\epsilon_{i2})d(\epsilon_{i2}) \\ \mathrm{E}V^{\{0\}}_{i2}(A_{i1}, X_{i1}|\pi_{23}) &= V^0_{i2}(A_{i1}, X_{i1}, d_2). \end{aligned}$$

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

$$\begin{aligned} V^1_{i1}(A_{i0}, X_{i0}, \epsilon_{i1}) &= \max_{0\le c\le K(1)} \{U_{i1}(c, y_{i1}=1) + \delta\pi_{12}\mathrm{E}V_{i2}[K(1)-c, X_{i0}+1, 1, 1|\pi_{13}] \\ &\quad +\delta(1-\pi_{12})\mathrm{E}V_{i2}(K(1)-c, X_{i0}+1, 1, 0|\pi_{13})\} \\ V^0_{i1}(A_{i0}, X_{i0}) &= \max_{0\le c\le K(0)} \{U_{i1}(c, y_{i1}=0) + \delta\pi_{12}\mathrm{E}V_{i2}[K(0)-c, X_{i0}, 0, 1|\pi_{13}] \\ &\quad +\delta(1-\pi_{12})EV_{i2}(K(0)-c, X_{i0}, 0, 0|\pi_{13})\}, \end{aligned}$$

where $K(y_{i1})=(1+r)A_{i0}+w_{i1}y_{i1}+N_{i1}$, while π_{12} and π_{13} are the agent's perceived likelihoods in period 1 of a reform occurring in periods 2 and 3, respectively. The decision rule in a frictionless world (when $J_{i1}=\{0,1\}$) is then

$$y^*_{i1}(A_{i0}, X_{i0}, \epsilon_{i1}) = 1 \quad \text{iff} \quad V^1_{i1}(A_{i0}, X_{i0}, \epsilon_{i1}) > V^0_{i1}(A_{i0}, X_{i1}),$$

$$y^*_{i1}(A_{i0}, X_{i0}, \epsilon_{i1}) = 0 \quad \text{otherwise,}$$

while, regardless of the realization of ϵ_{i1}, y^*_{i1} is always equal to 0 in an economy with job market frictions, that is, when $J_{i1}=\{0\}$.

In solving the optimal consumption and employment choices in the dynamic programming problem, we used standard value function approximation methods, where for each possible value of $(X_{it-1}, y_{it-1}, d_t)$, $EV_{i3}(A_{i2}, X_{i2}, y_{i2}, d_3)$ was approximated by a quadratic function in assets. These approximations were very close, with R^2 values always greater than 0.99.

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.[30] Along with other active labor market programs (such as the various welfare-to-work "New Dea" 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.[31] 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 child care costs, in the form of a basic child credit and a child care 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

[30] 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).

[31] 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 child care costs.[32] From October 1994, FC allowed eligible child care 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.[33] This meant that the maximum child care 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 child care. 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 child care 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 child care tax credit, and it was administered and paid out differently.

[32] To be "eligible" (or "relevant"), child care services must be provided by registered child minders, day nurseries, and after-school clubs, or certain other special schools or establishments that are exempt from registration. Relevant child care 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.

[33] 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

Abbring, Jaap H., and James J. Heckman. 2007. "Econometric Evaluation of Social Programs, Part III: Distributional Treatment Effects, Dynamic Treatment Effects, Dynamic Discrete Choice, and General Equilibrium Policy Evaluation." In J.J. Heckman and E.E. Leamer (eds.) *Handbook of Econometrics*, vol. 6B. Amsterdam: Elsevier, pp. 5145–5303.

Abbring, Jaap H., and Gerard J. van den Berg. 2003. "The Nonparametric Identification of Treatment Effects in Duration Models." *Econometrica*, 71(5): 1491–1517.

Altonji, Joseph G. 1986. "Intertemporal Substitution in Labor Supply: Evidence from Micro Data." *Journal of Political Economy*, 94(3, Part 2): S176–S215.

Andersen, Torben G., Tim Bollerslev, Francis X. Diebold, and Clara Vega. 2003. "Micro Effects of Macro Announcements: Real-Time Price Discovery in Foreign Exchange." *American Economic Review* 93(1): 38–62.

Ashenfelter, Orley. 1984. "Macroeconomic Analyses and Microeconomic Analyses of Labor Supply." *Carnegie-Rochester Conference Series on Public Policy*, 21: 117–55.

Attanasio, Orazio, Costas Meghir, and Ana Santiago. "Education Choices in Mexico: Using a Structural Model and a Randomized Experiment to Evaluate PROGRESA." *Review of Economic Studies*, forthcoming.

Attanasio, Orazio P., and Susann Rohwedder, 2003. "Pension Wealth and Household Saving: Evidence from Pension Reforms in the United Kingdom." *American Economic Review*, 93(5): 1499–1521.

Auerbach, Alan J., and Jonathan M. Siegel. 2000. "Capital Gains Realizations Of The Rich And Sophisticated." *American Economic Review Papers and Proceedings*, 2000, 90(2): 276–82.

Beaudry, Paul, and Franck Portier. 2006. "Stock Prices, News, and Economics Fluctuations." *American Economic Review*, 96(4):1293–1307.

Black, Dan A., Jeffrey A. Smith, Mark C. Berger, and Brett J. Noel. 2003. "Is the Threat of Reemployment Services More Effective Than the Services Themselves? Evidence from Random Assignment in the UI System." *American Economic Review*, 93(4): 1313–27.

Blundell, Richard, Mike Brewer, Howard Reed, and Andrew Shepard. 2004. "Evaluation the Labour Market Impact of the Working Families' Tax Credit Using Difference-in-Differences." Mimeo, Institute for Fiscal Studies, August.

Blundell, Richard, Mike Brewer, and Marco Francesconi. 2008. "Job Changes and Hours Changes: Understanding the Path of Labor Supply Adjustment." *Journal of Labor Economics*, 26(3): 421–53.

Blundell, Richard, and Hilary Hoynes. 2004. "Has 'In-Work' Benefit Reform Helped the Labour Market?" In R. Blundell, D. Card and R.B. Freemand (eds.) *Seeking a Premier Economy: The Economic Effects of British Economic Reforms, 1980-2000.* Chicago: University of Chicago Press, pp. 411–59.

