Financial Incentives and the Timing of Birth

by
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Declaration

I hereby declare that the work in this thesis was carried out in accordance with the Regulations of the University of Warwick. All materials in this thesis represent my own work, and none of these materials have been submitted for a degree at another university.

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Abstract

This thesis studies how financial incentives affect women's fertility timing decisions. Each chapter investigates this question by looking at a policy that exogenously increased fertility related financial incentives. The timing impacts of these policies are estimated using a discrete-time proportional hazard model with unobserved heterogeneity.

In the first chapter, the impact of the 1999 UK Working Families Tax Credit (WFTC) on the timing of birth is studied. This paper employs the 1991-2003 waves of the British Household Panel Survey and identifies the policy impact of WFTC by observing the change in the timing of birth using a difference in differences estimator. The main finding of this paper suggests little evidence of changes in the timing of all birth parity apart from first birth. Such a finding is likely to be explained by the policy design of WFTC that increased not only the fertility but also the labour supply incentives simultaneously. Moreover, a further analysis highlights the importance of other policies, which also influenced women's labour supply during the period of study.

The second chapter, on the other hand, studies the impact of the 1977-2001 US infertility health insurance mandates, which regulated the insurance companies to cover for infertility treatment cost. Although the majority of the past literature has studied impacts on older women who are likely to seek treatment, this paper proposes that the mandates may have had a wider impact on the US population. Specifically, it may have given an option for younger women to delay birth since these policies reduced the opportunity cost of having a child in the future. The chapter employs the 1980-2001 Panel Study of Income Dynamics. Results suggest a significant delay of 1-2 years in the time of first birth among highly educated white women.

Introduction

This thesis addresses a question regarding how financial incentives affect women's fertility timing decisions. To investigate this question, I study policies from the United Kingdom and the United States. These policies each provide short and long term incentives for the affected women to change when to have children. In the previous empirical literature, the analysis mainly focused on how the probability of birth changed in response to financial incentives. However, the observed change in the birth rate does not necessarily reflect an increase in the completed fertility, but may have been caused due to changes in the timing of birth. This thesis fills the gap in the literature on financial incentives and fertility by presenting relatively scarce evidence of the effect on the timing of birth.

In the first chapter of this thesis, the effect of the 1999 UK Working Families Tax Credit (WFTC) on the timing of birth is studied. This policy was introduced when the "New Labour" government took power as part of the attempt to make work pay. It was an in-work tax credit given to low income households with children when at least one parent was working more than 16 hours. The amount of tax credit was contingent on the level of household income as well as the number of children in the household. Although this policy was aiming to improve the labour market participation of low income families, the eligibility condition requiring the presence of children made the policy pronatalistic. The introduction of the policy, therefore, may have had an unintended shortening impact on the birth intervals of the affected families. This chapter uses a multiple-spell discrete-time proportional hazard model with unobserved heterogeneity and identifies the policy impact of WFTC by observing the change in the timing of birth using a difference-in-differences estimator. The data employed is the 1991-2003 British Household Panel Survey.

The second chapter, on the other hand, studies the impact of the US state-level infertility health insurance mandates, which regulated health insurance providers to cover infertility treatment cost. These state-level policies were introduced in 15 states over the period of 1977 and 2001 with different degrees of generosity. These mandates were introduced in order to help relieve the financial burden of infertile women caused by the high treatment cost. These women with infertility problems are typically aged above 35 and therefore these mandates primarily targeted the older women. However, this chapter sheds light on an unintended effect among younger women. US women, since the 1980s, are documented to have been struggling to balance work and life (see for example Rindfuss, Brewster, and Kavee (1996), Phipps, Burton, and Lethbridge (2001)). By making infertility treatment affordable, these mandates relaxed the biological constraints that US women have been facing. As a result, younger women who were planning when to have their first child may have delayed birth to advance in their careers first. To study the potential delaying effect, this chapter employs a single-spell discrete-time proportional hazard model with unobserved heterogeneity and investigates the delaying effect of first birth by a difference-in-differences estimator. The data from the 1980-1997 Panel Study of Income Dynamics is used.

In contrast to the UK WFTC in the first chapter, which directly affected the household income from the year 1999, the US infertility health insurance mandates changed the women's lifetime earning profiles. In other words, whilst the first chapter focuses on the impact of a policy that immediately changed the household income, the policy studied in the second chapter provides us with the rare opportunity to investigate the effect of a policy that changed the income profile in a longer time span. These two policies are also interesting as they each allow us to obtain understanding of the fertility timing responses among different demographic groups. More specifically, the demographic group studied in the second chapter is a group of highly educated women. This is in contrast to that in the first chapter, where the affected women are typically those with low educational attainment.

At first, the theoretical literature analysed the fertility behaviour in a static framework ignoring the stochastic element of fertility (Becker, 1960; Willis, 1973;

Becker and Lewis, 1973). However, the later studies highlight the importance of analysing fertility behaviour in a dynamic setting and studying the timing of birth rather than completed fertility. Joseph Hotz, Klerman, and Willis (1997) list three reasons why the dynamic setting is suited for the analysis of fertility. First, dynamic analysis allows for the stochastic element of the fertility process to be directly modelled. The uncertainties may arise partially due to the lifecycle household budget constraint and partially due to the fact that neither contraception nor conceptions are perfectly controllable. Second, under the static setting, the change in prices of children or the level of household income only affects the number of children the couples have. However, such changes may also influence the timing dimension of fertility. In fact, the observed changes in the total fertility may have been driven by the underlying changes in the timing of births. Third, fertility decisions are closely related to women's lifecycle labour supply as well as their human capital investment. The dynamic framework allows these elements to be explicitly modelled into the analysis.

These dynamic theoretical models provide predictions for the impact of changes in household income and women's wages and assist in forming the core theoretical predictions of the policy impacts investigated in this thesis.

For the purpose of studying the effect of changes in household income, the existing theoretical literature identifies two types of increase. The first type is when household income increases permanently from period t onwards (e.g. Newman (1988); Leung (1991)). The second case is when the household income profile becomes steeper over their lifecycle (Heckman and Willis, 1976). The former prediction is particularly relevant for the analysis in the first chapter whilst the latter is likely to help explain women's behaviour in the second chapter.

In the first case, models predict that the increase in household income raises the marginal utility of having children and thus leads to a larger expected family size. The theory does not directly predict what happens to the timing of birth. However, if families are having more children overall, this should translate into shorter birth intervals, since women are fertile for a limited period of their lives. The second type of increase in the household income directly predicts the impact on the timing of birth. In particular, Heckman and Willis (1976) argue that when the household income profile increases over time, families delay birth in order to postpone child-bearing until the environment is favourable. They assume that households cannot borrow or save and this assumption is likely to be too restrictive. However, the model discussed in Cigno and Ermisch (1989) and Cigno (1991) provides an additional delaying incentive of a steep earning profile. In their formulation, women's human capital accumulation process is explicitly incorporated. In such a framework, women who face steeper earnings profiles delay birth, as women invest more to form human capital in the early part of their lives.

On the other hand, models that allow for a stochastic fertility process give mixed directions of the impact of changes in the wage level on the timing of birth. (e.g. Heckman and Willis (1976); Wolpin (1984); Rosenzweig and Schultz (1985); Newman (1988) ; Leung (1991)). Under this setting, couples control their fertility by the use of contraception methods. On the one hand, the direct income effect leads couples to reduce the level of contraception and have birth earlier. On the other hand, the impact through the substitution effect is ambiguous depending on the assumption regarding the relationship between children and mother's leisure time. If having children is assumed to be strongly complementary to consumption, an increase in the wage leads women to shorten their time to birth. However, if having children is complementary to leisure, the opposite effect is predicted.

These elements from the above analysis form the theoretical predictions in each subsequent chapter. The mixture of the above effects generally gives us ambiguous predictions of the impact of the policies and, thus, leaves the identification of the policy impact to empirical investigations.

There are many studies that have looked at the impact of lump sum cash transfers on fertility. Moffitt (1997) summarises the US evidence from the 1970s and 1980s whilst Gauthier (2007) reviews evidence from Canada and Europe. However, the more relevant papers to the first chapter are those that investigate policies that affected both the labour supply as well as fertility. This is because the UK WFTC in the first chapter was an in-work cash transfer that also changed the labour supply of the affected households. Women's labour supply is an important factor that determines their opportunity cost of maternal time. In the case of increasing female labour supply, therefore, it is likely to deter women from entering motherhood or having subsequent children. This implies that the sizes of the effects of WFTC are likely to be smaller compared to those observed under the lump sum cash transfer policies.

Previous literature on the impact of the UK WFTC investigates the impact on fertility separately for single women (Francesconi and Van der Klaauw, 2007) and for women in couples (Francesconi, Rainer, and Van Der Klaauw, 2009; Brewer, Ratcliffe, and Smith, 2010). Francesconi and Van der Klaauw (2007) and Francesconi et al. (2009) use 1991-2001 British Household Panel Survey and identify the policy impact by using a difference estimator. Brewer et al (2010), on the other hand, use two repeated cross sectional datasets, the Family Resources Survey and the Family Expenditure Survey. They employ a difference-in-differences estimator. For single women, Francesconi and Van der Klaauw (2007) find insignificant delaying effects of WFTC for both the single women's entry to motherhood and lone mother's probability of subsequent births. For women in couples the results are mixed. On the one hand, Francesconi et al. (2009) show that the entry to motherhood and mothers' subsequent probability of birth both indicate insignificant delays. On the other hand, Brewer et al. (2010) show that the probabilities of all birth parity increased due to the introduction of WFTC and the impacts are particularly large and significant for the first and third births. In addition, Brewer et al. (2010) show that the affected women reduced the age at first birth, albeit insignificantly.

Studies on similar policies also exist in the US. Papers by Duchovny (2001) and Baughman and Dickert-Conlin (2003, 2009) all investigate the impact of the increases in the amount of US Earned Income Tax Credit on fertility. The EITC is also an in-work tax credit with similar features and eligibility conditions as those of WFTC. The EITC went through several reforms, which increased the amount of tax credit, and thus provided positive incentives to give birth. In all these papers, repeated cross sectional data is used for the analysis. Duchovny (2001) uses the 1989-2000 March Current Population Survey and Baughman and Dickert-Conlin (2003, 2009) employ the US birth certificate data between 1990 and 1999.

Results from these US studies are also mixed. For the impact on first birth, Baughman and Dickert-Conlin (2003) find a negative effect on the birth rate for white single women. They find positive impacts for all the other demographic groups. However, Baughman and Dickert-Conlin(2009) report negative impacts of the EITC reforms for all birth rates regardless of women's race or marital status. The impacts on the second birth estimated in Duchovny (2001) reveal positive impacts for non-white single women and white married women. On the other hand, Baughman and Dickert-Conlin (2009) show that only a very small positive effect is found for non-white single women and all the other women experienced reductions in the birth rates in response to the increase in the amount of EITC.

Finally, a recent Spanish study by Azmat and González (2010) looks at the impact of the 2003 Spanish tax reform. By employing repeated cross sectional data, Spanish Labour Force Survey, they identify the policy impact through the use of a difference estimator with linear or quadratic trends. Their linear probability model shows that the Spanish tax reform significantly increased the probability of first birth and the second and subsequent birth also insignificantly increased.

Given the mixed results, it is crucial to further investigate the impact of WFTC and the findings from the first chapter add complementary evidence to the existing literature. Compared to the above studies, the first chapter incorporates the following three novel features simultaneously for the first time.

Firstly, this chapter presents detailed analysis on the impact of WFTC on the timing of births using a multiple-spell discrete-time proportional hazard model with unobserved heterogeneity. As discussed earlier, studying the timing dimension allows us to enrich the existing literature that almost exclusively focuses on the impact on the probability of birth. The timing effect is uncovered by calculating the probability of birth conditioning on not giving birth in the previous periods. This presents a stark contrast to the probability of birth model employed in other papers, which instead estimates the unconditional probability of birth. The hazard model employed in the first chapter makes it possible to study how the risk of birth changed over time for a group of women under study. Whilst some papers investigate the policy impact on the timing of birth by evaluating the change in the age at first birth (Baughman and Dickert-Conlin (2009) and Brewer et al (2010)), such a model fails to take account of women who did not give birth. The exclusion of these right censored observations from the age at first birth model is likely to bias the estimates. The hazard model employed in this chapter, on the other hand, explicitly takes account of the censored observations in the specification. Moreover, compared to the age at first birth model, the multiple-spell model with unobserved heterogeneity allows us to study the impact on the timing of second and subsequent births whilst taking account of potential correlations across birth intervals.

Secondly, unlike most of the previous studies, this chapter uses a panel dataset. Although the generally larger sample size is a particularly attractive feature of repeated cross sectional data, these women are only observed once on an interview day. As a result, the individual characteristics from the particular interview day are often used for the analysis. In contrast, it is possible to follow the same set of individuals when panel data is employed. Using this feature, demographic characteristics prior to birth can also be observed. Since a birth is an outcome, which is observed several years after the decision for conception is made, it is particularly important to control for demographic characteristics prior to each birth. Moreover, the individual fertility history is easily uncovered and the correct birth parity for each woman can be obtained. Given that the probability of birth is likely to differ across parity, this ensures accurate selection of women for the analysis.

Thirdly and lastly, compared to some studies that use a difference estimator (i.e. Francesconi and Van der Klaauw, 2007; Francesconi et al., 2009; Azmat and Gonzalez, 2010), this chapter uses a difference-in-differences estimator. A difference estimator identifies the policy impact by comparing the probabilities of birth before and after the policy introduction. The identification strategy requires that there are no underlying macro trends aside from the policy introduction itself. When there are other trends, the estimated effect would also reflect such macro trends on top of the policy effect. To control for this potential trends, these studies include linear or quadratic trend terms. A difference-in-differences estimator employed in the first chapter controls for the trend in a more flexible manner by using a group of women, which shares similar characteristics but is not affected by the introduction of WFTC.

Turning to the relevant literature for the second chapter, the majority of papers looked at the impacts of the US infertility health insurance mandates among women older than 35.

Schmidt (2005; 2007) investigate how the proportion of older women giving first birth changed due to the introduction of the mandates by using a triple difference estimator. To identify the policy impacts, she compares women aged above 35 to those who are younger, assuming that the younger women are unaffected by the mandates. She argues that the improved accessibility of infertility treatments may have increased the chance that older women give births. Using 1985-1999 Vital Statistics Detail Natality Data and the Census Bureau, she finds that the introduction of mandates increased older women's first birth.

Bundorf, Henne and Baker(2007) investigate how the mandates affected the access to and the aggressiveness of Assited Reproductive Technologies (ART) by studying the change in the probability of multiple birth. Using the 1981-1999 Vital Statistics Natality Birth Data and the 1989-2000 registry data from the Society for Assisted Reproductive Technologies, they estimate the policy impact using a difference-in-differences estimator. They conclude that the mandates increased the utilization of ART, however the aggressiveness of the treatment did not change even after the introduction of the mandates.

Bitler (2008), on the other hand, studies potential consequences of wider availability of infertility treatments. She looks at whether the mandates changed the rate of multiple births and the child health outcomes. She finds no effect of mandates on birth outcomes for young women aged below 30, but results show some negative impacts on the birth outcomes of the twins and singletons among older women.

Whilst these past studies have focused on how the mandates affected an older group of women, it is also possible that these state mandates influenced women who were considering a potential use of treatment in the future. In other words, the introduction of mandates may have encouraged younger women to delay giving birth.

Buckles (2007) investigates whether women delayed births in response to these policies. Using the 1982-1999 Current Population Survey, she first estimate how the probability of first birth of older women aged between 35 and 44 changed before and after the introduction of mandates using a difference-in-differences estimator and finds that women residing in mandated states increased the first birth rate after five years of coverage. She then runs a separate regression to see at how the birth rates of younger women were affected. The estimates suggest that women aged between 22 and 25 as well as 26 and 30 both decreased the birth rates after five years of coverage. Bundolf et al. (2007) also devotes a small section to this issue and presents similar difference-in-differences estimates which indicate that the birth rate of women aged 25-29 decreased while it increased for women aged 35-39. Given these estimates, they conclude that there was a delaying of birth among younger women.

The second chapter in this thesis makes several contributions to the existing literature. Firstly, it provides additional evidence to Buckles (2007) and Bundolf et al. (2007) by further analysing the impact of the mandates on the younger women's timing of birth. This is important, since studying such an unintended policy impact allows us to comprehend the wider implications of a policy. In particular, the larger proportion of women delaying birth potentially leads to increased risks of birth complications among both mothers and children as such risks are strongly correlated with the age of mothers. The analysis of the timing of birth also contributes to the understanding of the previous results. If women in the mandated states were indeed delaying their timing of birth, findings from Schmidt (2005; 2007) and Bundorf et al. (2007) not only show increases in the number of first births due to more easily accessible treatment but also reflect more women at older ages giving birth because of their planned delay of birth. Moreover, the negative birth outcomes found among older women in Bitler (2008) may have also been due to more women giving birth at a later age. Studying the delaying effect is also interesting from the perspective of the fertility literature as the mandates allow us to study how women respond to changes in the long term earning profile.

Secondly, this thesis uses a single-spell discrete-time proportional hazard model with unobserved heterogeneity, which is better suited for the studying of the timing of birth. Buckles (2007) and Bundolf et al. (2007) make an implicit assumption that the fertility behaviour of the older cohort of women is a good proxy for that of the younger cohort of women in 10-20 years. Given that the women in the 1980s and 1990s went through drastic changes in their life styles, such an assumption is likely to be violated. The empirical specification used in the second chapter relaxes such an assumption by instead following the same women over years to study the delay of birth timing.

Findings from this thesis suggest that a policy that changes household income in the short term does have an impact on the timing of birth, although the size of this effect is small, which at least partially is likely to be due to the positive impact of the policy on the female labour supply. On the other hand, changes in the long-term earning profile together with the effect of the policy to reduce the dilemma between work and family seem to have a relatively large timing effect. In both cases, the results reconfirm the importance of female labour supply in determining the women's fertility behaviour.

1 Chapter One: Fertility timing response to financial incentives: Evidence from the Working Families Tax Credit in the UK

1.1 Introduction

The UK Working Families Tax Credit (WFTC) was introduced in October 1999, replacing the former tax credit, Family Credit (FC). Its central aim was to increase the returns generated from work for low income families with children. The two tax credits were similar in structure, but WFTC was considerably more generous compared to FC.

Although the structure of the tax credit was designed to encourage parents into work, larger credit and childcare cost support may have had an unintentional impact on the childbearing decisions of recipient families. The past literature primarily looked at the impact of WFTC on female labour supply but there has been limited research into how fertility behaviour has been influenced. An investigation of the fertility response would, therefore, provide a better understanding of the full impact of WFTC. Moreover, the labour supply analysis of WFTC typically assumes the absence of a fertility effect. Given that female labour supply is likely to be jointly determined with fertility, the reported labour supply effect may reflect not only the direct impact of work incentives on female labour supply but also the indirect effect of the changes in fertility.

There are several existing studies that investigated the fertility impact of policies that affected both the income as well as the labour supply of the households. In the UK, Francesconi and Van der Klaauw (2007), Francesconi, Rainer, and Van Der Klaauw (2009) and Brewer, Ratcliffe, and Smith (2010) investigate the impact of WFTC on fertility. There are also some US studies (Duchovny, 2001; Baughman and Dickert-Conlin, 2003; Baughman and Dickert-Conlin, 2009) that look at the effect of the US Earned Income Tax Credit, which is a tax credit that is similar in structure to WFTC. Lastly, Azmat and González (2010) investigate the impact of the Spanish income tax reform.

This chapter contributes to the literature by incorporating all of the following features simultaneously for the first time.

Firstly and most importantly, it provides a detailed analysis on the effect on the timing of birth using a multiple-spell discrete-time proportional hazard model with unobserved heterogeneity. Studying the timing effect is important because the observed effect on the probability of birth does not necessary indicate changes in the total fertility, but it may simply reflect changes in the timing of births. The probability of birth model, which is often used in other papers, looks at unconditional probability of birth in each period whereas the hazard model estimates the probability of birth in one period conditional on not giving births in the previous periods. The former model typically looks at how the birth outcome for a particular age group of women changes over time. However, under this framework, women from different cohorts are compared over years. On the other hand, the latter formulation allows us to study how the risk of birth changes over time for the same group of women. Whilst some studies investigate the timing effect by looking at the changes in the age at first birth, such a model typically ignores individuals who did not give birth by the time they were interviewed. The hazard model employed in this chapter explicitly incorporates such right censored observations into the specification. Additionally, the hazard model with unobserved heterogeneity allows us to study the impact not only on the first birth but also on the subsequent births taking account of potential correlations across birth intervals.

Secondly, the analysis in this chapter uses a panel data set, the 1991-2003 waves of the British Household Panel Survey (BHPS). This is in contrast to the majority of the previous studies that use repeated cross sectional data. Cross sectional data only records children that were born before the interview date. By construction, therefore, the observed characteristics are collected after the births occurred. However, fertility decisions are typically made several years before the observed births. As a result, it is important to control for characteristics prior to births. In addition, the correct birth parity can be uncovered because panel data makes it possible to obtain fertility histories of individuals.

Lastly, it identifies the policy impact by using a difference-in-differences estimator. Compared to a difference estimator that controls for the macro trends by including linear or quadratic terms, the difference-in-differences framework controls for the trends in a more flexible manner.

It is worth noting that the UK saw several welfare policy reforms during the period of focus. For example, the generosity of universal Child Benefit as well as Income Support (IS) for unemployed couples and non-working lone parents with young dependent children increased at the same time as the introduction of WFTC. Moreover, labour market policies such as the national minimum wage and various New Deal policies were also implemented to "make work pay". Since the program evaluation method cannot disentangle separate policy effects, the estimated impact is likely to reflect not only the WFTC impact but also the effect from other policies. Interpretation of the reported results, therefore, should be done with this fact in mind.

The rest of the chapter is organized as follows. Section 1.2 provides background information regarding WFTC and other policies introduced during this period. The expected impact on the timing of birth is discussed in Section 1.3. Section 1.4 summarizes the past empirical literature. Section 1.5 and 1.6 describe the identification strategy and the econometric specifications while Section 1.7 discusses the data employed. Section 1.8 looks at the estimated results and further analysis is included in Section 1.9. Finally, Section 1.10 concludes.

1.2 Background information

1.2.1 The structure of Family Credit and the Working Families Tax Credit

The Working Families Tax Credit (WFTC) was introduced in October 1999 approximately 19 months after its announcement in March 1998, replacing the former Family Credit (FC). WFTC was the main in-work tax benefit for families with children in the UK until the April 2003 introductions of the two new tax credits, Working Tax Credit and Child Tax Credit. Since WFTC was preceded by FC, a study of the policy impact requires comprehension of the structure of WFTC in relation to FC.

Table 1.1 summarizes the eligibility conditions and each element of the two tax credits. Both FC and WFTC were granted to parents of low income families in which at least one parent was working 16 hours a week or more. The amount of both tax credits was contingent on the number and the age of children in each household as well as the level of household income. Additional credit was given to those families if at least one parent was working longer than 30 hours. For both tax credits, the maximum credits were given to families earning less than a threshold. Once the level of household income reaches this threshold, the credit was reduced at a specified rate.

Although these two tax credits had similar structures, WFTC was significantly more generous than FC. The generosity of WFTC stems from four main components. Compared to FC, the amount of maximum credit increased. Moreover, the weekly income threshold for the entitlement of maximum credit was raised from £79 to £90. Additionally, the deduction rate of the tax credit was reduced from 70 to 55%. Lastly, WFTC provided more financial support towards the childcare cost. The childcare cost subsidy was given to families if both parents were working more than 16 hours a week. Under FC, childcare cost up to £60 was disregarded from the family's income when the calculation was made. However, it was often criticized since families with maximum tax credits did not attain additional support for their childcare cost. WFTC, on the other hand, supported the cost more actively by providing subsidies for 70% of childcare cost. Moreover, the applicable age of children was raised from 11 to 15.

Figure 1.1 illustrates the structure of FC and WFTC. The initial difference in the amount of tax credit is due to the larger amount of each element of WFTC compared to FC. The threshold level was higher and deduction started later under WFTC compared to FC. The slower rate of deduction under WFTC is reflected in the less steep slope at the taper. The large jump in the amount of tax credits around the level of £115.2 is due to the childcare cost subsidies. Finally, a smaller jump seen around £170 is due to the additional awards given to families working 30 hours or more.

1.2.2 Other policies in the UK

Aside from the introduction of WFTC, the New Labour government also implemented various policies targeting low income families. These policies are relevant to the analysis in this chapter as they are likely to have also influenced the fertility behaviour of WFTC individuals. Identifying separate policy effects is beyond the scope of this chapter and, thus, the estimated results are likely to also reflect impacts from these other policies. Brief explanations of relevant policies are given below and the theoretical implications of these policies are discussed in Section 1.3.

The two reforms in 1999 that potentially reinforced the fertility incentives of WFTC are the increases in the amounts of means tested Income Support and universal Child Benefit, although the magnitude of the effects from these policy introductions are marginal compared to that of WFTC. The main recipients of Income Support are lone parents who are working less than 16 hours per week and with savings less than £16,000. It is given to individuals who are aged between 16 and pension entitlement age. Child Benefit, on the other hand, is a flat rate cash transfer which is paid per child. In 1999, the weekly amount of each of these benefits was increased. For instance, the amount of Income Support for children aged between 0 and 10 increased in real terms by £2.50. On the other hand, Child Benefit for the eldest child was also increased by £2.80 in real terms.

In addition, policies that are likely to have influenced women's employment were also implemented in 1998/1999. For example, New Deal policies for young people, lone parents and individuals aged above 25 were implemented in 1998. The New Deal for Young People is a mandatory program for individuals aged between 18 and 24 who claimed Jobseeker's Allowance for 6 months. On the other hand, New Deal for Lone Parents and 25 plus are voluntary program each targeting lone parents claiming Income Support and individuals aged above 25 who have claimed Jobseeker's Allowance for 18 months or more. The main aim of these New Deals is to assist the long-term unemployed to re-enter the labour market. On top of these New Deal policies, the national minimum wage was also introduced on 1 April 1999, following the recommendations from the Low Pay Commission. This policy particularly affected workers at the lower end of the wage distribution. The law sets separate minimum wage rates for adults and youths aged between 18 and 21. In 1999, the adult rate was £3.60 per hour whereas the youth rate was set at £3.00 per hour. These amounts were increased again in 2000 to £3.70 and £3.20 per hour for each group respectively.

1.3 Theoretical predictions

Assuming that WFTC was only a lump sum cash transfer that did not change labour supply of the affected families, the expansion of the amount of credit is likely to induce having an additional child earlier either to fulfill the eligibility condition for WFTC by having first child or to claim a higher amount by giving subsequent births.

Potential implications of WFTC are somewhat more complex due to the requirement of at least one member of the household to be in work. On the one hand, the increase in the household income is likely to have reduced the employment incentives of individuals through the income effect. On the other hand, the higher wage rates after WFTC are likely to have induced individuals to work longer hours through the substitution effect. Blundell, Duncan, McCrae, and Meghir (2000) applies a discrete choice structural approach on the 1994-1995 and 1995-1996 Family Resources Survey (FRS). Their simulation results suggest positive policy impacts on lone mothers' number of hours worked as well as their participation rate. They also observed that the policy increased both the number of working hours and participation of women with unemployed partners. On the other hand, women with employed partners are estimated to have reduced their hours and participation rate. Brewer, Duncan, Shephard, and Suarez (2006) also use a structural model of labor supply using FRS data pre and post reform and find very similar results to those in Blundell et al.(2000). Blundell, Brewer, and Shephard (2005) apply the difference-in-differences estimator using FRS and the Labour Force Survey to carry out an ex-post analysis. Their results suggest that lone mothers experienced a positive employment effect while married mothers saw little change in their participation rates. Francesconi and Van der Klaauw (2007) and Francesconi, Rainer, and Van Der Klaauw (2009) each investigate the transition rate into and out of the employment among lone mothers and women with partners, respectively. They employ the 1991-2001 BHPS and identify the policy impacts by using a triple differences estimator. Francesconi and Van der Klaauw (2007) find a significant increase in the employment rate among lone mothers, which was due to a higher rate of entry into as well as a lower rate of exit out of employment. Francesconi, Rainer, and Van Der Klaauw (2009), on the other hand, report insignificant impact on mothers with partners, but find stronger positive effect among women with low earning partners. In summary, empirical evidence suggests a significantly positive labour supply effect of WFTC among lone mothers and women with unemployed partners, whilst evidence shows a limited effect for women with working partners. The estimates from these studies range between 2.2 and 7 percentage points increases for lone mothers; 0.1 and 4.8 percentage points for women with unemployed partners; -0.6 and 2.7 percentage points for women with employed partners.

The above analysis exclusively focuses on the potential impacts of WFTC. However, as discussed in Section 1.2.2, this period in the UK saw other employment related policies that also affected childless households.

Firstly, the 1999 national minimum wage raised the wage rates of low income households but may have also reduced the employment opportunities. The impact on labour supply, however, is likely to be limited. Machin, Manning, and Rahman (2003) and Machin and Wilson (2004) studied the impact on particularly low paid workers, those in the care homes sector, and found reduction in employment but the magnitude of such an effect was small. Stewart and Swaffield (2008), on the other hand, find small but negative impact of the minimum wage on the number of working hours. However, empirical evidence from the other UK studies almost uniformly reports no adverse effects of the national minimum wage on the employment rate (Stewart, 2002) nor the probability that employed individuals remain employed for all demographic groups, including female workers (Stewart, 2002; Stewart, 2004b; Stewart, 2004a; Connolly and Gregory, 2002).[1] In the case of relatively limited labour supply effect, the increased household income is likely to have shortened birth intervals. If women were in employment, they would additionally face the substitution effect, which would have worked to delay births due to the increased opportunity cost of having a child.[2]

Secondly, the New Deal policies are also likely to have offered improved labour

[1] Metcalf (2004) provides several most probable reasons for the lack of strong negative labour market impacts. In particular, he points out that firms may have increased the labour productivity and effort. Moreover, he suggests that the firms may have passed on the increased cost through higher prices and lower profits in response to the introduction of the minimum wage.

[2] Currently, there is no empirical evidence on the impact of minimum wage on fertility.

market opportunities. Blundell, Dias, Meghir, and van Reenen (2004) and Wilkinson (2003) both report significantly positive employment effects of the New Deal for Young People among the male participants. Although only 25% of the participants were women, these studies also find a positive employment impact among women. Additionally, the New Deal for Lone Parents is also found to have had a positive employment effect (Evans and Britain, 2003). Similarly to the case of the national minimum wage, if New Deals affected women's partners, this would shorten the birth intervals through the income effect. If, on the other hand, women increased their labour supply, the substitution effect would prolong the intervals.

Aside from the impact on fertility and labour supply, the WFTC and other policy introductions may have had an additional unintended impact on the partnership formation of affected individuals, although the potential direction of the impact is ambiguous. Partnership opportunities may have been positively pursued for the purpose of additional childbearing. At the same time, policies introduced during this period may have made partnership less attractive. The higher tax credits under WFTC may have enabled individuals to maintain their lives in the absence of the financial assistance from their partners. Additionally, the improved labour market opportunities due to various policies as well as the childcare credit element of WFTC that made childcare affordable are likely factors that potentially have accelerated this trend. Francesconi and Van der Klaauw (2007) report a statistically significant reduction of partnership formation among lone mothers whereas Francesconi, Rainer, and Van Der Klaauw (2009) report a significant increase in the divorce rate among women with partners working less than 16 hours. Both effects were particularly strong for women with young children where the impact of WFTC is likely to be the strongest.

Summing up the above, the potential overall impact on the timing of birth is mixed and the WFTC incentive to shorten birth intervals is likely to have been offset, at least partially, by the increase in the labour supply of the affected women.

Single women and lone mothers were particularly prone to the positive labour supply effect. Therefore, the fertility timing is likely to be limited for these demographic groups of women. On the other hand, women with partners are less likely to be affected by the increase in the labour supply, but rather are likely to have benefitted from their partners' increased labour market participation. This, thus, implies a potentially stronger fertility effect among women in couples. However, the size of the shortening of birth interval impact is likely to be smaller for women with unemployed partners compared to those with employed partners, since the former group of women are observed to have increased their labour supply in response to the WFTC introduction.

1.4 Previous literature

There is a large literature studying the effects of various lump sum cash transfer programs on families' fertility decisions (see for example, Moffitt (1997) and Gauthier (2007) for summaries of the literature in the US and Europe respectively). However, what sets WFTC apart from the others is that it was not a simple cash transfer aiming to affect fertility but was also an in-work tax credit, requiring at least one member of the family to participate in the labour market. For this reason, only those studies that evaluated policies that simultaneously affected the labour supply and fertility behaviours are reviewed in this section.

There are three papers that investigate the WFTC impact on fertility. Francesconi and Van der Klaauw (2007) evaluate the impact of WFTC on single women, whilst Francesconi, Rainer, and Van Der Klaauw (2009), and Brewer, Ratcliffe, and Smith (2010) study the effect on women in couples.