Brewer, Mike, Alan Duncan, Andrew Shepard, and María José Suárez. 2006. "Did Working Families' Tax Credit Work? The Impact of In-Work Support on Labour Supply in Great Britain."

Labour Economics, 13(6): 699–720.

Brewer, Mike, Marco Francesconi, Paul Gregg, and Jeffrey Grogger. 2009. "In-Work Benefit Reform in a Cross-National Perspective: Introduction." *Economic Journal*, 119(535): F1–F14.

Card, David, and Rebecca M. Blank. 2000. *Finding Work: Jobs and Welfare Reform.* New York: Russell Sage Foundation.

Card, David, Richard Blundell, and Richard B. Freeman. (eds.) 2004. *Seeking a Premier Economy: The Economic Effects of British Economic Reforms, 1980-2000.* Chicago: University of Chicago Press.

Card, David, Raj Chetty, and Andrea Weber. 2007. "Cash-On-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market" *Quarterly Journal of Economics*, 122(4): 1511–60.

Card, David, and Dean R. Hyslop. 2005. "Estimating the Effects of a Time-Limited Earnings Subsidy for Welfare-Leavers." *Econometrica*, 73(6): 1723–70.

Chetty, Raj, John N. Friedman, Tore Olsen, and Luigi Pistaferri. 2009. "Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records." *Quarterly Journal of Economics*, 126(2): 749–804. (b)

Chetty, Raj, Adam Guren, Day Manoli and Andrea Weber. 2011. "Are Micro and Macro Labor Supply Elasticities Consistent? A Review of Evidence on the Intensive and Extensive Margins." *American Economic Review Papers and Proceedings*, 101(3): 471–75. (a)

Chetty, Raj, Adam Looney, and Kory Kroft. 2009. "Salience and Taxation: Theory and Evidence." *American Economic Review* 99(4): 1145-77.

Chetty, Raj, and E. Saez. 2009. "Teaching the Tax Code: Earnings Responses to an Experiment with EITC Recipients." NBER Working paper 14836.

Connolly, Helen and Peter Gottschalk. 2006. "Differences in Wage Growth by Education Level: Do Less Educated Workers Gain Less from Work Experience?". Boston College Working Paper 473.

Council of Economic Advisers. 1997. *Technical Report: Explaining the Decline in Welfare Receipt, 1993–1996.* Washington, DC: Council of Economic Advisers, May.

Crépon, Bruno, Marc Ferracci, Grégory Jolivet, and Gerard J. van den Berg. 2010. "Analyzing the Anticipation of Treatments Using Data on Notification Dates." IZA Discussion Papers No. 5265. Bonn: Institute for the Study of Labor (IZA).

Dominitz, Jeff, Charles F. Manski, and Jordan Heinz. 2003. "Will Social Security Be There for You? How Americans Perceive Their Benefits." NBER Working Paper No. 9798. Cambridge, MA: National Bureau of Economic Research.

Dustmann, Christian, and Costas Meghir. 2005. "Wages, Experience and Seniority." *Review of Economic Studies*, 72(1): 77–108.

Eckstein, Zvi, and Kenneth I. Wolpin. 1989. "Dynamic Labour Force Participation of Married Women and Endogenous Work Experience." *Review of Economic Studies*, 56(3): 375–90.

Evans, Martin D.D., and Richard K. Lyons, 2008. "How is Macro News Transmitted to Exchange

Rates?" *Journal of Financial Economics*, 88(1): 26–50.

Farber, Henry S. 2008. "Employment Insecurity: The Decline in Worker-Firm Attachment in the United States." Working Paper # 530, Princeton University, Industrial Relations Section.

Francesconi, Marco. 2002. "A Joint Dynamic Model of Fertility and Work of Married Women." *Journal of Labor Economics*, 20(2, part 1): 336–380.

Francesconi, Marco, and Wilbert van der Klaauw. 2007. "The Socioeconomic Consequences of 'In-Work' Benefit Reform for British Lone Mothers." *Journal of Human Resources*, 42(1): 1–31.

Francesconi, Marco, Helmut Rainer, and Wilbert van der Klaauw. 2009. "The Effects of In-Work Benefit Reform in Britain on Couples: Theory and Evidence." *Economic Journal*, 119(535): F66–F100.

French, Eric B. 2004. "The Labor Supply Response to (Mismeasured but) Predictale Wage Changes." *Review of Economics and Statistics*, 86(2): 602–13.

Goolsbee, Austan. 2000. "What Happens When You Tax the Rich? Evidence from Executive Compensation." *Journal of Political Economy*, 108(2): 352–78.

Gregg, Paul, Susan Harkness, and Sarah Smith. 2009. "Welfare Reform and Lone Parents in the UK." *Economic Journal*, 119(535): F38–F65.

Grogger, Jeffrey, and Charles Michalopoulos. 2003. "Welfare Dynamics under Time Limits." *Journal of Political Economy*, 111(3): 530–54.

Ham, John C., and Kevin T. Reilly. 2002. "Testing Intertemporal Substitution, Implicit Contracts, and Hours Restriction Models of the Labor Market Using Micro Data." *American Economic Review*, 92(4): 905–27.

Heckman, James J., and Salvador Navarro. 2007. "Dynamic Discrete Choice and Dynamic Treatment Effects." *Journal of Econometrics*, 136(2): 341–96.

Heckman, James J., and Edward J. Vytlacil. 2007. "Econometric Evaluation of Social Programs, Part I and II." In J.J. Heckman and E.E. Leamer (eds.) *Handbook of Econometrics*, vol. 6B. Amsterdam: Elsevier, pp. 4779–4874 and 4875–5144.

Heim, Bradley T. 2008. "Tax Rebates and Consumption Expenditures: State Tax Rebates as Natural Experiments." *National Tax Journal*, 60(4): 685–710.

Heisz, Andrew. 2005. "The Evolution of Job Stability in Canada: Trends and Comparisons with U.S. results." *Canadian Journal of Economics*, 38(1): 105–27.

H.M. Treasury. 1998. *The Modernisation of Britain's Tax and Benefit System (Number Two) - Work Incentives: A Report by Martin Taylor*. London: HM Treasury.