Francesconi and Van der Klaauw (2007) study the effects of WFTC on single women. Employing the 1991-2001 BHPS, they separately estimate linear probability models to find out how WFTC affected single childless women's entrance to lone motherhood as well as lone mothers' probabilities of subsequent births. More specifically, the former group is studied by including a sample of childless women in year t who remained single for at least two periods. Similarly, women with children in period t who remained single for the minimum of two years were included for the study of lone mothers' transition to subsequent births. The policy impact was identified by using a difference estimator comparing each groups transition probabilities before and after the introduction of WFTC. The estimated results indicate insignificant 0.2 percentage points reductions in the risk of entering lone motherhood. This is approximately a 15 % decline in the risk. For single women with children, the risk of subsequent births is also insignificantly reduced by between 0.2 and 1 percentage points depending on the number and the age groups of previously born children.

Using the same data and the empirical specification, Francesconi, Rainer, and Van Der Klaauw (2009) look at the fertility impact on women who are in couples by studying the transition to first and higher order births. They find insignificant delay of first and higher order birth (-0.07 and -0.5 percentage points respectively).

Brewer, Ratcliffe, and Smith (2010) also study the WFTC impact on women with partners. In particular, by using two repeated cross sectional data, 1995-2003 Family Resources Survey and 1990-2003 Family Expenditure Survey, they estimate a linear probability model to study the effect on the probability of birth. Policy impact is identified using a difference-in-differences estimator comparing women with O-level or less education (treatment group) to those with A-level and more qualification (control group).[3] Contrary to the findings from Francesconi, Rainer, and Van Der Klaauw (2009), they find a significantly positive impact on the probability of first and third birth. Overall estimated impact on the probability of all birth parity is an increase of 15%. Additionally, they also studied the timing effect by looking at how age at first birth changed during the period of study. They find an insignificant negative effect on the age at first birth.

Studies on a similar policy also exist in the US. Papers by Duchovny (2001)

[3] An O-level is a UK qualification, which is usually taken at the age of 15 or 16. An A-level, on the other hand, is an advanced level UK qualification that is taken at the age of 18.

and Baughman and Dickert-Conlin (2003, 2009) all investigate the impact of the increases in the amount of US Earned Income Tax Credit (EITC) on fertility. Just as for WFTC, EITC requires parents' participation in the labour market as it is paid to tax filers.

EITC has gone through various reforms since its introduction in 1975. Prior to the Omnibus Budget Reconciliation Act (OBRA) 1990, the amount of the maximum EITC was fixed regardless of the family size. After 1990, the amount was made to depend on the number of children in the household and higher credits were given to families with two or more children. Moreover, the OBRA 1990 increased the amount of the base credit over the period of 1991-1993. The OBRA 1993 brought a further increase in the amounts of both the base and incremental elements, and these increases were implemented during the period of 1994-1996. As a result of these reforms, affected families were presented with additional incentives to have first and higher order births. In addition, the federal EITC reforms were accompanied by introductions of state level EITCs.[4] The amount of tax credits varies by state and are typically calculated as percentages of the federal EITC. The 1990 and 1993 OBRA together with the introductions of the state EITCs presented variations in the amounts of EITC across states over the years (for a detailed explanation of the structure and the reforms of EITC, see for example Baughman and Dickert-Conlin (2009)).

Duchovny (2001) focuses on the impact of EITC expansion on the probability of having a second child. Using the March Current Population Survey for the years between 1989 and 2000, she estimate linear probability models separately by marital status and race. A difference-in-differences estimator is employed comparing women with different parity and educational level. Baughman and Dickert-Conlin (2003), on the other hand, report the impact on the first birth whilst Baughman and Dickert-Conlin (2009) study the impacts separately for first and higher order births. Using

[4]Although there were only four states offering state-level tax credits prior to 1990, additional seven states introduced the tax credits between 1990 and 1999.

the US birth certificate data between 1990 and 1999, they calculate a birth rate, which is a ratio between the number of women who gave birth in a year and childless women, for each of the age-state-year-race-education cell. They use the amount of EITC in the previous year to identify the policy effect.

The US evidence on the impact of EITC on fertility is also mixed. For the first birth, Baughman and Dickert-Conlin (2003) find a negative effect of the increase in the tax credits on the birth rate for white single women, but find a positive impact for all the other demographic groups. However, Baughman and Dickert-Conlin (2009) report negative overall impacts of the EITC reforms for all birth rates regardless of women's race or marital status. In addition to the probability of birth analysis, Baughman and Dickert-Conlin (2009) also estimate how the age at first birth changed over time. The findings are consistent with those from the probability of birth analysis as they find a small but a significant delaying effect of the increase in the maximum amount of EITC. For the second birth, the reported estimates in Duchovny (2001) reveal positive impacts for non-white single women and white married women. However, Baughman and Dickert-Conlin (2009) show that only a very small positive effect is found for non-white single women and all the other women experienced reduction in the birth rates in response to the increase in the amount of EITC.

Finally, a recent Spanish study by Azmat and Gonzalez (2010) look at the impact of the 1999 and 2003 Spanish tax reform on the probability of birth.

Before 1999, the Spanish households with children received income tax credits. The 1999 reform replaced the tax credits with the "child reductions", which was deducted from the taxable income. The amount of the reductions was dependent on the number of children in each household. Due to this reform, families with children saw a reduction in the amount of income tax and were presented with the incentives to have additional children. The 2003 reform further increased the amount of the "child reductions", but also introduced a new tax credit for employed mothers with children under the age of three. Azmat and Gonzalez (2010) exploit the increase in the financial incentives that encourage childbearing and female labour market attachments and study the impact on the probability of birth. They use the 1992-2008 Spanish Labour Force Survey, which is a repeated cross sectional data set, and identify the policy impact by using a difference estimator. The estimates from a linear probability model indicate a positive and significant impact of the 1999 reform for the probability of third birth (i.e. 12%). They also report a significant increase in the probability of first birth (i.e. 7%) as a result of the 2003 reform.

In summary, the existing papers present mixed evidence of the policy impacts. It is, therefore, crucial to further investigate the impact of WFTC and the findings from this chapter add complementary evidence to the existing literature. Compared to the above studies, the second chapter incorporates the following three novel features simultaneously for the first time.

Firstly, this chapter studies the impact of WFTC on the timing of birth by using a multiple-spell discrete-time proportional hazard model with unobserved heterogeneity. The timing effect is studied by calculating the probability of birth conditioning on not giving birth in the previous periods. This is in contrast to the probability of birth model employed in most of the previous papers, which instead estimates the unconditional probability of birth. Whilst some papers investigate the policy impact on the timing of birth by evaluating the change in the age at first birth (i.e. Baughman and Dickert-Conlin (2009) and Brewer et al (2010)), such a model fails to take account of women who did not give birth by the time of the interview. The exclusion of these right censored observations is likely to bias the estimates. The hazard model employed in this chapter, on the other hand, explicitly takes account of the censored observations in the specification. Moreover, in contrast to the age at first birth model, the multiple-spell model with unobserved heterogeneity allows us to study the timing impact of the second and subsequent births whilst taking account of potential correlations across birth intervals.

Secondly, unlike most of the previous studies, this chapter uses a panel dataset. In repeated cross sectional data, women are only observed once on an interview day. As a result, the individual characteristics from the particular interview day are often used for the analysis. On the other hand, it is possible to follow the same individuals when panel data is employed. Using this feature, demographic characteristics prior to birth can also be observed. When analysing the fertility behaviour, it is particularly important to control for individual characteristics prior to births. This is because a birth is an outcome, which is observed several years after the decision for conception is made. The panel data structure also allows us to observe individual fertility history and the correctly identify birth parity for each woman. Given that the probability of birth is likely to differ across parity, this ensures accurate selection of women for the analysis.

Thirdly, compared to some studies that use a difference estimator (i.e. Francesconi and Van der Klaauw (2007); Francesconi et al. (2009); Azmat and Gonzalez (2010)), this chapter uses a difference-in-differences estimator. A difference estimator identifies the policy impact by comparing the probabilities of birth before and after the policy introduction. This identification strategy fails if there are other macro trends. In such a case, the estimated effect would also reflect the trends on top of the policy effect. To control for this potential trends, these studies include linear or quadratic trend terms. In contrast, a difference-in-differences estimator employed in the second chapter controls for the trend in a more flexible manner by using a control group of women, who shares similar characteristics but is not affected by the introduction of WFTC.

1.5 Identification strategy

One potential way to identify the policy impact is to compare the birth intervals of affected individuals before and after the introduction date. This, however, is problematic if there are other factors affecting the birth durations over time. In such a case, the estimated impact would include not only the policy effect but also other macro trends. Instead, this chapter uses a difference-in-differences estimator in order to isolate other effects and identify the policy impact by defining a comparison group which includes women who have similar characteristics but were unaffected by WFTC. Using this comparison group, the policy impact is uncovered by evaluating how the differences in the timing of birth between the two groups of women changed before and after 1999.

One of the eligibility condition of WFTC requires that individuals come from low income households. The use of a household income variable would, therefore, ensure correct selection of individuals who were actually qualified to receive WFTC. However, the variable is likely to be endogenous, since WFTC directly affected household income. Moreover, it is likely to be simultaneously determined with fertility if female labour supply is reduced due to pregnancy. As a result, an educational variable is used instead to proxy for the level of household income. More specifically, individuals with O-level or less education are included in the treatment group.

Defining the control group is more problematic. One crucial assumption for the difference-in-differences is that the two groups of women experience a common trend in the absence of the policy. This assumption poses a difficulty in the context of WFTC. Since it was introduced at the national level, there are no counterparts with similar demographic characteristics. Therefore, although less than perfect, women with more than O-level education are used as the control group. These women may also be affected by WFTC, but they are much less likely to receive large amounts of WFTC compared to those in the treatment group.

To see how women's timing of birth differ by educational attainment, Figure 1.3 presents the trends of mean age at first birth between 1991 and 2003. The thick line shows the trend for women with O-level or less education (i.e. women in the treatment group). The dotted line presents the trends for women with A-level or more but below first degree qualification and thin line indicates the trends for women with more than first degree. These statistics are calculated using the 1991-2003 raw BHPS dataset. Comparing the three lines, trends of the age at first birth seem to be relatively stable for all groups of women. The trend for the control group is the most stable over the years with a slight upward trend after 1999. Women in the treatment group seem to have experienced an upward trend that is particularly evident from around 1998. Finally, women with more than first degree experienced the largest increase in the age at first birth. This group of women are least likely to have been affected by any of the UK in-work benefits and thus the observed increase is likely to be due to other factors. One possible cause is the drastic increase in the participation of female students in further education. The Department for Children, Schools and Families reports that the number of female students enrolled in the further education increased from 352,800 in 1990/1991 to 794,000 in 2004/2005 academic year. The size of expansion is particularly large among postgraduate degrees where the number of females increased by approximately three times (from 33,800 to 114,200) in 2004/2005 compared to 1990/1991. If this, indeed, was the factor for the increase in the age at first birth among highly educated women, it is unlikely that such trend is shared with low income women. To ensure similarities between the two groups as much as possible, women with more than first degree are excluded from the control group.

1.6 Empirical specification

Birth is a continuous and sequential process where the first birth is followed by the second and third births. However, the BHPS only collects the birth month and year of each child. Reflecting the multiple-spell structure of birth processes together with the grouped nature of the data, a multiple-spell discrete-time proportional hazard model is employed. Time of birth is studied for a sample of childless women to have up until their third child. This chapter considers the time until first birth as the first spell, and the time to second and third births as second and third spells respectively.[5]

In the following section, the subscripts i, k, j are used to each denote the ith individual, kth spell (where 1≤k≤3), and jth period. The grouped nature of the data implies that the kth birth is recorded to have been given in the jth period if she gave birth on the continuous time scale between $(j-1)$ and j of the kth spell. Some individuals experience all spells (i.e. have a total of three children) whilst others only go through some of the spells. Only those individuals who complete the kth spell proceeds to the $k+1th$ spell. J_{ik} is used to describe the last time period that individual i is observed for the kth spell. As an example, Figure 1.2 illustrates the fertility processes of two individuals (i=1 and 2). Individual 1 begins her fertility process in a calender year τ_0 when she is childless and experiences three spells. She completes her first spell (i.e. has her first child) after five periods (J_{11}=5 in k_1=1 spell) and the second spell in the fourth period of the second spell (J_{12}=4 in k_1=2 spell). She finally finishes her process by having her third birth in the fourth period of the third spell (J_{13}=4 in K_1=3). On the other hand, the second individual begins her process in another calender year τ_1 and completes her first spell after the seventh period (J_{21}=7, k_2=1). Although she enters her second spell, she does not give birth to her second child and reaches the end of the observation period T. Her last spell, therefore, is K_2=2 and her observation is right censored.

The underlying continuous hazard is given in Eq. 1.1 for an individual i, in the jth period of the kth spell. The underlying hazard is the instantaneous rate of having kth child in period j conditioning on her only having $k-1$ child in period $j-1$.

$$\theta_{ik}(j|x,\beta) = \lambda_k(j)exp[x_{ik}(j)'\beta]v_i, v_i > 0 \tag{1.1}$$

where $\lambda_k(j)$ is the spell-specific baseline hazard and v_i denotes the individual spe-

[5]The analysis in this section is based on Jenkins (2005) and Willett and Singer (1995).

cific unobserved heterogeneity, which is spell invariant. Inclusion of unobserved heterogeneity is important, since uncontrolled unobserved heterogeneity would cause spurious negative duration dependence as those with higher hazards tend to exit first (Lancaster and Nickell, 1980; Van den Berg, 2001). Moreover, controlling for the unobserved heterogeneity addresses the potential inter-birth spell correlations. This is done by assuming that the individual specific effect does not change across spells (e.g. individuals' preferences for birth are stable from time to first until third birth) and such fixed individual characteristics are the source of correlations.[6]

The discrete hazard function is given in Eq.1.2.

$$\begin{aligned} h_{ik}(j|x_{ik}(j), v_i) &= P[T_k = j|T_k > j-1, x_{ik}(j), v_i] \\ &= 1 - exp[-\int_{j-1}^{j} \theta_{il}(l)dl] \\ &= 1 - exp[-exp(x_{ik}(j)'\beta + \gamma_k(j) + lnv_i)] \end{aligned} \tag{1.2}$$

where

$$\gamma_k(j) = ln[\int_{j-1}^{j} \lambda_k(l)dl] \tag{1.3}$$

where $\gamma_k(j)$ denotes a spell-specific piece-wise constant baseline hazard, which is estimated by including a set of period and spell-specific indicator variables. The $x_{ik}(j)$ contains individual characteristics together with the following three variables which are crucial for the identification of the policy impact. $Treatment_i$ is a time-invariant dummy variable, which equals 1 if the ith woman has O-level or less

[6]Spell invariant unobserved heterogeneity may be a rather restrictive assumption if, for example, individuals' preferences for second and third children change when they have their first child. To ensure tractability, only a time invariant unobserved heterogeneity is included in this chapter. This, therefore, implies that the unobserved heterogeneity only captures the time invariant individual fertility characteristics such as the level of fecundity or fixed preferences towards having children.

qualifications in the initial period and 0 if she has A-level or more but less than first degree. This variable picks out the constant difference between the two groups of women. $After_j$ is an indicator variable for the period after 1999 to control for the potential macro difference before and after the introduction of WFTC. Finally, a variable interacting the previous two, i.e. $Treatment_i * After_j$, captures the policy impact. These three variables are separately estimated by parity to allow for the impact to differ by birth order. Under the specification in Eq. 1.2, these covariates proportionally influence the baseline hazard function. The shape of the spell-specific baseline hazard, however, is common between the two groups of women.

One advantage of studying the timing response by using duration analysis framework is that it clearly addresses the differential probability of birth depending on the length of the prior time periods that women remained childless. For example, an individual who remained childless until the age of 20 is likely to experience a differential probability of first birth compared to women who is without a child at the age of 30. In the duration analysis framework, such duration dependence is addressed by the inclusion of the baseline hazard, $\gamma_k(j)$. More specifically, each period specific dummy calculates the baseline hazard rate of giving kth birth in period j, provided that she remained childless until j-1th period. In addition, the duration dependence is likely to differ across birth parity. In order to address the differential duration dependence across parity, the spell specific baseline hazard allows the duration dependence to be estimated separately for each birth order.

The direction of the duration dependence is not clear. On the one hand, women may experience increasing hazard rate the longer they stay without giving births. This would imply positive duration dependence. On the other hand, the level of fecundity goes down with age. As a result, the hazard rate is likely to exhibit negative duration dependence as women get older. Previous findings seem to suggest positive duration dependence for the time to first birth (e.g. Neumark (1992), Newman and McCulloch (1984), Heckman and Walker (1990)), with an exception of Heckman, Holtz, and Walker (1985), who find negative quadratic duration dependence. For subsequent births, the evidence is divided. On the one hand, Heckman and Walker (1990) suggest positive duration dependence. On the other hand, however, Newman et al. (1984) and Heckman, Holtz, and Walker (1985) report negative quadratic duration dependence.

The discrete-time survival function, which describes the probability that individual i does not have birth between the period 1 to J_{ik} is given by

$$\begin{aligned} S_{ik}(J_{ik}) &= \prod_{j=1}^{J_{ik}} P[T_k > j] \\ &= \prod_{j=1}^{J_{ik}} (1 - h_{ik}(j)) \end{aligned} \tag{1.4}$$

On the other hand, the discrete density, which is the probability of remaining childless until $J_{ik} - 1$ and having the kth child in period J_{ik}, is given by

$$\begin{aligned} f_{ik}(J_{ik}) &= P[T_k = J_{ik} | T_k > J_{ik} - 1] \prod_{j=1}^{J_{ik}-1} P[T_k > j] \\ &= h_{ik}(J_{ik}) \prod_{j=1}^{J_{ik}-1} (1 - h_{ik}(j)) \end{aligned} \tag{1.5}$$

Let c_{ik} be an indicator variable that equals one if the kth spell of individual i is censored and zero if uncensored. Then the likelihood function, ignoring the unobserved heterogeneity v_i is

$$
\begin{aligned}
L &= \prod_{i=1}^{n}\prod_{k=1}^{K_i}[(f_{ik}(J_{ik}))^{1-c_{ik}}(S_{ik}(J_{ik}))^{c_{ik}}] \\
&= \prod_{i=1}^{n}\prod_{k=1}^{K_i}[h_{ik}(J_{ik})\prod_{j=1}^{J_{ik}-1}(1-h_{ik}(j))]^{1-c_{ik}}[\prod_{j=1}^{J_{ik}}(1-h_{ik}(j))]^{c_{ik}} \\
&= \prod_{i=1}^{n}\prod_{k=1}^{K_i}[(\frac{h_{ik}(J_{ik})}{1-h_{ik}(J_{ik})})^{1-c_{ik}}][\prod_{j=1}^{J_{ik}}(1-h_{ik}(j))]
\end{aligned}
\tag{1.6}
$$

Let y_{ikj} be an indicator variable which shows whether birth is observed in each period of the individual i's observations. If individual i's kth spell is censored (i.e. $c_{ik} = 1$), then y_{ikj} is zero for all periods for the spell. On the other hand, for the uncensored spells, y_{ikj} is zero for all periods apart from the last period of the spell, J_{ik}. Using y_{ikj}, Eq.1.6 can be rewritten as

$$
L = \prod_{i=1}^{n}\prod_{k=1}^{K_i}\prod_{j=1}^{J_{ik}}[h_{ik}(j)]^{y_{ikj}}[1-h_{ik}(j)]^{1-y_{ikj}} \tag{1.7}
$$

The above likelihood ignores the unobserved heterogeneity. Since v_i is unobserved, however, it needs to be integrated out to marginalise the likelihood function. The likelihood, thus, becomes

$$
L = \prod_{i=1}^{n}(\int[\prod_{k=1}^{K_i}\prod_{j=1}^{J_{ik}}[h_{ik}(j)]^{y_{ikj}}[1-h_{ik}(j)]^{1-y_{ikj}}]f(v_i)dv_i) \tag{1.8}
$$

Marginalisation of the likelihood function requires an assumption which imposes a particular distribution for the density of v_i. In this chapter, v_i is assumed to follow a normal distribution, although using a gamma distribution gives almost identical estimates. This is not surprising, since identification of the model under multiple-spell setting is independent of the choice of mixing distribution in the absence of lagged duration dependence (Honoré, 1993).

The above expression does not have a closed form solution. Instead, it is approximated by adaptive quadrature, which replaces the continuous $f(v_i)$ with a discrete distribution using m possible values from v_i. Then the parameters are estimated by finding the values of β, $\gamma_k(j)$ (from Eq. 1.2) and the variance of $f(v_i)$, σ_v^2. The reported estimates set m to be 12. The number of points was altered between 8 and 24 to check the robustness of the estimates, but this only had a very minor effect on the size of the estimates (i.e. less than 3 decimal places).

1.7 Data

The main data employed is the 1991-2003 BHPS. The BHPS is a nationally representative longitudinal data set collected annually since 1991. The panel consists of over 10,000 individuals from 5,000 UK households. The interviews were carried out at the household and individual level and include all individuals over the age of 16, who reside in the same house. Aside from the main BHPS file, this chapter also uses the BHPS consolidated marital, cohabitation and fertility file. This file contains lifetime fertility and partnership history information on the BHPS respondents. Since the main BHPS file only contains information on children who reside with their parents, the oldest child may no longer be recorded in BHPS if a woman gave birth at a very young age. The lifetime fertility information, thus, allows us to correctly identify the parity of each birth as well as the birth year. This is particularly important in the context of fertility analysis as the incentive to have additional children is likely to depend on the number of existing children. Similarly, partnership information is important when identifying the policy impact separately by marital status as the data allows us to find out when individuals started or ended their partnerships.

The years of observations used in this chapter is between 1991 and 2003. The cut off year 2003 was chosen in order to avoid including the impact of the two new credits, the Working Tax Credit and the Child Tax Credit, which were implemented since April 2003.

Observations are selected using a flow-sampling and are organized in a person-year format. The flow-sampling includes individuals whenever they enter a state of interest during the observation years and these individuals are followed until they either give birth or reach the year 2003. Under the alternative stock sampling scheme, all childless women in a particular year are included. In this chapter, the flow-sampling is chosen over the stock sampling, since the latter scheme implicitly collects individuals with longer durations. In other words, the probability of being included in the stock sample is higher for individuals with longer durations. The flow-sampling avoids such length bias. The person-year format implies that the same individual appears in the sample as many years until she either gives her third birth or she reaches the year 2003 with less than three births. The width of the step, a year, is decided in order to impose less parametric structure on the baseline hazard whilst having enough birth observations.

Reflecting the chosen sampling scheme, women who are childless at the age of 19 are included as soon as they turn 20 at any point between 1991 and 2003. The sample of women is followed until either the third births are observed or they reach the year 2003 with less than three children. The choice of the initial age 20 implies that the women are assumed to face the risk of conception from the age of 20. The age 20 is chosen as a starting point as more than 90% of women are childless at this age. Moreover, the sample of women is divided into the treatment and control group on the basis of their educational attainment. By the age of 20, it is clear whether they are pursuing further education or they have completed schooling before starting their first degree. Individuals who are long-term ill or in full-time education are excluded from the sample.

Due to the relatively small sample size, some individuals who were older than 20 in 1991 are followed back until when they were 20 as long as they were childless at the age of 19. The information from the BHPS consolidated marital, cohabitation and fertility file provides us with individual fertility and marital history and allows us to find out whether and when individuals gave birth or got married. As a result, the years of observations span between 1985 and 2003.[7]

The dependent variable is a binary indicator that equals one if the individual gave birth in a particular year. The demographic variables included are characteristics that are likely to affect fertility decisions such as ethnicity, age in the initial period and its square, as well as housing tenure. The regional dummies are also included in order to allow for differential characteristics across regions. The analysis uses a long panel of observations. Individuals who are 20 in 1991 may have different timing of birth compared to those who are 20 in 2000. In order to control for this differential fertility timing across cohorts, indicator variables that take account of when women entered the sample are also included.

Summary statistics are presented in Table 1.2. Table 1.2 show that the 7 % of control group women and 9 % of treatment group women give first birth. Moreover, out of those who gave first birth, nearly 17 % (6 %) of women give second (third) birth in each group. Table 1.2 also shows that affected women tend more likely to be non-white and less likely to own houses compared to those in the control group. The use of the educational variable to identify treatment and control groups seems to clearly separate lower income households from families with slightly higher income. Because of the evident disparities in some of the demographic characteristics between the two groups, further analysis is carried out in Section 1.9 to test whether the two groups of women experienced a similar trend before 1999.

[7]Although the inclusion of many years prior to the policy introduction may cause two groups to exhibit differential time trends, regressions estimated using data between 1991 and 2003 did not change the findings. These additional results are available upon request.

1.8 Results

1.8.1 Graphical examinations of the policy effects

Figure 1.4 presents the life table survival functions for each birth spell. The life table analysis allows us to visually examine the policy impact by using the sample of women discussed in Section 1.7.

The survival functions, indicating the proportion of women who remained childless until each period, are calculated separately by birth spells. Moreover, within each spell, the functions are presented separately by treatment status and comparing the rate of survival before and after 1999. More specifically, the left side panels ((a), (c), (e)) compare the survival functions of the treatment group before and after 1999. Similarly, the right hand side panel ((b), (d), (f)) show the survival functions for the women in the control group. In all these figures, lines indicate the proportion of women who remained childless until each period.

One thing to note is that the definition of the x axis, denoted as "period", differ according to the spell of interest. For the first spell analysis in panels (a) and (b), all women start their spells at the age of 20. Therefore, the values on the x axis imply the year since the age 20. On the other hand, the axis in panels (c) and (d) implies years since the observed first birth. Similarly, for the third spell, the values on the x axis in panels (e) and (f) indicate years since the observed second birth.

Looking at panel (a) in Figure 1.4, more women in the treatment group remain childless for a longer period after 1999. For example, in panel (a), the proportion of women in the treatment group who remained childless until the age of 27 before the year 1999 is approximately 80% of the sample. On the other hand, those who remain childless until the same age after the year 1999 is nearly 90% of the sample.[8]. On the contrary, panel (b) suggests that the control group women did not experience

[8]These statistics are calculated using the formula $\hat{S}(t) = \prod_{s=1}^{j}(1 - \frac{d_s}{n_s})$ where d_s and n_s each denotes the number of birth in a particular period and the number at risk at the start of each period, respectively. The proportions of birth for each parity reported in Table 1.2 are rough approximations for $\frac{d_s}{n_s}$.

significant changes in the timing of first birth before and after 1999. Assuming that these two groups of women followed similar trends in the absence of WFTC and the other policies introduced in 1999, these two figures indicate a potential delaying effect of the policies on the timing of first birth.

A similar analysis from panels (c) and (d) reveals an opposite story. Women in the control group, again, experienced only a marginal effect on the timing of second birth. However, those in the treatment group reduced the time to second birth. Combining the two panels, thus, presents evidence that the affected women shortened the time to second birth.

Lastly, potential changes to the timing of third birth can be found from panels (e) and (f). Here, unlike the other cases, the control group women exhibit differential survival rates before and after 1999. In particular, evidence seems to suggest shortening of the third spell during the post-reform period. On the contrary, women in the treatment group did not experience such a change. This could potentially indicate a delaying effect among the affected women. However, the small sample size of the third spell (only 5% of the sample proceeded to the third spell) raises doubts on the accuracy of the estimates to reflect the policy impact. Therefore, the interpretation of the third spell results needs to take this into account accordingly.

1.8.2 The regression results

Turning to the regression results, Table 1.3 shows the main results estimated using the multiple-spell discrete-time proportional hazard model with normally distributed unobserved heterogeneity. The reported coefficients on $Treatment_i \times After_j$ present the policy impacts by each parity. These coefficients scale the baseline hazard proportionally, keeping the shape of the baseline hazard constant. The negative coefficients imply delaying of birth as the hazard is lower due to the policy introduction.

Column (1) of Table 1.3 reports the estimates for all women regardless of their initial marital status. It is of interest not to condition on the marital status at first, since this allows us to understand the overall impact of policies on the affected women. The estimate for all women in column (1) suggests a statistically significant delay of the first birth. The timing of the second birth is positive, albeit insignificant. Although the results for the third spell should be interpreted with caution due to the small sample size, the reported coefficient is negative and thus suggests a delaying effect of the third birth.

Due to the nonlinearity of the hazard model, understanding the size of the estimated impact on fertility requires interpretation of the coefficients in relation to the baseline hazard. In order to do this, the hazard for each period is predicted with and without the policy impact coefficient on $Treatment_i \times After_j$ and by setting the continuous variables to the sample mean. Comparing the predicted hazard with and without the policy impact coefficient reveals a reduction in the probability of first birth by approximately 40%. This reduction in the hazard implies an average delay of first birth by approximately 1.7 years. Similarly, the estimates indicate approximately 40% and 28% increases in the probability of second and third birth, respectively. This increase in the probability suggests an average shortening of time to second birth by 1.8 years. Time to third birth, on the other hand, seems to have been delayed by approximately 8 months.

The column (1) of Table A.1 in the Appendix shows all the other estimates that are not included in Table 1.3. The estimated baseline hazard function for first spell suggests positive duration dependence until period 12 or until the age of 32. However, the hazard rate declines after period 12. Similar negative quadratic duration dependence can be observed for the second and the third spell. These estimates seem to confirm the prediction that younger women exhibit positive duration dependence whilst the hazard rate gradually falls once women start to experience reduced fecundity.

The theoretical predictions in Section 1.3 suggest differential policy effects by marital status. In the next analysis, therefore, policy impacts are separately estimated for women with and without partners.

Firstly, column (1) of Table 1.4 presents the estimated effect on the single women's entry to lone motherhood. A subgroup of women is selected from the "all women"sample discussed in Section 1.7. In particular, women are included in the subsample if they became 20 year old at any time during the observation years and also, if they stayed single throughout the first spell. Only the effect on the timing of first birth is estimated, since very few women continued to stay single to have second child.

Secondly, column (2) looks at the policy impact on lone mothers' subsequent birth timing. For this analysis, women with one child are included in the sample from the year that they are observed to have separated from their first partners. The year of separation is chosen as the initial period of observation, since women who have been separated for several years are likely to have a different probability of birth compared to women who recently became separated.

Lastly, the policy impact on women with partners is shown in column (3). Similarly to the "lone motherhood" sample, women are included in the sample from the year in which they are observed to have formed the first partnership conditional on being childless a year before. One thing to note is that apart from the first subsample (i.e. a sample to study the single women's entry to lone motherhood), neither the "lone motherhood" nor the "women in couples" samples condition on the individuals' marital status in subsequent periods. If, for example, WFTC affected the probability of remaining in the first partnership, such an effect would also be reflected in the reported policy impacts.[9]

Table 1.4 shows the results for each group of women. Theoretically, both the

[9]Due to the limited number of observations in the "single women" and the "lone mother" samples, regressions for these groups are estimated with a quadratic baseline hazard instead of the flexible piecewise constant baseline hazard. The quadratic baseline hazard is chosen, since the results are similar to those that are computed with the flexible piecewice constant baseline hazard. Although this imposes an additional parametric assumption on the baseline hazard, estimates can be obtained even when the sample size is relatively small.

single women and lone mothers were exposed to positive labour supply impacts of the UK policies during this period. It is, therefore, not surprising that the fertility impacts estimated for these groups of women are generally negative, although the estimates are all insignificant. The estimated policy impact on the timing of single women's entry to lone motherhood in column (1) suggests an insignificant reduction in the probability of first birth by approximately 25% or a delay of six months. Column (2), on the other hand, presents the policy impacts on the lone mothers' timing of second and third births. For this group of women, the estimated impacts indicate approximately 32% reduction in the probability of second birth or a 2-year delay. The estimated result for the timing of third birth, on the other hand, is an insignificant shortening of birth interval by 1 year.

Women in couples, in contrast, are less likely to have experienced the labour supply impact of the UK policies, particularly if their partners were employed. As a result, out of the three demographic groups, this group of women are expected to have been exposed to the strongest fertility incentives to shorten the birth intervals. The estimates in column (3) of Table 1.4 somewhat confirms the prediction, although all estimates again are insignificant regardless of the birth parity. The policies are estimated to have negatively affected the hazard for first birth by approximately 20%, which is an average delay of approximately 1 year. On the other hand, the estimated impact on the second birth seems to suggest an increase of hazard by approximately 50% or a reduction in the birth interval by 2 years. The birth interval to third birth also seems to have been shortened by 1.6 years, although just as all the other third birth estimates, the small sample size undermines the precision of the estimates.

1.9 Robustness check

1.9.1 Test for the common trend assumption

The results in the previous section reflect the policy impact only if women in the treatment group follow the same time trend as those in the control group in the absence of the policy introductions. Column (1) of Table 1.5 replicates the analysis of Table 1.3, but restricts the sample to only include observations prior to 1999 in order to test whether women experienced differential trends before the policy introductions.