Holzer, Harry J., Michael A. Stoll, and Douglas Wissoker. 2004. "Job Performance and Retention among Welfare Recipients." *Social Service Review*, 78(3): 343–69.

Imai, Susumu and Michael P. Keane. 2004. "Intertemporal Labor Supply and Human Capital Accumulation." *International Economic Review*, 45(2): 601-641.

Jaimovich, Nir, and Sergio Rebelo. 2009. "Can News about the Future Drive the Business

Cycle?" *American Economic Review*, 99(4): 1097–1118.

Kari, Seppo, Hanna Karikallio, and Jukka Pirttilä. 2008. "Anticipating Tax Changes: Evidence from the Finnish Corporate Income Tax Reform of 2005." *Fiscal Studies*, 29(2): 167–96.

Keane, Michael P. and Kenneth I. Wolpin. 2002. "Estimating Welfare Effects Consistent with Forward Looking Behavior, Part I: Lessons from a Simulation Exercise." *Journal of Human Resources*, 37(3): 570–99.

Lee, Chul-In. 2007. "Finite Sample Bias in IV Estimation of Intertemporal Labor Supply Models: Is the Intertemporal Substitution Elasticity Really Small?" *Review of Economics and Statistics*, 83(4): 638–46.

Leigh, Andrew. 2007. "Earned Income Tax Credits and Labor Supply: New Evidence from a British Natural Experiment." *National Tax Journal*, 60(2): 205–24.

MaCurdy, Thomas E. 1981. "An Empirical Model of Labor Supply in a Life-Cycle Setting." *Journal of Political Economy*, 89(6): 1059–85.

Manski, Charles F. 2004. "Measuring Expectations." *Econometrica*, 72(5): 1329–76.

Meyer, Bruce D. 1990. "Unemployment Insurance and Unemployment Spells." *Econometrica*, 58(4): 757–82.

Moffitt, Robert. 1985. "Unemployment Insurance and the Distribution of Unemployment Spells." *Journal of Econometrics*, 28(1): 85–101.

Moffitt, Robert A. 1987. "Life Cycle Labor Supply and the Effect of the Social Security System: A Time-Series Analysis." In G. Burtless (ed.) *Work, Health, and Income Among the Elderly.* Washington: The Brookings Institution.

Moffitt, Robert A. 1999. "The Effect of Pre-PRWORA Waivers on AFDC Caseloads and Female Earnings, Income, and Labor Force Behavior." In S.H. Danziger (ed.) *Economic Conditions and Welfare Reform.* Kalamazoo, MI: Upjohn Institute Press.

Pencavel, John H. 2001. "The Response of Employees to Severance Incentives: The University of California's Faculty, 1991–94." *Journal of Human Resources*, 36(1): 58–84.

Pissarides, Christopher A., and Jonathan Wadsworth. 1994. "On-the-Job Search: Some Empirical Evidence from Britain." *European Economic Review* 38(2): 385–401.

van der Klaauw, Wilbert, and Kenneth I. Wolpin. 2008. "Social Security and the Savings and Retirement Behavior of Low Income Households." *Journal of Econometrics*, 145(1–2): 21–42.

Yeo, Alasdair. 2007. "Experience of Work and Job Retention Among Lone Parents: An Evidence Review." Department for Work and Pensions Working Paper No. 37. Leeds: Corporate Document Services.

Ziliak, James P., and Thomas J. Kniesner. 2005. "The Effect of Income Taxation on Consumption and Labor Supply." *Journal of Labor Economics*, 23(4): 769–796.

Table 1: Key Parameters of the Tax Credits at Baseline (FS regime), at Announcement, and at WFTC Implementation

	(1) **1998** (FS regime)	(2) **1998 Budget** (Announcement)	(3) **1999** (WFTC regime)	(4) **Difference** (c)–(b)
Basic rate	£48.80	£48.80 [£49.78]	£52.30	£3.50 (+7.2%) [+5.1%]
Additional credit for working 30+ hours	£10.80	£10.80 [£11.02]	£11.15	£0.35 (+3.2%) [+1.2%]
Credit for child aged:				
0–10	£14.85	£17.35 [£17.70]	£19.85	£2.50 (+14.4%) [+12.1%]
11–15	£20.45	£20.45 [£20.86]	£20.90	£0.45 (+2.2%) [+0.2%]
16–18	£25.40	£25.40 [£25.91]	£25.95	£0.55 (+2.2%) [+0.2%]
Taper rate	70%	55%	55%	none
Threshold	£79.00	£90.00	£90.00	none
Childcare tax credit	Costs deducted from earnings	70% of up to: £100 (1 kid) £150 (2+ kids)	70% of up to: £100 (1 kid) £150 (2+ kids)	none

Notes: Figures are in nominal values, except those in square brackets in column (2), which are expressed in constant 1999 prices using the Retail Price Index. In column (4), the figures in parentheses are percentages computed over the corresponding figures in column (2), while those in square brackets are percentages computed over the corresponding figures in square brackets in column (2).

Table 2: WFTC Implementation and Announcement Effects — Eligible Employment

	BHPS (N=15,260)[a]		FRS (N=76,886)[b]	
	(1)	(2)	(1)	(2)
OLS				
Implementation (ϕ_1)	**0.051**	**0.060**	**0.033**	**0.040**
	(0.016)	(0.018)	(0.008)	(0.011)
Announcement (ϕ_0)		**0.029**		0.018
		(0.014)		(0.016)
FE				
Implementation (ϕ_1)	**0.049**	**0.059**		
	(0.018)	(0.019)		
Announcement (ϕ_0)		0.027		
		(0.015)		

Sources: British Household Panel Survey (BHPS), 1991-2002; Family Resources Survey (FRS), 1995/96-2002/03.
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* as captured by ϕ_0 in (7). 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. 'Implementation' stands for *implementation effect* as captured by ϕ_1 in (7). 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.
[a] 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 Implementation and Announcement Effects — Other Employment Outcomes

	BHPS			FRS		
	(1)	(2)		(1)	(2)	
	(ϕ_1)	(ϕ_1)	(ϕ_0)	(ϕ_1)	(ϕ_1)	(ϕ_0)
Panel A: Full time employment[a]						
OLS	**0.045**	**0.054**	**0.026**	**0.030**	**0.039**	**0.019**
	(0.017)	(0.017)	(0.013)	(0.009)	(0.012)	(0.009)
FE	**0.042**	**0.049**	**0.020**			
	(0.020)	(0.021)	(0.009)			
Panel B: Employment[b]						
OLS	**0.056**	**0.061**	0.017	**0.052**	**0.058**	0.016
	(0.017)	(0.020)	(0.014)	(0.020)	(0.021)	(0.012)
FE	**0.052**	**0.055**	0.019			
	(0.017)	(0.019)	(0.014)			
Panel C: Hours worked[c]						
OLS	**3.32**	**4.60**	**2.41**	**3.58**	**4.21**	**1.91**
	(0.73)	(0.93)	(0.75)	(0.77)	(0.90)	(0.56)
FE	**2.95**	**4.12**	**1.96**			
	(0.057)	(0.68)	(0.63)			
Panel D: Hours worked[d]						
OLS	**2.45**	**2.51**	0.11	**2.30**	**2.92**	0.93
	(0.78)	(1.03)	(0.82)	(0.63)	(0.67)	(0.68)
FE	**2.30**	**2.37**	0.06			
	(0.58)	(0.72)	(0.38)			

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 Implementation and Announcement Effects — Eligible Employment by Age of Youngest Child and Number of Children

	BHPS			FRS		
	(1)	(2)		(1)	(2)	
	(ϕ_1)	(ϕ_1)	(ϕ_0)	(ϕ_1)	(ϕ_1)	(ϕ_0)
OLS						
One child aged 0–4	**0.097**	**0.121**	**0.048**	**0.066**	**0.083**	**0.040**
	(0.021)	(0.029)	(0.017)	(0.018)	(0.030)	(0.018)
One child aged 5–10	**0.078**	**0.094**	**0.037**	**0.059**	**0.071**	**0.031**
	(0.023)	(0.035)	(0.018)	(0.019)	(0.030)	(0.013)
One child aged 11–18	0.031	0.033	0.004	0.030	0.031	0.009
	(0.026)	(0.028)	(0.019)	(0.022)	(0.024)	(0.016)
Two children or more, youngest aged 0–4	0.044	**0.047**	0.026	**0.046**	**0.049**	**0.019**
	(0.032)	(0.023)	(0.019)	(0.021)	(0.023)	(0.016)
Two children or more, youngest aged 5–10	0.003	0.003	0.011	0.008	0.006	0.007
	(0.028)	(0.030)	(0.021)	(0.019)	(0.025)	(0.012)
Two children or more, youngest aged 11–18	0.002	0.005	0.004	0.006	0.006	0.001
	(0.031)	(0.033)	(0.020)	(0.027)	(0.030)	(0.018)
FE						
One child aged 0–4	**0.085**	**0.096**	**0.038**			
	(0.025)	(0.026)	(0.017)			
One child aged 5–10	**0.070**	**0.084**	**0.029**			
	(0.031)	(0.024)	(0.013)			
One child aged 11–18	0.032	0.028	0.011			
	(0.022)	(0.023)	(0.023)			
Two children or more, youngest aged 0–4	0.038	**0.043**	0.016			
	(0.021)	(0.020)	(0.018)			
Two children or more, youngest aged 5–10	0.020	0.019	0.010			
	(0.024)	(0.024)	(0.019)			
Two children or more, youngest aged 11–18	0.009	0.011	–0.002			
	(0.033)	(0.032)	(0.025)			

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

	BHPS (N = 5,616)			FRS (N = 35,469)		
	(1)	(2)		(1)	(2)	
	(ϕ_1)	(ϕ_1)	(ϕ_0)	(ϕ_1)	(ϕ_1)	(ϕ_0)
Panel A: Paid child care utilization[a]						
OLS	**0.031**	**0.031**	–0.004	**0.019**	**0.021**	–0.007
	(0.010)	(0.011)	(0.011)	(0.004)	(0.006)	(0.007)
FE	**0.028**	**0.029**	0.001			
	(0.013)	(0.012)	(0.013)			
Panel B: Paid child care utilization by child's age and number of children[a]						
OLS						
One child aged 0–4	**0.047**	**0.044**	0.003	**0.032**	**0.035**	–0.003
	(0.010)	(0.013)	(0.010)	(0.008)	(0.010)	(0.014)
One child aged 5–10	**0.038**	**0.041**	0.002	**0.028**	**0.028**	–0.001
	(0.009)	(0.016)	(0.015)	(0.007)	(0.009)	(0.006)
Two children or more,	0.013	0.011	–0.006	0.003	0.005	–0.010
youngest aged 0–4	(0.019)	(0.017)	(0.013)	(0.006)	(0.012)	(0.009)
FE						
One child aged 0–4	**0.040**	**0.043**	0.001			
	(0.014)	(0.019)	(0.022)			
One child aged 5–10	**0.031**	**0.030**	–0.001			
	(0.012)	(0.014)	(0.014)			
Two children or more,	0.011	0.006	–0.0001			
youngest aged 0–4	(0.023)	(0.025)	(0.009)			
Panel C: Unpaid child care utilization[b]						
OLS	0.002	–0.001	**0.029**	–0.003	0.001	**0.017**
	(0.019)	(0.014)	(0.012)	(0.009)	(0.011)	(0.007)
FE	0.008	0.004	**0.018**			
	(0.023)	(0.026)	(0.009)			
Panel D: Unpaid child care utilization by child's age and number of children[b]						
OLS						
One child aged 0–4	0.003	0.005	**0.034**	0.002	0.001	**0.025**
	(0.008)	(0.014)	(0.014)	(0.013)	(0.018)	(0.010)
One child aged 5–10	0.001	–0.002	**0.026**	–0.04	0.001	**0.020**
	(0.011)	(0.015)	(0.012)	(0.027)	(0.025)	(0.008)
Two children or more,	–0.003	–0.004	0.008	–0.001	–0.003	–0.004
youngest aged 0–4	(0.018)	(0.023)	(0.022)	(0.014)	(0.019)	(0.029)
FE						
One child aged 0–4	0.009	0.009	**0.023**			
	(0.018)	(0.022)	(0.011)			
One child aged 5–10	0.006	–0.001	0.019			
	(0.022)	(0.024)	(0.010)			
Two children or more,	0.008	0.004	0.007			
youngest aged 0–4	(0.025)	(0.018)	(0.013)			

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.