Looking at the estimates presented in columns (1), the previously observed significant delaying of first birth was not present before 1999 for the "all women" sample. The size of the estimates for the time to second birth is also smaller and, thus, confirms the absence of differential trends.

1.9.2 Estimates with alternative sample division years

January, 1999 is approximately nine months after the policy announcement date, March 1998. If the individuals are assumed to have changed their fertility behaviour since the announcement date, any births after January 1999 would reflect the responses to the policy introduction. Therefore, the main analysis discussed in Section 1.8 uses the year 1999 to divide the sample into pre and post-reform periods. To check if the results indeed showed the impact of the 1999 reforms, additional analysis is carried out by replicating the results of Table 1.3, but using the year 1998 and 2000 as alternative division years. If the year 1999 was indeed the correct division year, dividing the sample by 1998 should reduce the size of the estimates, since the shift would allocate one-year worth of unaffected observations and label them as affected. On the other hand, using the year 2000 may increase the size of the estimates, since this allows the individuals one additional year to respond to the policy introduction. However, at the same time, moving the 1999 year data to the pre-policy period may reduce the size of differences between the before and after periods.

The results presented in column (2) of Table 1.5 tell us the same story as those in the main analysis in Table 1.3. As expected, the estimates with 1998 division year are generally smaller. Even when the year 2000 is used, the significant delaying of first birth is still present, although the sizes of impacts on the timing of the second and third birth both are now much smaller.

1.9.3 Further analysis

One somewhat surprising result from Section 1.8 is the strong delaying impact on the timing of first birth (i.e. column 1 of Table 1.3). This finding is the only result that is persistently found even when the definition of the division year is changed. Although the theoretical prediction suggests a mixed impact from WFTC alone, it is not clear why particularly the first birth was strongly affected. In particular, since these women are not readily eligible for WFTC, they do not face the major delaying incentive from the WFTC labour supply effect.

One potential explanation for this result is the influences from policies other than WFTC. In particular, these women may have increased employment due to other policies discussed in Section 1.2.2. Column (1) in Table 1.6 shows the estimated impact on employment among the childless women aged between 16 and 40. This result is estimated using the 1991-2003 Family Resources Survey, which is a large UK repeated cross sectional data. As expected, these childless individuals increased employment, although it is beyond the scope of this chapter to identify which of the policies introduced during this period were the cause of this outcome.

Results in columns (2) and (3), on the other hand, suggest that those already with one child did not increase employment after 1999, but women with two children increased employment insignificantly. Interestingly, these results coincide with our findings on fertility behaviour. In particular, estimates for the "all women" sample in column (1) of Table 1.3 suggest that women delayed first and third birth, but shortened the timing of second birth. Therefore these estimates confirm the importance of the labour supply effect at least partially when analysing the impact of WFTC and other policies on fertility.

1.9.4 Comparisons with the previous estimates

Comparing the estimates reported in this chapter to those in previous papers is difficult due to the differential choices of data and empirical specification. Nonetheless, this section attempts to check if the reported estimates from the UK literature reach any consensus.

Francesconi and Van der Klaauw (2007) and Francesconi, Rainer, and Van Der Klaauw (2009) are similar in spirit to this chapter as they also estimate the probability of birth conditioning on no birth in the previous periods.

Francesconi and Van der Klaauw (2007) report the estimated impacts separately for single women's entry to lone mothers and lone mothers' subsequent births. They suggest average declines in the probabilities of first and second birth by approximately 15% and 19%, respectively. In this chapter, Table 1.4 reports the relevant estimates. In particular, the estimates suggest a decline of the probability of first birth by 25%, whereas the impact on the second birth is a reduction of the probability by 32%.

For the analysis of women with partners, Francesconi, Rainer, and Van Der Klaauw (2009) report a reduction in the probability of first birth by 13%. They also report a reduction of the probability of second and subsequent birth (2%). On the other hand, Table 1.3 suggests a reduction of the probability of first birth by 20% and an increase of the probabillity of second birth by 50%.

For both analysis on the single women and the women in couples, all the estimates discussed above are insignificant. However, although the direction of the impacts are similar, the reported impacts from this chapter are somewhat larger.

A potential source of disparity in the size of the impact is the choice of identification strategy. Francesconi and Van der Klaauw (2007) and Francesconi, Rainer, and Van Der Klaauw (2009) use a difference estimator whilst this chapter employs a difference-in-differences estimator. On the one hand, the estimates from the present paper are prone to bias caused by the use of an invalid control group, but such a problem is absent when a difference estimator is used. On the other hand, a difference estimator may also be problematic if there are any macro trends other than the welfare reforms that affected the timing of birth. They address the macro effect by including a linear and a quadratic trend terms, though a difference-in-differences estimator would take account of this trend more flexibly when a correct control group is used. The latter point may be relevant in explaining the observed differences in the size of the reported impacts as the coefficients on the $After_j$ dummy variable reported in this chapter generally suggest a positive macro trend at the national level.

Most of the estimates reported in Brewer, Ratcliffe, and Smith (2010) are not comparable to those in this chapter, since they estimate the unconditional probabilities of birth rather than the conditional probabilities. However, they also report the impact on the age at first birth, and show that the affected women in couples insignificantly quickened their timing of first birth by approximately 8.4 months. In this chapter, the results from column (3) of Table 1.4 report an insignificant delay of approximately one year.

The differential sampling scheme may explain the observed differences in the reported results. This chapter uses a sample of women who formed a partnership in the first period, but these women could flexibly change their marital status in the subsequent periods. However, they include women who are found to be in partnerships in each year. If fertility is positively related to the partnership formation, then their sample would include a subgroup of women who formed stable relationships due to births. Inclusion of this additional effect is likely to positively influence their estimates compared to the ones in this chapter, which indeed is what we observe here.

1.10 Conclusions

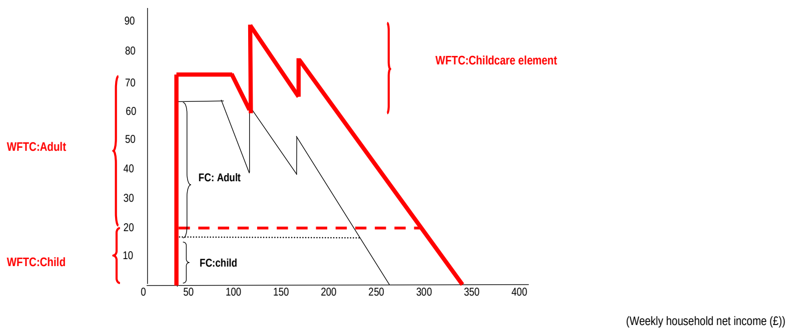
This chapter investigated the impact of the 1999 WFTC introduction on the fertility behaviour of the UK women. It provided complementary evidence to the previous fertility studies of WFTC by presenting the findings for the policy impact on the timing of birth.

In general, the results in this chapter showed that the overall impact of the UK policies only affected the timing of first birth significantly. Moreover, further analyses also revealed the delaying effect on first birth regardless of marital status, albeit insignificantly. However, the results do conform to the theoretical predictions and the delaying effect was particularly strong for the single women compared to those with partners. In addition, the insignificant shortening effect of intervals to second and third births were observed among the women in couples.

The effectiveness of WFTC to shorten the birth intervals may have been limited for the first birth compared to subsequent births if the marginal cost of entering parenthood is higher. This is likely to be the case, since compared to higher order births, childbearing for the first time is more time intensive due to the need to acquire the relevant skills to take care of a child. The results also suggest that the positive fertility incentives may have had a stronger impact among women who were eligible for WFTC, namely those women with partners who had at least one child before the policy introduction. In fact, those without children were likely to have been affected more strongly by other policies introduced during this period. In particular, the further analysis in this chapter revealed that those without children increased labour supply significantly. The strong delaying effect of first birth, therefore, can be explained, at least partially, as the result of the increased labour market opportunities provided to UK women during the period of analysis.

1.11 Chapter One: Figures and Tables

Figure 1.1: Structures of FC and WFTC



Note: Amount of FC and WFTC are calculated for a family with one child under 11 years old. Parents are assumed to be working at hourly wage of 3.6 pounds. Childcare cost is assumed to be 45 pounds per week. Both parents are assumed to be working 16 hours or more when the net household income reaches £115.2 per week and hence are eligible to receive childcare element of FC and WFTC. One parent is assumed to be working 30 hours or more when the net weekly household income is over £158.94. Housing benefit and Council tax benefit are ignored for simplicity.

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Figure 1.2: Description of the data structure and notations

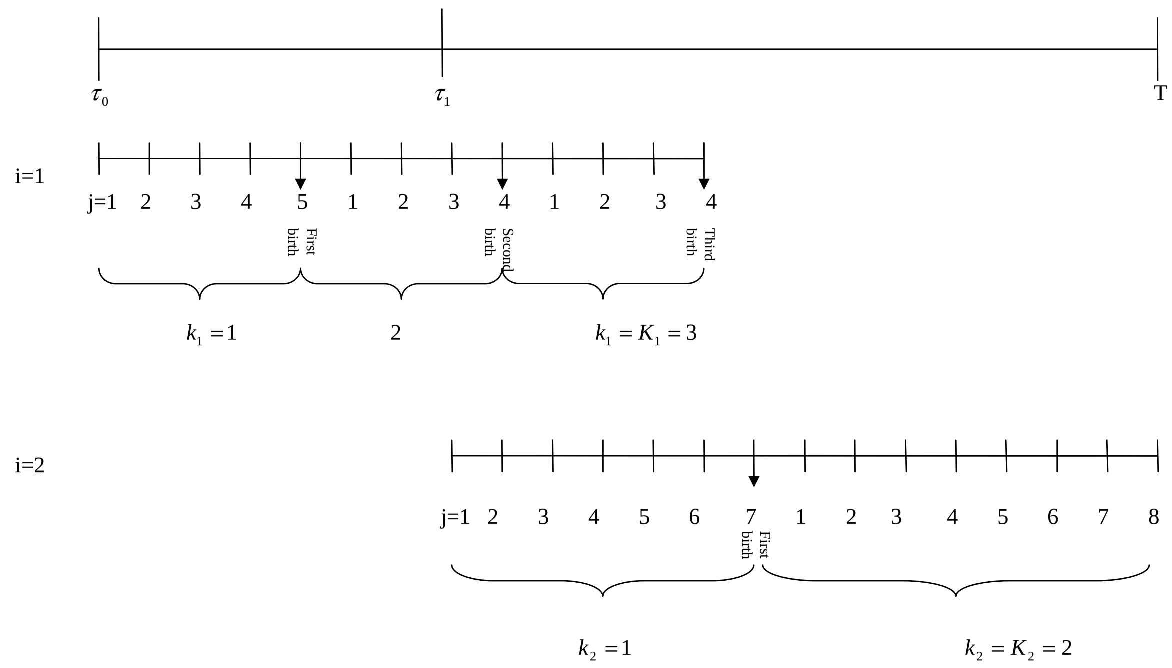
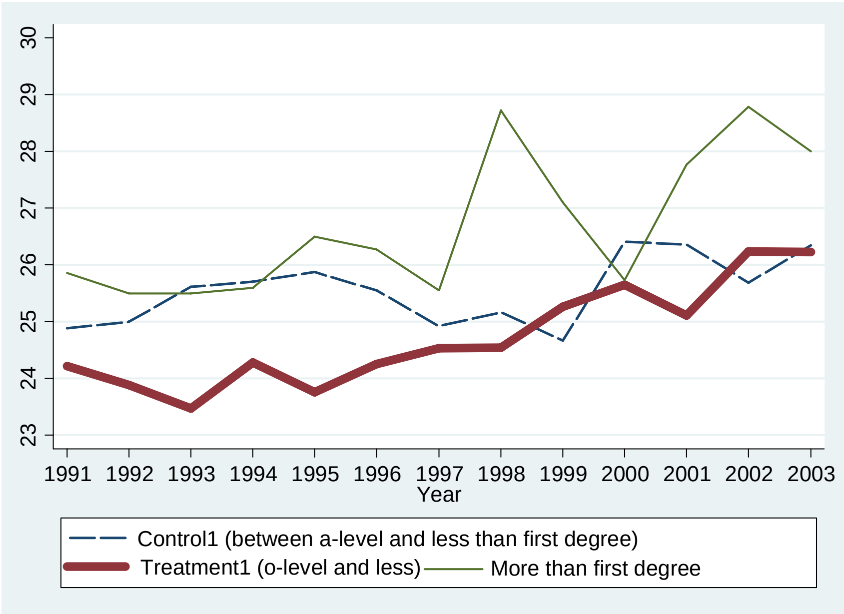


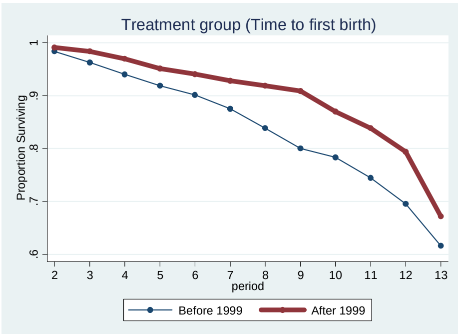
Figure 1.3: Trends of mean age at first birth by women's educational attainment



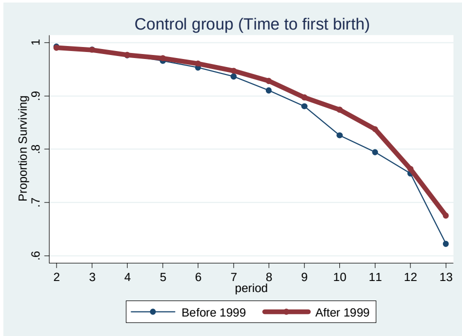
Note: This figure shows the UK trends of age at fist birth by educational attainment between 1991 and 2003. The statistics are calculated using the 1991-2003 BHPS. The thick line shows the trends for women in the treatment group (those with or less than o-level education). The dotted line presents the trend for women in the control group (those with a-level and above but below first degree attainment), and the thin line indicates the trends for women with more than first degree.

Figure 1.4: Life table survival functions

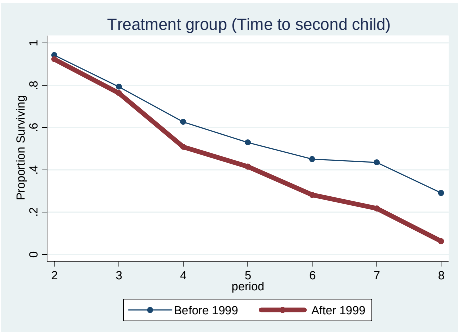
(a)



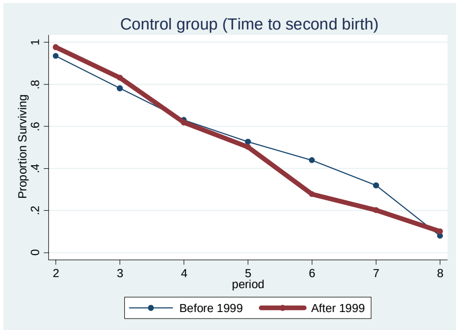
(b)



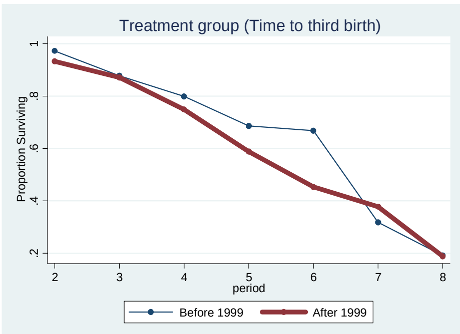
(c)



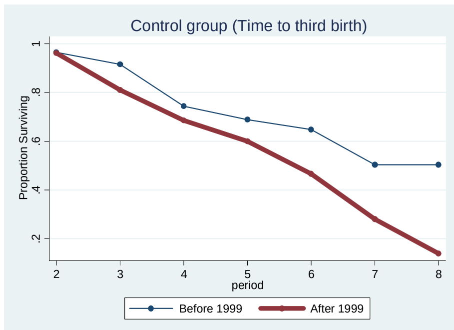
(d)



(e)



(f)



Note: The lifetable estimates of the survival functions are plotted. In all these figures, these lines show the proportion of women who remained childless until each period. The survival functions are plotted for before and after 1999 by treatment status and birth parity. The left and right figures each shows the survival functions for treatment and control groups respectively. Data employed is the 1991-2003 BHPS. Sample of childless women who were 20 in the initial period are included regardless of the marital status.