[a] 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.

[b] 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)

	(1)	(2)		
	(ϕ_1)	(ϕ_1)	(ϕ_0)	N
Persistence probability[a]	**0.058**	**0.070**	**0.024**	6,478
	(0.028)	(0.033)	(0.008)	
Entry probability[b]	**0.035**	**0.054**	**0.022**	5,429
	(0.015)	(0.023)	(0.010)	

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)

Type of transition	Implementation (ϕ_1)	Announcement (ϕ_0)	N
From nonwork to eligible employment with paid child care[a]	**0.039**	–0.001	1,871
	(0.015)	(0.010)	
From nonwork to eligible employment without paid child care[a]	0.013	**0.021**	1,871
	(0.022)	(0.010)	
From eligible employment without paid child care to eligible employment with paid child care[b]	**0.047**	–0.003	2,093
	(0.013)	(0.023)	
Persistence in eligible employment without paid child care[b]	0.020	**0.025**	2,093
	(0.021)	(0.011)	

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

Selected labor market outcomes	Implementation (ϕ_1)	Announcement (ϕ_0)	N
Eligible employment	**0.038**	0.012	10,224
	(0.016)	(0.032)	
Full time employment	**0.033**	**0.016**	10,224
	(0.015)	(0.008)	
Employment	**0.042**	0.008	10,224
	(0.020)	(0.021)	
Hours worked (including zero hours)	**3.58**	**1.88**	10,224
	(1.73)	(0.67)	
Persistence probability in eligible employment	**0.055**	0.016	4,340
	(0.026)	(0.025)	
Entry probability in eligible employment	0.050	0.019	3,637
	(0.029)	(0.015)	

Notes: Standard errors are shown in parentheses. Estimates in bold are significant at the five percent level. Estimates are obtained from OLS models. See Tables 2, 3 and 6 for details.

Appendix Table A1: Summary Statistics by Sample

	BHPS		FRS	
	Unmarried women without children	Lone mothers	Unmarried women without children	Lone mothers
Outcomes				
Eligible employment[a]	0.636	0.415	0.686	0.431
Employment[b]	0.721	0.622	0.730	0.602
Full time employment[c]	0.526	0.270	0.543	0.302
Weekly hours worked	25.6	15.1	26.2	15.4
(including zeros)	(16.7)	(13.2)	(15.8)	(12.7)
Weekly hours worked	32.7	24.1	33.0	24.5
(conditional on work)[d]	(16.7)	(13.2)	(16.2)	(12.9)
Transition probabilities in eligible employment:				
Persistence probability	0.903	0.644		
Entry probability	0.262	0.191		
Paid child care utilization		0.134		0.142
Main explanatory variables				
Age (years)	33.096	30.728	34.339	33.951
	(13.510)	(11.412)	(13.340)	(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		
Left education at age 18			0.700	0.893
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		0.598
		(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).

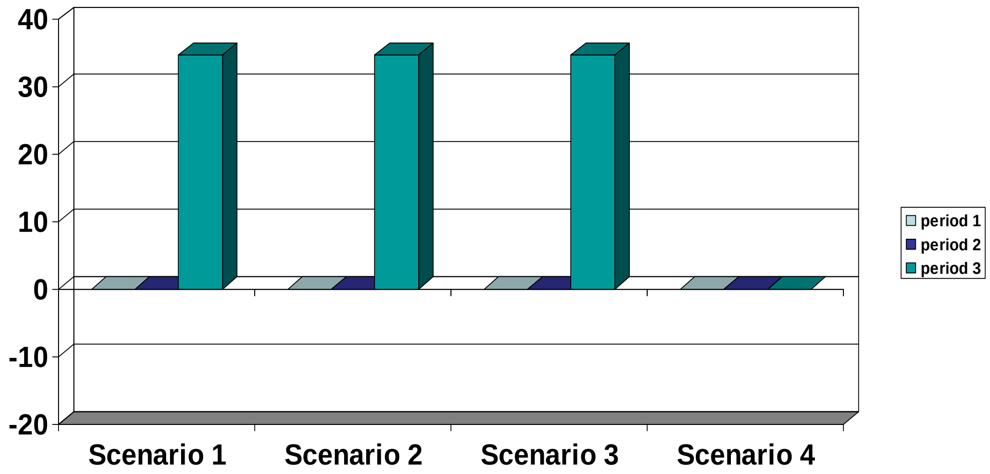

Figure 1: Benchmark Model (No Saving) — Impact on Employment Rates

Note: All figures are percentages computed with respect to a *baseline* scenario in which there is no reform and in which the possibility of a reform is never envisaged by women, i.e., $\pi_1 = \pi_2 = 0$ and no reform is introduced. Scenario 1: $\pi_1 = \pi_2 = 0$ and the reform is introduced in period 3, $d_3 = 1$. Scenario 2: $\pi_1 = 0, \pi_2 = 1$ and the reform is introduced in period 3, $d_t = 1$. Scenario 3: $\pi_1 = 0.5, \pi_2 = 1$ and the reform is introduced in period 3, $d_3 = 1$. Scenario 4: $\pi_1 = 0, \pi_2 = 1$ and the reform is not introduced in period 3, $d_3 = 0$. The other parameter values are: $\delta = 0.95, r = 0.05, N_{it} = 0.5, w_0 = 0.2$, $\alpha = 0$ (no human capital accumulation), $\beta = 0.45$, $\gamma_1 = -1.4, \gamma_2 = 0$ (utility is time separable), $\gamma_3 = 0$ and $\lambda_t(0) = 1$ (no job search frictions).

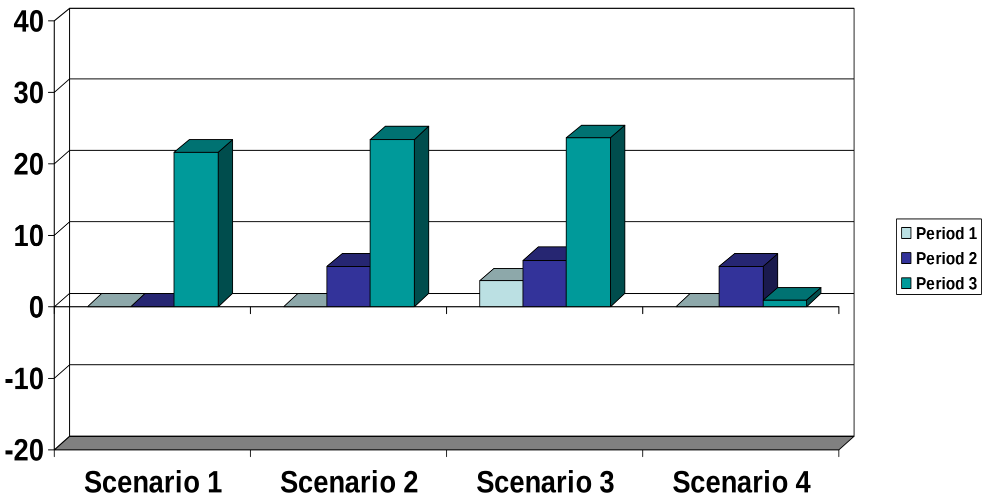

Figure 2: Model with Search Frictions — Impact on Employment Rates

Note: For a description see note to Figure 1. All parameter values are as in Figure 1, except $\lambda_t(0) = 0.5$.

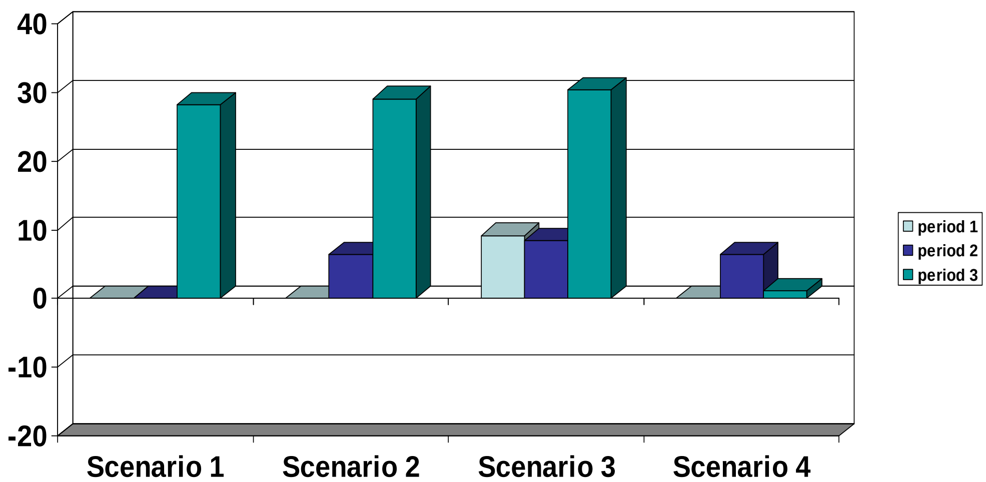

Figure 3: Model with Human Capital Formation — Impact on Employment Rates

Note: For a description see note to Figure 1. All parameter values are as in Figure 1, except $\alpha = 0.25$.

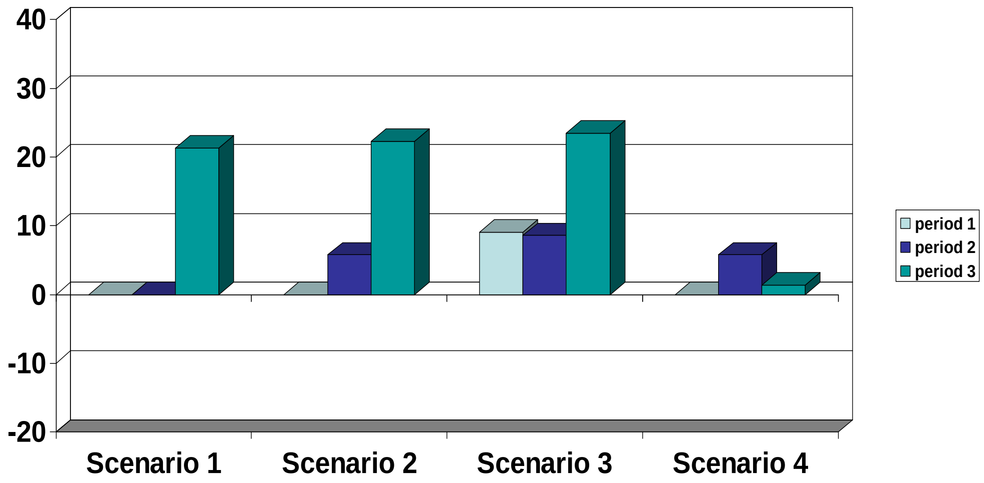

Figure 4: Model with Habit Persistence — Impact on Employment Rates

Note: For a description see note to Figure 1. All parameter values are as in Figure 1, except $\gamma_2 = 0.25$.

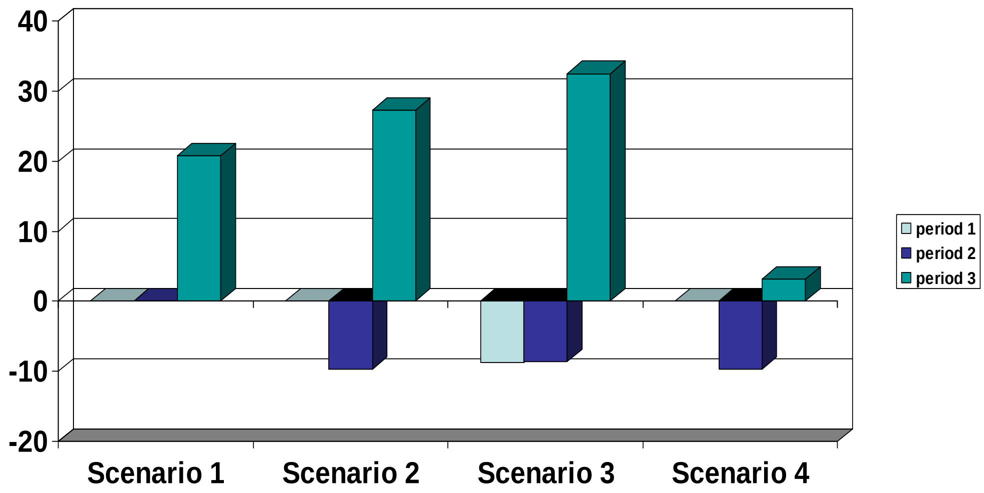

Figure 5: Model with Intertemporal Substitution — Impact on Employment Rates

Note: For a description see note to Figure 1. All parameter values are as in Figure 1, except $\gamma_2 = -2.5$.