Table 1.1: Structure comparisons of FC and WFTC

Family Credit (1999 rate)	**Working Families Tax Credit (1999 rate)**	**Eligibility condition**
Adult credit £48.80	**Basic tax credit** £52.30	At least one earner working more than 16 hours and has a child
Child credits £15.15(Under 11) £20.90(11-16) £25.95(16-18)	**Child tax credit** £19.85(Under 11) £20.90(11-16) £25.95(16-18)	At least one earner working more than 16 hours and has a child
30 hour tax credit £11.05.	**30 hour tax credit** £11.05	At least one earner working more than 30 hours
Childcare disregard Childcare costs of up to £60 for one child and £100 for two or more children were disregarded from the family's income when the calculation was made.	**Childcare cost subsidy** 70% of eligible childcare cost (£100 for one child and £150 for two or more children) were subsidized.	Both adults working more than 16 hours and has a child (eligibility condition for Childcare tax credit)
Income threshold £79	**Income threshold** £90	N/A
Deduction rate 70%	**Deduction rate** 55%	N/A

Note: This table compares the structure of FC and WFTC. The first and second columns each summarises the structure of FC and WFTC respectively. The third column presents the eligibility conditions for each elements of FC/WFTC.

Table 1.2: Summary statistics

	All women	
Variables	Control 1	Treatment1
	Mean	Mean
1 if first birth observed	0.07	0.09
1 if second birth observed	0.18	0.17
1 if third birth observed	0.06	0.07
Age	25.75	25.87
	(4.55)	(4.74)
Household income	28780.19	23661.51
	(17937.62)	(16765.85)
Race		
White	0.93	0.89
Black	0.003	0.01
Other race	0.03	0.02
Housing tenure		
Owned	0.08	0.06
Rented	0.65	0.51
Mortgages	0.18	0.27
Region of residence		
North East	0.04	0.06
North West	0.09	0.12
Yorkshire	0.07	0.09
East Midlands	0.07	0.07
West Midlands	0.09	0.07
East of England	0.08	0.07
London	0.08	0.08
South East	0.12	0.12
South West	0.06	0.08
Wales	0.10	0.10
Scotland	0.19	0.13
Starting year dummy		
1 if observed from 1985	0.08	0.10
1 if observed from 1986	0.08	0.08
1 if observed from 1987	0.09	0.07
1 if observed from 1988	0.07	0.05
1 if observed from 1989	0.06	0.05
1 if observed from 1990	0.07	0.06
1 if observed from 1991	0.05	0.07
1 if observed from 1992	0.05	0.06
1 if observed from 1993	0.05	0.06
1 if observed from 1994	0.05	0.05
1 if observed from 1995	0.06	0.04
1 if observed from 1996	0.05	0.03
1 if observed from 1997	0.05	0.04
1 if observed from 1998	0.04	0.04
1 if observed from 1999	0.03	0.03
1 if observed from 2000	0.02	0.02
1 if observed from 2001	0.02	0.02
1 if observed from 2002	0.01	0.01
1 if observed from 2003	0.01	0.01
N	6241	7525
N(Individuals)	667	819

Table 1.3: The estimated policy impact

VARIABLES	(1)
	All women
Treatment×After×Spell1 (Impact on the timing of first birth)	-0.52***
	(0.187)
Treatment×After×Spell2 (Impact on the timing of second birth)	0.38
	(0.281)
Treatment×After×Spell3 (Impact on the timing of third birth)	-0.35
	(0.490)
After×Spell1	0.63***
	(0.162)
After×Spell2	0.90***
	(0.219)
After×Spell3	1.45***
	(0.390)
Treatment×Spell1	0.55***
	(0.136)
Treatment×Spell2	-0.00
	(0.201)
Treatment×Spell3	0.29
	(0.340)
LR test of unobserved heterogeneity	13,766
Observations	1,486
Number of women	172.3***

Note: This table presents the main estimates on the timing of each birth parity from the multiple-spell discrete-time proportional model with normal frailty.
Column (1) includes results estimated using a sample of all women regardless of their marital status in the initial period.
Data employed is the 1991-2003 British Household Panel Survey (BHPS). The dependent variable is a dummy that equals 1 if a woman gave birth and 0 otherwise.
Covariates included are: housing tenure, race, region of residence, initial age and its squared term, and cohort effect. Estimates of other covariates are included in the appendix.
Standard errors are given in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: The estimated treatment effect by marital status

VARIABLES	(1)	(2)	(3)
	Single women's entry to lone motherhood	Lone mothers	Women in couples
Treatment×After×Spell1 (Impact on the timing of first birth)	-0.30		-0.25
	(0.699)		(0.222)
Treatment×After×Spell2 (Impact on the timing of second birth)		-0.41	0.46
		(0.299)	(0.319)
Treatment×After×Spell3 (Impact on the timing of third birth)		0.29	0.73
		(0.618)	(0.969)
After×Spell1	0.37		0.41**
	(0.663)		(0.188)
After×Spell2		0.11	-0.02
		(0.337)	(0.243)
After×Spell3		-0.70	-1.72**
		(0.520)	(0.764)
Treatment×Spell1	-0.06		-5.46**
	(0.557)		(2.488)
Treatment×Spell2		5.16***	-6.05**
		(0.998)	(2.480)
Treatment×Spell3		4.41***	-6.39**
		(1.017)	(2.503)
LR test of unobserved heterogeneity	13.78***	0.0001	16.48***
Observations	2,064	2,886	6,616
Number of women	433	356	831

Note: This table presents estimates on the timing of birth by women's marital status change. proportional model with normal frailty.

Results are estimated using the multiple-spell discrete-time .

Column (1) includes the estimated policy impact on single women's entry to lone motherhood.

Column (2) includes the estimated policy impact on lone mothers' timing of subsequent births.

Column (3) includes the estimated policy impact on the timing of birth among women in couples.

Data employed is the 1991-2003 British Household Panel Survey (BHPS). The dependent variable is a dummy that equals 1 if a woman gave birth and 0 otherwise.

Table 1.5: Robustness analysis

VARIABLES	(1)	(2)	(3)
	Pre-reform period 1985-1998	After=1998	After=2000
	All women	All women	All women
Treatment×After×Spell1 (Impact on the timing of first birth)	-0.11	-0.46**	-0.65***
	(0.244)	(0.184)	(0.194)
Treatment×After×Spell2 (Impact on the timing of second birth)	-0.02	0.27	0.04
	(0.346)	(0.280)	(0.289)
Treatment×After×Spell3 (Impact on the timing of third birth)	0.24	-0.05	-0.09
	(0.672)	(0.495)	(0.510)
After×Spell1	0.38*	0.64***	0.58***
	(0.228)	(0.163)	(0.163)
After×Spell2	0.18	0.96***	1.07***
	(0.294)	(0.220)	(0.221)
After×Spell3	0.88	1.66***	1.08***
	(0.575)	(0.397)	(0.417)
Treatment×Spell1	0.76***	0.57***	0.54***
	(0.190)	(0.143)	(0.128)
Treatment×Spell2	-0.03	0.02	0.13
	(0.295)	(0.215)	(0.187)
Treatment×Spell3	0.33	0.34	0.14
	(0.619)	(0.382)	(0.310)
LR test of unobserved heterogeneity	36.45***	181.6***	163.9***
Observations	7,020	13,766	13,766
Number of women	1,043	1,486	1,486

Note: This table replicates the analysis in Table 3 using BHPS data from pre-reform period (1985-1998).
Reported estimates are from the multiple-spell discrete-time proportional model with normal frailty.
Column (1) shows results of the placebo test, which is conducted using a pre-policy period data (1985-1998).
Column (2) shows results from a regression, which uses the year 1998 to divide the before and after periods .
Column (3) shows results from a regression, which uses the year 2000 to divide the before and after periods .
Information on variables used can be found in Table 3. Standard errors are given in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Estimated marginal effects of the welfare reform impacts on women's employment status)

VARIABLES	(1)	(2)	(3)
	All childless women	Women with one child	Women with two children
Treatment×After	0.04**	-0.00	0.03
	(0.020)	(0.024)	(0.021)
After	-0.03***	-0.03***	-0.03***
	(0.004)	(0.008)	(0.007)
Treatment	-0.17***	-0.06***	-0.07***
	(0.017)	(0.020)	(0.018)
Observations	37,086	20,398	25,879

Note: This table presents the policy impact on employment by the number of children in the household. Data employed is the 1993-2003 Family Resources Survey . The dependent variable is a dummy that equals 1 if a woman is in employment.
Covariates included are: age and its squared and cubed terms, housing tenure, race, region of residence. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

2 Chapter Two: Did the US infertility health insurance mandates affect the timing of first birth?

2.1 Introduction

Currently 6 million women in the United States experience difficulties in conceiving a child even after a year of unprotected intercourse. The figures for the proportion of women who faced impaired fecundity increased from 8% in 1982 to 10% in 1995 (Chandra and Stephen, 1998). Whilst various infertility treatment options are available to assist these couples, they are often extremely costly. Some countries provide financial help, but in the U.S. patients often faced the full financial burden of the treatment.[10] Between 1977 and 2001, state-level legislation was introduced in 15 states which mandated health insurance providers to offer coverage for the fertility treatment cost.

Mosher and Bachrach (1996) suggest that the observed increase in the number of US women suffering from infertility problems is not caused by an increase in the rate of infertility, but rather due to more women postponing their fertility activities. This delay of motherhood is prominent among women with higher educational attainment as well as stronger labour market attachment (Rindfuss, Morgan, and Offutt, 1996). One possible reason for observing the delay particularly among highly educated women is the difficulties they face in balancing work and life. Phipps, Burton, and Lethbridge (2001) document that, on average, women have more job interruptions than men and 80% of these interruptions are related to motherhood. Since there is a substantial amount of evidence pointing out possible detrimental impacts of career interruptions on women's future wages (for example, see Eckstein and Wolpin, 1989; Altuğ and Miller, 1998; Korenman and Neumark, 1992), women with high career

[10]For example, the public system in Denmark offers up to three cycles of In Vitro Fertilization treatment for free.

ambitions may be postponing births to advance in their workplaces.

If this is the case, the introductions of state-level infertility insurance mandates may have induced women to further delay giving birth, since the knowledge of accessible and affordable infertility treatment may have led women to focus on their careers for a longer period of their lives and postpone giving birth. Given that the majority of women who obtain treatment are older and highly educated white women who have stronger attachment than average to the labour market, such an argument is rather plausible (Bitler and Schmidt, 2007).

Previous literature on the US infertility insurance mandates has mainly highlighted the impact of mandates on the take up and the outcomes of treatment among an older group of women. This is a logical choice of the age group to study as women who need and thus access the treatment are often those who are above 35. In contrast, this chapter investigates a potentially unintentional impact among younger women. In particular, this chapter studies how the U.S. infertility insurance mandates affected the timing of first birth of young women by employing the framework of event history analysis. The impacts of mandates are identified using the difference-in-differences approach by comparing the timing of births for women residing in states with and without the mandates. The main data is taken from the 1980-1997 Panel Study of Income Dynamics (PSID) and the Current Population Survey (CPS).

The focus on the timing of first birth among younger women is appealing, since understanding how younger women's decisions to have a child alter in response to such finanicial incentives is an interesting question on its own. Moreover, although these women having the wider choice of birth timing is welfare improving, the increase in the age at first birth is likely to cause various negative health outcomes. For example, Menken, Trussell, and Larsen (1986) report that delay of birth from the age group 25-29 to 30-34 increases the proportion of infertile women from 9 to 15%. First births at older ages are also associated with health risks for both the mothers and the new born children (the American Society for Reproductive Medicine (ASRM), 2003) . The probability of down syndrome increases for birth after 30 and the probabilities of miscarriages and pregnancy complications also increase for older mothers. The delay would, therefore, be likely to increase the heath care costs due not only to the higher demand for infertility treatment but also to the more intensive pre and post-natal care required. Lastly, several of the previous works on this topic use young women as a control group in order to investigate the impact of mandates on older women. These papers typically assume that younger women are unaffected by the introduction of the infertility health insurance mandates. If, however, younger women are indeed delaying birth, the policy impacts would be overestimated.

The remainder of this chapter is organized as follows. The next section looks at background information regarding the U.S. infertility treatment and the health insurance system as well as the structure of the state mandates. Section 2.3 describes the theoretical framework and 2.4 looks at the past literature. Section 2.5 and 2.6 each describes the identification strategy and the empirical specification. Section 2.7 presents the data used for the analysis. Section 2.8 and 2.9 describe the results and robustness analysis respectively. Section 2.10 concludes.

2.2 Background

2.2.1 Infertility treatment

The initial step taken by couples seeking treatment is the examination of both partners' reproductive organs. As a next step, the majority undergo several less invasive methods such as the use of fertility drugs which induce women to produce multiple eggs per ovulation. If the cause of infertility is clear, women proceed straight to surgery in order, for example, to unblock their fallopian tubes. Whilst most women successfully conceive a child without using more invasive methods, a small proportion of women proceed to receive treatment via the Assisted Reproductive

Technologies (ART), which are any treatments that handle either sperms and eggs or both. Details of these treatments are summarized in Table 2.1.

Although two thirds of couples who seek treatment in the U.S. successfully conceive children, the success rates vary with the age of women. For example, the pregnancy rates of ART for women aged 29 is 44.9% but this figure drops to 37.6% for women aged 35 (the Centers for Disease Control and Prevention, 2005).

Infertility treatments are often very costly. Hormone therapies which are used to induce the releasing of an egg could cost between $50 and $5,000 per cycle whilst tubal surgery would cost between $3,000 and $10,000 (see Table 2.1). One of the most expensive treatments, In Vitro Fertilization (IVF) on average costs $12,400 per cycle, although this treatment only accounts for approximately 5% of the U.S. infertility treatment (ASRM, 2009) . When these treatments are combined or used for repeated cycles, the financial burden for patients quickly becomes too heavy for them to continue the infertility treatment.

2.2.2 Structure of health insurance in the United States

High medical cost in the US is covered by various forms of health care insurance. The US health care insurance can be divided into public and private insurance. Public or federally funded programs are under the control of federal laws and currently cover approximately 27% or 85 million individuals that often face difficulties obtaining private insurance policies (DeNavas-Walt, Proctor, Smith, and the Bureau of the Census, 2008). Private insurance, however, is the relevant insurance for the purpose of this chapter as the state-level mandates only affect those that are insured privately.

Private insurance policies are either purchased individually or through employers under group purchasing agreements. Whilst only 12% of individuals purchase their own insurance, the majority obtain their coverage through their employers (DeNavas-Walt et al., 2008). The importance of employer sponsored insurance in the U.S. is evident from the sheer number of individuals that are covered by their employers. However, the increasing cost of medical care in the U.S. has also posed significant financial difficulties for the employers. As a result, individuals with employer-sponsored health insurance are likely to be working as full-time employees in large firms (Sullivan, Miller, Feldman, and Dowd, 1992).

One type of organization which became increasingly important as a cost cutting measure during the period of interest is the Health Maintenance Organizations (HMOs). It is a type of managed care organization (MCO), which provides health care coverage. In this system, the care providers charge a low fee for a health care service and in return, employers who contract with HMOs ensure a steady inflow of patients. The introduction of the Health Maintenance Organization Act of 1973 forced employers with more than 25 employees to offer federally certified HMO options as well as traditional indemnity insurance plans when requested. Although HMOs had only 16% of the market in 1988, this figure increased to 31% by 1996 (Claxton, Gabel, Gil, Pickreign, Whitmore, Finder, DiJulio, and Hawkins, 2006). A large proportion of US women have attained their health insurance coverage through either their or their dependents' employers who in turn have their insurance packages to be administered by HMOs.

2.2.3 Infertility insurance mandates

As a way to provide coverage for the cost of infertility treatment, states individually implemented insurance mandates between 1977 and 2001 (see Table 2.2 in the appendix).

The extent of coverage varies across the states and these differences in the generosity of coverage stem from three main components. Firstly, there are mainly two types of mandates implemented. "Mandate to cover" regulates insurance companies to cover the infertility treatment cost regardless of the policy purchased. This is a stronger form of legislation compared to "Mandate to offer" which requires insurance companies to offer the option for consumers to purchase the coverage. Secondly, some states cover the cost of IVF while the others do not. Although IVF is not one of the most commonly used treatments, it is the most costly option (see Table 2.1). As a result, the differential degree of coverage for IVF by each state creates variations in the generosity of financial support given to couples. Lastly, some states implemented the mandates for HMOs whilst others excluded them from the need to cover the treatment costs. As mentioned in the previous section, HMOs play an increasingly important role in the US health insurance system. States with mandates which include HMOs, therefore, would be more likely to have a larger impact on the timing of birth than those without.

One thing to note is the lack of age limits in most states. In fact until after 2000, no states had any imposition of age restrictions. This is slightly surprising as the treatment success rate is heavily dependent on the age of the woman. Such lack of restriction may have acted as another factor to encourage women to delay giving birth.