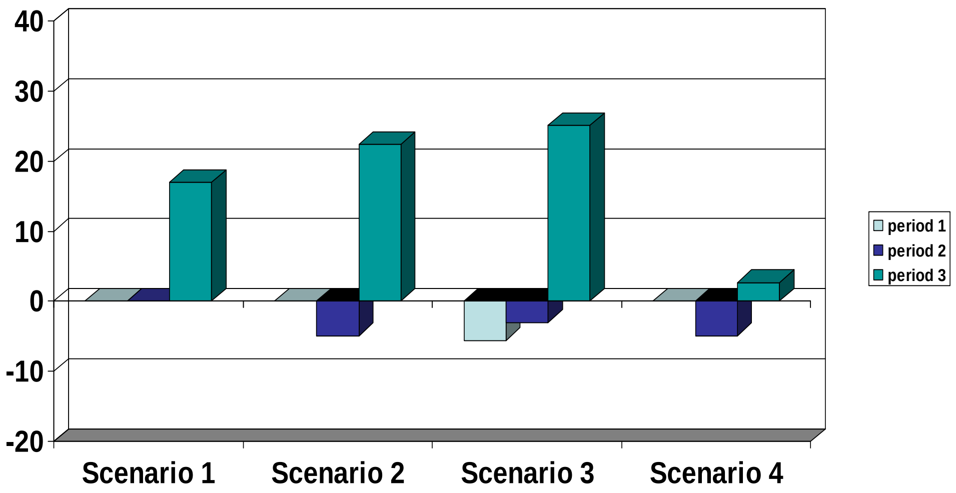

Figure 6: Model with Saving — Impact on Employment Rates

Note: For a description see note to Figure 1.

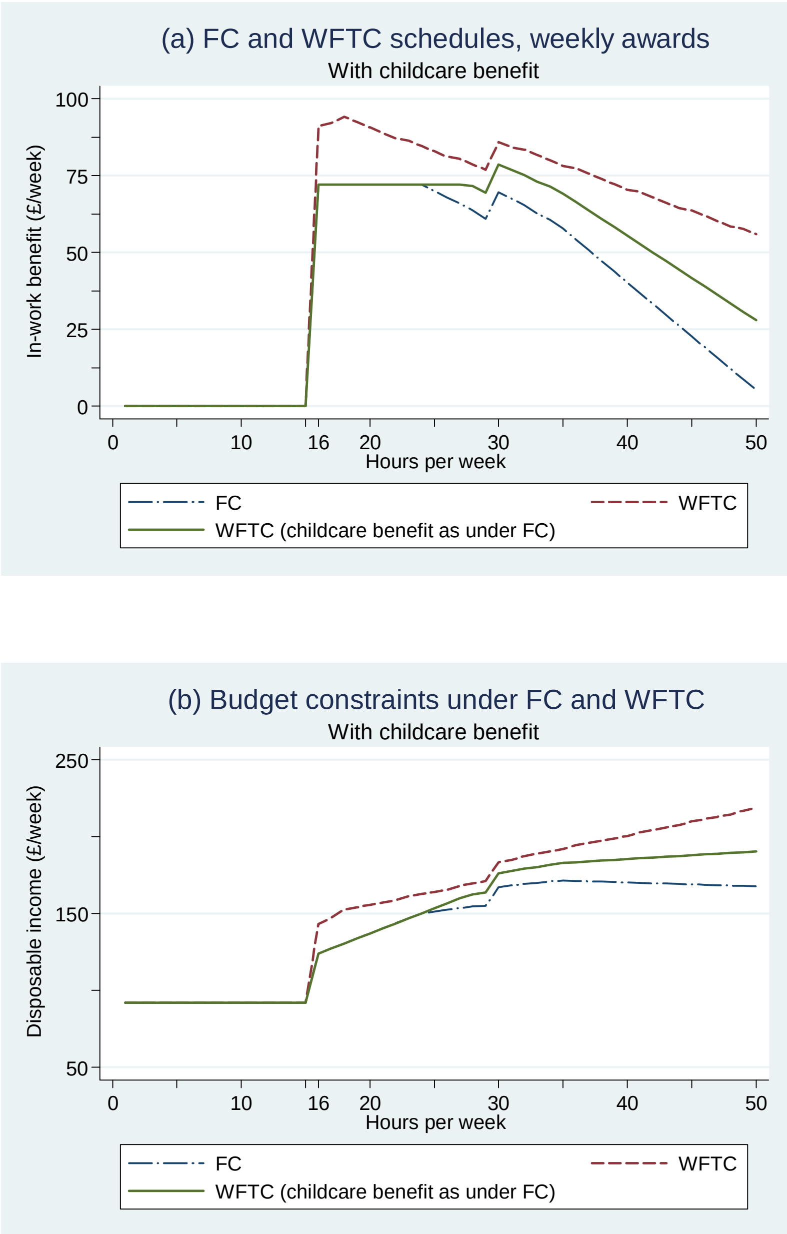

Figure 7: FC and WFTC Schedules and Budget Constraints

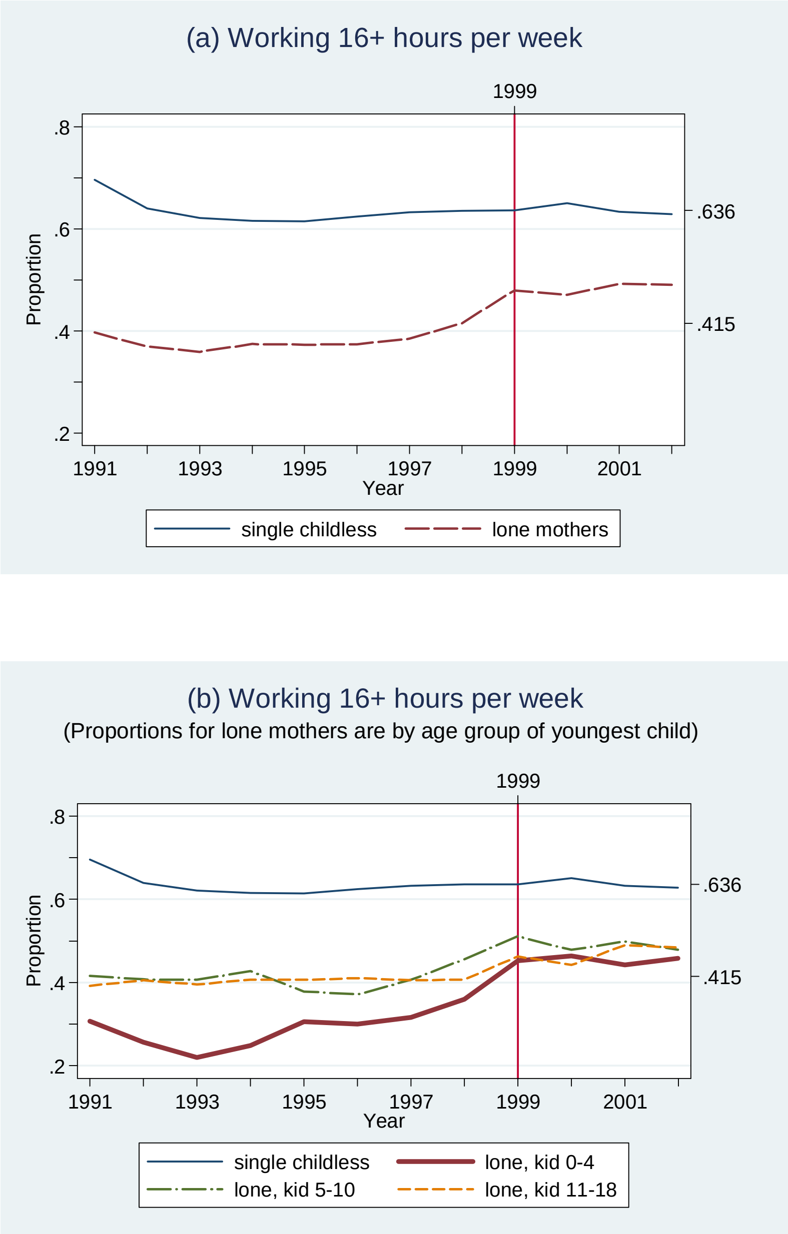

Figure 8: Trends in Eligible Employment

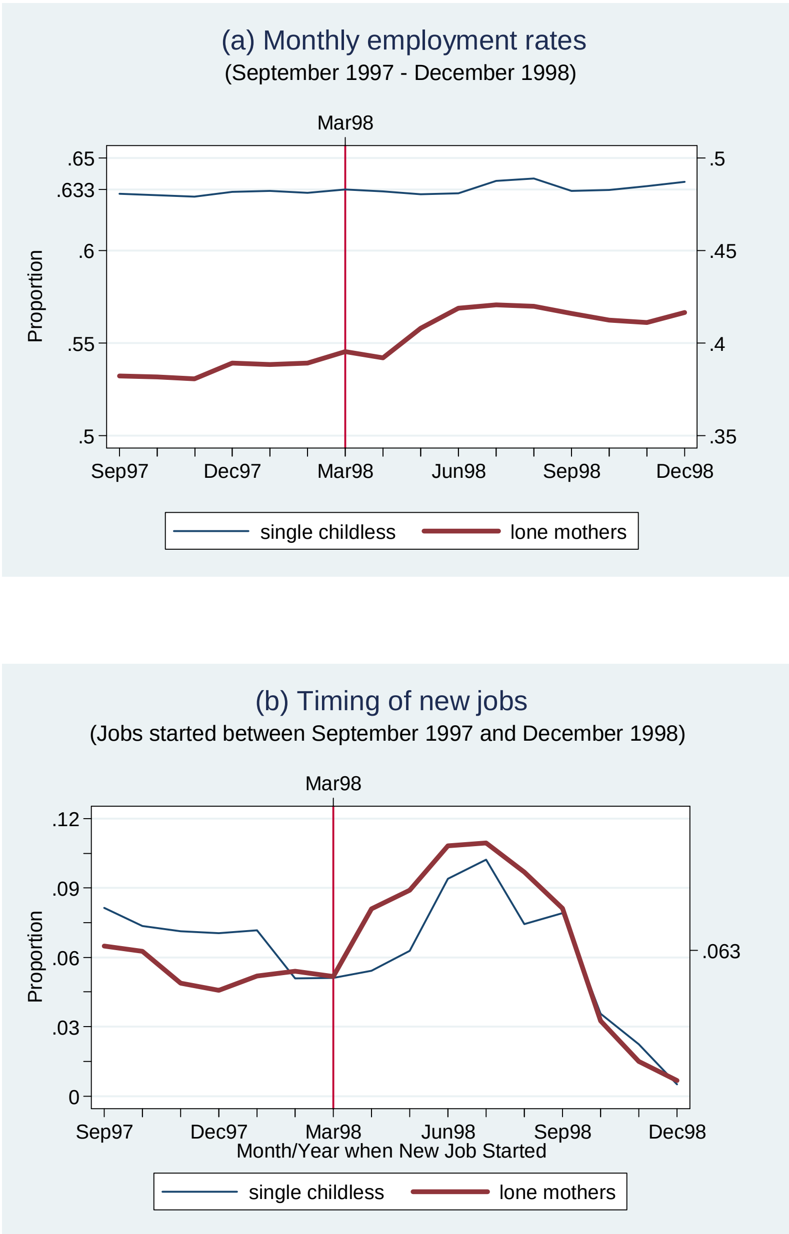

Figure 9: Employment Rates and Entry into New Jobs Around the Announcement Date