2.3 Theoretical framework

Heckman and Willis (1976), illustrate a delay of birth when a couple experience a steeply rising income profile. They use a dynamic fertility model where individuals maximize discounted value of utility over her lifetime given a flow budget constraint. They assume that couples can choose, in any months of the fertile period, the level of contraception and hence the probability of conception. Under this theoretical framework, they find that couples sustain a high level of contraception until their flow of income is sufficiently high to conceive a child. Given this theory, women have incentives to delay birth in order to minimize the loss of wages, and seeing that their income profiles would improve due to hard work during their earlier years provide additional incentive for women to further delay giving birth.

When women determine when to have a child, however, they have another factor to consider, namely the biological constraint. Women could postpone giving birth if they consistently stay fertile. Women's fecundity, however, declines with age. The introduction of infertility insurance mandates reduced the price of treatment and made it possible for women to have a child for a longer period of their lives. As a result, the policy introductions effectively reduced the opportunity cost of having a child in the future. Although it is likely that not many women possess knowledge of the procedures and the costs of various infertility treatments in detail before they try to conceive through natural method, a study by Hewlett (2004) suggest that women are aware of the fact that the treatment relaxes their fertility constraint to some extent. In fact, she suggests that they may be over-estimating the effectiveness of infertility treatments as approximately 89% of young career driven women believe that infertility treatment enables them to have a child well into their 40s when in fact the treatment success rate drops sharply after the age of 35 (the American Society for Reproductive Medicine, 2003) . Knowing that infertility treatment could bring them a child together with the knowledge that their health insurance covers the cost of the treatment in the future, they are presented with an incentive to delay giving birth.

Although the introduction of mandates reduced the cost of future treatment, the financial burden faced by the health insurance providers was likely to have been passed on to the consumers in the form of either reduced wages if individuals obtained their insurance through their employers or higher premiums or both. There are no studies that directly investigated this effect of the infertility insurance mandates on the insurance premiums. However, several studies used other health insurance mandates to understand the impact on wages and insurance premiums. Using 1989 cross sectional data, Acs, Winterbottom, and Zedlewski (1992) note that the health insurance mandates increased premiums by 4 to 13%. Gruber (1994), on the other hand, studies how the state maternity mandates introduced in three states affected the wages, and concludes that the full cost of mandates was paid by women aged between 20 and 40.

Such an increase in the premiums reduces the demand for health insurance and thus the number of individuals affected by the policies are likely to have declined. A change in wages, on the other hand, generates both income and substitution effects. The reduction of wages leads women to delay birth due to an income effect. The substitution effect, however, predicts shortening of birth intervals if childbearing is complementary to leisure.

In summary, affected women are likely to face opposing incentives when determining their timing of birth and the evaluation of the policy impact requires an empirical investigation.

2.4 Literature

The majority of the previous literature has highlighted the impact of the mandates on older women. This is a natural choice of group as these are the women who are more likely to seek treatment. In contrast, this chapter sheds light on how the mandates changed the fertility timing preferences of younger women when they take account of the availability of cheaper infertility treatments.

The introductions of mandates are thought to have increased the use of various infertility treatments and thus are likely to have affected the birth rate. Using 1985-1999 Vital Statistics Detail Natality Data and the Census Bureau, Schmidt (2005; 2007) looks at the policy impacts on the rate of first birth, which is defined as the proportion of women with particular demographic characteristics giving first birth. Estimates from a difference-in-difference-in-differences estimator show that while the mandates did not significantly affect all US women, white women who were older than 35 experienced a significant increase in the rate of first birth (approximately 32%).

Bundorf, Henne and Baker(2007) study how the mandates affected the access to and the aggressiveness of ART. Due to the high cost of these treatments, women may implant multiple embryos per cycle in the hope to increase the success rate and reduce the number of cycles they need to undergo. However, such action would increase the rate of multiple births which is taxing both for maternal and fetal health. The reduction in the cost of treatment may have reduced the level of aggressiveness and multiple birth rates. Using the 1981-1999 Vital Statistics Natality Birth Data and the 1989-2000 registry data from the Society for Assisted Reproductive Technologies, they estimate the policy impact using a difference-in-differences estimator and conclude that the mandates increased the utilization of ART, however the aggressiveness of the treatment did not change even after the introduction of the mandates.

Bitler (2008), on the other hand, studies whether the mandates changed the rate of multiple births and the child health outcomes. She employs the 1981-1999 Birth Certificate Data and the 2000 Decennial Public Use Microdata Sample and 2001-2002 American Community Service Data, and finds that the probability of a twin birth increased by 10 to 23%. She also studies how the mandates affected various health outcomes of the newly born children. In particular, she looks at the impacts on birth weight, gestation, and 5-minute Apgar score for samples of singleton and twin births. Although no effect of mandates on these birth outcomes are found for young women aged below 30, she finds some negative impacts on the birth outcomes of the twins and singletons among older women.

Whilst these past studies have focused on how the mandates affected an older group of women, it is also possible that these state mandates influenced women who were considering a potential use of treatment in the future. In other words, the introduction of mandates may have encouraged younger women to delay giving birth. If women in the mandated states were indeed delaying their timing of birth, findings from Schmidt (2005; 2007) and Bundorf et al. (2007) not only show increase in the number of first births due to more easily accessible treatment but also reflects more women at older ages giving birth because of their planned delay of birth. This interpretation also fits with the results presented by Bitler (2008). The negative birth outcomes found among older women may be due to more women giving birth at a later age. This in turn highlights the importance of studying the timing of birth effect.

Similarly to the present paper, Buckles (2007) investigates whether women delayed births in response to these policies. Using the 1982-1999 Current Population Survey, she first looks at how the first birth rates of older women aged between 35 and 44 changed before and after the introduction of mandates using a difference-in-differences estimator and finds that women residing in mandated states increased the first birth rate by approximately 40% after five years of coverage. She, however, argues that estimates may simply be picking up the ability of older women to give birth due to the increasing availability of infertility treatment over time. In order to identify the cause of the delay, she then looks at how the birth rates of younger women were affected. The estimates suggest that women aged between 22 and 25 as well as 26 and 30 both decreased the birth rates by approximately 26% after five years of coverage. Bundolf et al. (2007) also devotes a small section to this issue and presents similar difference-in-differences estimates which indicate that the birth rate of women aged 25-29 decreased while it increased for women aged 35-39.[11]

Although the evidence presented by Buckles (2007) and Bundolf et al. (2007) indicates a potential delaying effect of the mandates, they both assume that the behavior of the older cohort of women proxies for that of the younger cohort in 10 or 20 years time. Given that the lifestyles of women changed drastically over the period of observation, this may be a rather strong assumption. Moreover, the repeated cross sectional data only allows one to observe the fertility activity until the interview date and thus makes it difficult to investigate whether these young individuals are delaying births or simply not having any children at all. As a result,

[11]There is another work-in-progress research on this topic by Machado and Sanz-de-Galdeano, which was presented in the 2010 American Society for Health Economists in Cornell University. They also investigate the impact of the US infertility health insurance mandates on the timing of first birth using repeated cross sectional data.

one may obtain an even stronger understanding of the policy impact by following how the same women responded to the mandates at different points of their lives. This chapter, hence, proposes to use the framework of duration analysis using longitudinal data in order to investigate how the mandates affected the timing of birth.

2.5 Identification strategies

Special care is needed when studying the timing of birth using longitudinal data. Firstly, unlike other subjects of economic studies such as unemployment, a woman gives birth to the first child only once in her life time. As a result, we fail to observe the timing of first birth of the same individual with and without the policy even with the availability of longitudinal data. Secondly, women's lifestyle and fertility behavior changed drastically over the sample years.

Taking account of the first point, it is necessary to compare the influence of the mandates on the timing of birth across individuals over time. However, the second point raises a concern that one cohort of women observed in later years are not comparable to those from earlier years. Instead, I attempt to identify the policy impact by defining a comparison group which includes women are residing in the non-mandated states. These women have similar demographic characteristics to those women who were living in the mandated states but were unaffected by the policies. By comparing the timing of birth of women in mandated and non-mandated states, the policy impact is uncovered by evaluating the change in differences before and after the policy introduction dates. I, therefore, employ a difference-in-differences estimator exploiting the variation in exposure to cheaper infertility treatment across both states and time.

2.6 Empirical specifications

This chapter carries out the analysis using the 1980-1979 Panel Study of Income D. The Panel Study of Income Dynamics (PSID) records birth month and year of children born to women in the core sample. Due to the grouped nature of the data and the flexibility to incorporate a nonparametric baseline hazard, this chapter employs a discrete-time proportional hazard model. This section follows materials from Jenkins (2005).

The underlying continuous-time hazard, which is the conditional hazard rate of having a first child, for the ith individual at time j is given by

$$\theta_i(j|x,\beta) = \lambda(j)exp[x_i(j)'\beta] \tag{2.1}$$

$\lambda(j)$ is the baseline hazard and $x_i(j)$ denotes covariates to control for the ith individual's characteristics. In this chapter, j is measured in age years, and the discrete nature of the PSID data implies that a birth is recorded to have been given in the jth age if she gave birth on the continuous time scale of between $(j-1)$ and j. The discrete hazard function, thus, characterizes the probability of first birth by the jth age provided that she has not yet given birth by the $j-1th$ age.

$$\begin{aligned} h_i(j|x_i(j)) &= P[T=j|T>j-1, x_i(j)] \\ &= 1-exp[-\int_{j-1}^{j}\theta_i(s)ds] \\ &= 1-exp[-exp(x_i(j)'\beta + Mandate_i(j-2)'\delta + \gamma(j))] \end{aligned} \tag{2.2}$$

where

$$\gamma(j) = ln[\int_{j-1}^{j}\lambda(s)ds] \tag{2.3}$$

.

In order to allow the baseline hazard to be flexible, a piece-wise constant specification is chosen. By fitting a period specific indicator variable, the baseline hazard is allowed to vary across periods. As discussed in Chapter 1, the baseline hazard captures the differential hazard rates that are caused by the lengths of childless periods prior to the observed birth. The vector of parameters $\gamma(j)$ captures the baseline hazard. The covariate vector $x_i(j)$ is assumed to be time-invariant within an interval but changes across intervals. $x_i(j)$ contains individual and state-level characteristics, a time-invariant *Policy* dummy that picks up states which introduced the infertility insurance mandates. The most important variable for our analysis is $Mandate_i(j)$. It is an indicator variable that equals one if the individual was residing in a state where the mandate had been introduced for at least two years. This definition allows individuals two years to respond to the introduction of mandates.[12] To illustrate how this variable is defined, consider a woman who is living in Connecticut and is included in the sample from year 1985. Since Connecticut introduced its mandate in 1989, $Mandate_i(j)$ would be 0 for this individual for the first seven years of observation and equals 1 from 1991 onwards. The dummy equals to 1 only from 1991 in order to allow women additional two years to respond to the introduction of the mandates. If another woman in the same state is included in the sample from 1989, her $Mandate_i(j)$ would equal to 0 for the first two periods and 1 in the subsequent periods . In a usual framework of difference-in-differences, this dummy variable is the interaction between the *Policy* dummy and another dummy that indicates years after policies are introduced. The coefficient δ captures the policy impact which is identified by taking the differences in $ln[-ln(1 - h_i(j|x_i(j)))]$ between the two groups of states and evaluating the change in these differences before and after the introduction of mandates.

The discrete hazard function specified in Eq. (2.2) allows the set of covariates, and importantly the $Mandate$ dummy, to proportionally affect the baseline hazard function. The shape of the baseline hazard, however, is common between the two groups. Since the focus of this chapter is to identify how long women delayed their first birth, an alternative hazard specification is given by

[12] Additional analysis allowing for three years of exposure did not change the findings discussed here.

$$
\begin{aligned}
h_i(j|x_i(j)) &= P[T=j|T>j-1, x_i(j)] \\
&= 1 - exp[-\int_{j-1}^{j} \theta_i(s)ds] \\
&= 1 - exp[-exp(x_i(j)'\beta + \gamma(j) + Mandate_i(j) \times \alpha(j))].
\end{aligned} \tag{2.4}
$$

Eq. (2.4) specifies flexible duration dependence by treatment status. Given the above specification, the discrete survival function is

$$
S_i(j|x_i(j)) = \prod_{k=1}^{j}(1 - h_i(k)) \tag{2.5}
$$

Let c_i be an indicator variable that equals one if the spell is censored (i.e. the individual reaches the end of observation period without having any children). The contribution of the ith individual to the likelihood function is

$$
\begin{aligned}
L_i &= [P(T_i = j)]^{1-c_i}[P(T_i > j)]^{c_i} \\
&= [h_i(j)S_i(j-1)]^{1-c_i}[\prod_{k=1}^{j}(1-h_i(k))]^{c_i} \\
&= [\frac{h_i(j)}{1-h_i(j)}\prod_{k=1}^{j}(1-h_i(k))]^{1-c_i}[\prod_{k=1}^{j}(1-h_i(k))]^{c_i} \\
&= [(\frac{h_i(j)}{1-h_i(j)})^{1-c_i}\prod_{k=1}^{j}(1-h_i(k))]
\end{aligned} \tag{2.6}
$$

The above analysis does not account for unobserved heterogeneity. However, Lancaster(1980) and Van den Berg (2001) point out that uncontrolled unobserved heterogeneity would cause spurious negative duration dependence as those with higher hazards tend to exit first. By taking account of the unobserved heterogeneity, the discrete hazard functions with the unobserved heterogeneity are given by

$$
\begin{aligned}
h_i(j|x_i(j), \epsilon_i) &= P[T = j|T > j-1, x_i(j), \epsilon_i] \\
&= 1 - exp[-\int_{j-1}^{j} \theta_i(s)ds] \\
&= 1 - exp[-exp(x_i(j)'\beta + Mandate_i(j)'\delta + \gamma(j) + \epsilon_i)].
\end{aligned} \tag{2.7}
$$

and

$$
\begin{aligned}
h_i(j|x_i(j), \epsilon_i) &= P[T = j|T > j-1, x_i(j), \epsilon_i] \\
&= 1 - exp[-\int_{j-1}^{j} \theta_i(s)ds] \\
&= 1 - exp[-exp(x_i(j)'\beta + \gamma(j) + Mandate_i(j) \times \alpha(j) + \epsilon_i)].
\end{aligned} \tag{2.8}
$$

where ϵ_i is the unobserved heterogeneity.

Assuming that the density function, $f_\epsilon(\epsilon_i)$, follows a gamma distribution, the likelihood function is marginalised with respect to the unobservables. The choice of a gamma distribution as the unobserved heterogeneity distribution is rather convenient as all the relevant functions have closed form solutions. Moreover, Abbring and Van den Berg (2007) showed that when the unobserved heterogeneity is specified to proportionally affect the hazard, the unobserved heterogeneity distribution converges rapidly to a gamma distribution.[13] The likelihood contribution for a person with a spell length j, therefore, is

$$
L_i = \int [(\frac{h_i(j)}{1 - h_i(j)})^{1-c_i} \prod_{k=1}^{j} (1 - h_i(k))] f_\epsilon(\epsilon_i) d\epsilon_i \tag{2.9}
$$

[13] Another possible approach is to use the nonparametric maximum likelihood estimator which fits an arbitrary distribution of unobserved frailty approximated by a set of mass points and the probability of a person at each mass point ((Heckman and Singer, 1984)). However, Baker and Melino (2000), through a Monte Carlo experiment, showed that estimating both flexible duration dependence and unobserved heterogeneity leads to a significant bias in the parameters of these components. They identify the cause of this bias to be the nonparametric maximum likelihood estimator (NPMLE) to find too many spurious mass points.

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2.7 Data

The main data used for the analysis is the 1980-1997 Panel Study of Income Dynamics (PSID). The PSID is a nationally representative longitudinal dataset and the data is collected both at the individual and family level. The number of households surveyed initially in 1968 was 4,800 but the sample size increased to 7,000 households by 2000 as the children of initial core sample members left households to establish their own families. From 1973, interviews were conducted over the phone and computer assisted phone surveys were introduced from 1993.

While repeated cross-sectional data have attractive features such as the large number of births and sample size, panel data presents us with several advantages. One major merit is the ability of longitudinal data to follow the same individuals. This characteristic is a crucial feature for our analysis for three reasons. Firstly, the main focus of this chapter is to see how women changed their fertility behavior over time. Secondly, identifying the policy impacts requires one to know whether women stayed in a particular state long enough for the mandates to have had some impacts on their timing of birth. From the cross sectional data, it is not possible to unveil this information as they only reveal in which state the individual was residing in at the time of interview. Lastly, fertility histories of women can reasonably be recovered from earlier waves. This ensures a correct selection of women, namely those without any children, into the sample.

The main period of observation is chosen to be between 1980 and 1997. As shown in Table 2.2, most states introduced the mandates between mid 1980s and early 1990s. The period of observation, therefore, allows us to observe how the fertility behavior changed over years in response to the mandates. The year 1997 is chosen as the end of the observation period, since the frequency of PSID data collection changed from annually to every other year after 1997. The reduced frequency of data collection is problematic for the analysis, since information on the state of residence would be missing for the year that was not collected.[14]

Observations are organized in a person-year format. This implies that the same individual appears in the sample as many years until she either gives birth to her first child or she reaches the end of the observation period without giving birth. The width of the step, a year, is decided in order to impose less parametric structure on the baseline hazard whilst having enough birth observations. The focus of this chapter is to identify the policy impacts on women's timing of birth, and so a cohort of women who turned between 20 and 30 anytime during the observation period are included in the sample.

Some women in the PSID moved to different states during the observation period. These women have likely experienced limited influence from the mandates as their stay in a mandated state was short. It is, therefore, rather difficult to determine whether they should be included in the treatment or the control groups as such short stays may or may not be sufficient for individuals to be affected by the mandates. As a result, only women who did not move are included in the sample.[15] After selecting groups of individuals for the analysis, the number of individuals in the sample became 2685 contributing 10829 observations.

The dependent variable is a binary indicator that equals one if the individual gave first birth in a particular year. The demographic variables included are characteristics that are likely to affect fertility decisions such as women's educational level, ethnicity, age at the start of the observation period and its squared term. The

[14]Additional regression is estimated using data from 1980 to 2005 for robustness check. Individuals recorded to have resided in the same state before and after the missing years are assumed to have not moved across states. Although the conclusion remain the same, the magnitude of the estimates are smaller when additional years of observations are included. One possible explanation for this finding is that individuals moved across states during the years when interviews were not carried out. As a result, the mandated group may have included individuals who did not stay in the mandated state long enough to be affected, thus diluting the effect. The results are available upon request.

[15]Even when these movers are included, the general conclusion remains the same but the estimated policy effect is less significant. Such reduction in the significance level may be due to the inclusion of individuals who moved for reasons other than the mandates in the affected group diluting the effect. The results are available upon request.

regional and year dummies as well as state-level economic characteristics are also included in order to allow for differential characteristics across regions and years. These economic characteristics also control for the level of labour demand during the period of observation.[16] [17] During the 17 year period being considered in the analysis, female labour force participation increased and women's lifestyles and preferences drastically changed. Moreover, availability of infertility treatment increased over years. As a result, individuals from later periods are more likely to give birth at an older age. In order to control for this differential fertility timing over the years, cohort dummies that take account of in which year women entered the sample are included. Marital status is not controlled in these regressions, since the status is likely to be jointly determined with fertility.

Table 2.3 presents the summary statistics of the PSID sample used in this chapter. The first two columns show the average characteristics of women residing in mandated and non-mandated states separately prior to the introduction of the mandates (i.e. 1970-1980). The third and fourth columns also show the statistics of the two groups from 1980-1997.

Comparing the statistics, these two groups of women have similar averages in most variables. However, there are minor differences in their characteristics. For example, there seems to be a higher concentration of black women in the non-mandated states throughout the period of 1970-1997. Moreover, women in mandated states are more highly educated.

The identification strategy in this chapter requires the exogenous introduction of the mandates. The employed estimator would eliminate the differences in the fertility behavior between the two groups of women as long as they experience the same trend. If, however, states introduced their policies due to the increasing demand for

[16]The state-level economic indicators are calculated using the 1980-1997 March Current Population Survey (CPS). CPS is a repeated cross-sectional survey collecting information from over 50,000 households.

[17]The regressions shown in this chapter only include regional level dummies due to the limited number of observations in some states. However, the results remain unchanged even when additional regressions are estimated using state fixed effect.

treatment, the demographic characteristics, and more importantly, birth trends of the two groups of women would differ. The violation of this assumption would bias the reported estimates and they would instead reflect both the policy impact as well as differential trends in fertility behavior of women.

To investigate if the disparities in the observed average characteristics translates into differential birth trends, Figure 2.1 display the trends of age at first birth during the pre-policy period 1970 and 1985 by mandate status and race. These trends are calculated by using the 1970-1985 NCHS's Vital Statistics Natality Birth Data which collects birth information via US birth certificates. Although there seem to be constant differences between those residing in states with and without mandates for both races, this figure indicates no disparities in trends between the two groups of women regardless of race.

Additionally, Figure 2.2 displays various economic characteristics such as top 10% income, median income, unemployment rates and female labour force participation during the pre-policy period for the treatment and control groups separately. Since highly educated women are the primary users of the infertility treatment, differential trends in economic characteristics would likely to unveil potential difference in the demand for treatment. These trends of economic characteristics are estimated using the 1977-1985 Current Population Survey (CPS).[18] Again, all economic characteristics indicate common trends between the two groups for all statistics.

Although raw data suggests a common trend among women residing in mandated and non-mandated states, additional robustness checks are carried out in order to further ensure comparability of women in these two states in Section 2.9.

[18]The duration of the CPS statistics is shorter as the data only reports the break down of the region of residences from 1977 onwards.

2.8 Results

2.8.1 Graphical analysis using the life table survival functions

The life table survival rates are plotted in Figure 2.3 for women with and without the exposure to the mandates. Points on these lines Iindicate the proportion of women remaining childless until a particular age. The left hand side figure shows the estimates when the entire sample is used while the figure in the middle presents estimates of women with more than 13 years of education. Moreover, since white women have more access to health insurance and thus are more likely to be affected by these mandates (Bitler and Schmidt, 2007), the right hand side figure presents the estimates for highly educated white women. All of the three figures indicate that women affected by the mandates remain childless until later stages of their lives. The observed delay seems to be more pronounced for the group of highly educated white women above the age of 30.

2.8.2 Regression analysis

Turning to the regression estimates, the discrete-time proportional hazard model with gamma unobserved heterogeneity given in Eq. (2.2) is estimated and the results are presented in Table 2.4. This specification allows for a vertical shift of the baseline hazard function proportionally to the set of demographic characteristics, but the shape of the baseline hazard function is unaltered across groups of individuals. The estimates presented in Table 2.4, therefore, show the scaling factor of the baseline hazard function. The standard errors in the parenthesis are bootstrapped to take account of the state-level clustering.

The "Mandate" dummy selects a subgroup of women from the affected group. In particular, it picks out women from the group of affected women who were living in a state and had already been exposed to the mandates for at least two years. Its coefficient, therefore, measures the policy impact of the mandates. A negative coefficient implies a delay of birth as it indicates a smaller probability of first birth.

The first column in Table 2.4 shows the estimates when the treatment and control groups are defined as all women in mandated states and non-mandated states respectively.[19] The estimate of the policy impact (i.e. coefficient of the "mandate" dummy) from the first column is insignificant and positive and shows no impact of the mandates.

Considering that the state-level mandates affected women with private health insurance, highly educated white women are more likely to be exposed to the policies (Bitler and Schmidt, 2007). Moreover, this group of women may face higher needs to delay birth in order to balance work and life. Reflecting this point, columns (2) to (4) in Table 2.4 presents the policy impact separately by various levels of educational attainment. Column (2) shows the estimated impact of women with 10 to 12 years of education. Column (3), on the other hand, presents results of women who attained 13 years or more education. Due to the limitations of the sample size, 13 years of education, which implies first year of undergraduate degree, is used as an indicator for selecting highly educated women. Results suggest differential impacts of mandates by educational attainment. As expected, significant negative policy impact are observed only for the highly educated women. Women with 10-12 years of education shortened the time until first birth instead. To see how white women are affected, column (4) in Table 2.4 presents estimates for highly educated white women only. Since this is a demographic group that is most likely to purchase health insurance, we expect to observe stronger impact from these women. As expected, the estimate in the last column is larger when only highly educated white women are included.[20]

[19]The estimated coefficients of other covariates are given in Table 9.

[20]Due to the small sample size of black women, the results reported in columns (1) to (3) are estimated assuming that white and black women went through similar experiences and the that differences between the two groups stem from the constant racial factor which proportionally affects the baseline hazard. This may be a rather strong assumption, however, Figure 2.1 show very similar trends between the white and black women. Moreover, although not reported in this chapter, additional regressions were estimated by interacting the policy impact dummy, $Mandate_i(j-2)$, with racial characteristics. These results indicate that highly educated black women are affected

Estimates in Table 2.4 merely show how the baseline hazard function is shifted by a constant scale due to the introduction of the mandates. It is, however, very likely that the affected women exhibit a different baseline hazard over years in response to the mandates. As a result, Eq. (2.8) is estimated for highly educated women where the baseline hazard functions are separately estimated for those who were unaffected by the mandates and those who were exposed to the mandates for at least two years. Table 2.5 presents the estimated baseline hazard coefficients where the width of a period is a year. Since the baseline hazard can only be estimated when there are birth observations, some periods are combined assuming that the hazard rates are constant between the two periods. The first column shows $\gamma(t)$ for individuals who are unaffected by the mandates. The estimates of interests, however, are shown in the second column. These estimates present differences in the baseline hazard rates between the two groups. They suggest that when exposed to the mandates for at least two years, individuals exhibit lower probabilities of birth continuously until the 5th period.

To better illustrate how the conditional probability of giving birth to first child changed over years, Figure 2.4 plots the predicted hazard functions. These figures present white highly educated women's predicted conditional probability of having a first child. The left figure illustrates how affected women would exhibit differential trends if they were exposed to the mandates for two years by the time they turned 20 conditional on not having a first child until this age. Similarly, the middle and right hand side figures present the trends for women who were affected for two years by the age of 25 and 30 respectively. In all figures, the plotted predicted hazard functions clearly indicate initial lower conditional probabilities of first birth among women affected by the policies. The differences in the probabilities between the two groups are relatively constant until the fifth year.

The plotted predicted survival functions can be found in Figure 2.5. Each point

in a very similar manner to white women although the sizes of the impacts are smaller.

on these lines indicates the probability of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those women who were exposed to the policies for at least two years at a particular age. It now becomes clearer that women who were exposed to the mandates for two years by the age of 30 exhibit a greater delay of first birth compared to those who were 20 or 25. For example, when we look at the middle figure, 50% of the unaffected women were still childless at the age of 27 whilst 50% of those who were affected remain childless until the age of 28.5. On the other hand, we observe approximately a 2 year delay among women who were affected by the mandates at the age of 30 (from the age of 35 to 37). Since, less than 50% of women gave birth in the left figure, it is not possible to compare the years of delay at the median for women who were exposed to the mandates for two years by the age of 20. However, the gap is narrowing around the median, suggesting a smaller delay.

Next, Table 2.6 shows estimated policy impacts by the differential coverage of the mandates. As discussed in Section 2.2.1, each individual state adopted mandates of varying levels of generosity. If women were aware of the details of the mandates, we would expect to observe more delay among women residing in states with more generous cost coverage. The estimates presented here, however, are likely to lack precision due to the small sample size. There are only 1180 observations of highly educated women and the analysis using subsamples of theses women exacerbate the small sample size problem further. This is particularly problematic when estimating the policy impact of weak mandates as states that introduced weak mandates are typically only 3-5 out the 15 mandated states. Interpretation of these results, therefore, must be done with caution.

Looking at the results in column (1) of Table 2.6, highly educated women seem to respond to "Mandate to cover" more strongly by significantly delaying first birth. Since "Mandate to cover" is a more generous policy compared to "Mandate to offer", this result matches with the prediction. Column (2), on the other hand, provides results that do not conform to the theoretical prediction. When the mandates include IVF coverage, the estimated delay is significant. However, the size of the delay seems to be larger, albeit insignificant, when IVF is not covered. In Column(3) the impacts of mandates regulating all insurance firms are compared to those that exempts some firms. Here again, women exposed to weaker mandates are responding more strongly by delaying birth.

Although the problem of the limited sample size is clearly evident in the larger standard errors among the estimated impacts for the weak mandates, further analysis is required to see if the results reflect factors other than the policy introductions.

States that are included in both the "No IVF" and "Not all insurance firms" groups are New York, Montana and West Virginia. Out of these three states, New York is the largest and is thus likely to be dominating the results. There are several potential reasons why women in New York may exhibit significant delay. One possibility is that women in New York have more access to fertility clinics compared to women in other states. However, the annual ART Success Rates Reports published by the Centers for Disease Control and Prevention reports that other states such as California and Illinois and Texas also all have equally many fertility clinics. Another potential explanation is that these women are inherently different from the others and that they would have delayed birth even in the absence of the mandates. This would be the case, for example, if these women are more career driven prior to the introduction of the mandates and thus had strong tendencies to delay birth. If this is true, the estimated policy impacts do not reflect the results of the introduction of the mandates but simply highlight the differences between women in New York and the non-mandated states.

To investigate this issue further, regressions excluding New York are run to see the size of impact New York has on the estimates. In Column (4), IVF covered states excluding New York are compared to the states which excludes IVF. The size of the estimates are now smaller, although the “No IVF” states still seem to suggest a delaying impact. On the other hand, Column (5) reports estimates of “All insurance firms” versus “Not all insurance firms” (excluding New York). Again, New York does seem to affect the size of the estimate, but the “Not all firms” mandate coefficient is still negative and significant. From these results, New York seems to be one of the factors contributing to the large negative estimates of the weaker mandates, but is not the only cause.

The results in this section imply that women who were affected by the mandates exhibit approximately 1-2 years of delay depending on the age at which they were affected. Although plagued by small sample size problem, further analyses on the impact of mandates by differential coverage suggest potential differences other than the mandate introductions in women who were in the mandated and non-mandated states.

2.9 Robustness checks

The previous section presented evidence of delay in the timing of birth in response to the introduction of the state infertility health insurance mandates. This section describes an additional analysis that is carried out to ensure the robustness of the findings.

2.9.1 Analysis of the plausibility of the results

The estimated delay from the previous section suggests 1-2 years of delay in the timing of birth, which seems to be large. To see the plausibility of these results, Figure 2.6 plots trends of the average age at first births for highly educated white women in the non-mandated and mandated states between 1980-1997. These trends are calculated using the 1980-1997 NCHS's Vital Statistics Natality Birth Data. To ensure that only states that are actually affected by the mandates contribute to the average, the mandated states include states from the year of enactment. For example, New York is included in the non-mandated group until 1989 and is defined as the mandated state only from 1990, which was the year New York enacted its mandate.

These figures indicate that women in both groups of states experienced increases in the age at first birth during this period. However, the size of the delay is larger for women in the mandated states. In particular, whilst women in non-mandated states increased their age at first birth by approximately 1.5 years, those women residing in the mandated states went through an increase of 3.5 years. These raw statistics suggest delaying of approximately 3 more years among the women in mandated states compared to those in the non-mandated states. The national statistics, thus, support the estimated delay reported in the previous section.

2.9.2 Test for the identification assumption

Identification strategy employed in this chapter requires that the infertility insurance mandates are exogenously introduced. If, instead, these mandates were introduced in response to greater demands for infertility treatment, the employed identification strategy would not reveal the policy impact. However, there are two main reasons to believe that the introduction of the mandates do not directly reflect the demand for infertility treatment.

Firstly, insurance mandates were popular in the US between 1970s and 1990s. In fact, Jensen and Morrisey (1999) showed that the number of mandates increased by 25 folds during this period from 35 to 860.[21] Jensen and Morrisey (1999) also argued that the philosophy towards health insurance mandates differed significantly across states, and a state with a large number of mandates is more likely to pass new insurance mandates. [22] This fact seems to suggest the state-level preference

[21] One of the policies that could also contribute to the delaying of birth is the mandate to cover for contraceptive methods. However, such mandates only came in effect from 1998. Maternity leave policies are also likely to be important when studying the timing of births. The first paid maternity leave was introduced in California in 2002. As our analysis only covers up until 1997, the estimated results are free of the influences from these policies.

[22] Lambert and McGuire (1990) also show that the states with many mandates were more likely

towards insurance mandates rather than the demand for infertility treatment as a driving force behind the enactments of the mandates.

Secondly, the lobbying activity for the infertility insurance mandates is mainly carried out by a non-profit organization, RESOLVE (Fulwider, 2009). RESOLVE actively seeks coverage for infertility treatment on local, state and national levels. It is founded in 1974 and run by a group of volunteers broadly consisting of both health care professionals and individuals who have had personal experiences with infertility and/or adoption. Although there is a concern that RESOLVE's choice of states is driven by the underlying demand for the infertility treatment within a state, there are several other states, where the lobbying activities took place but were not fruitful. Examples of these states include Virginia , which went through 6 attempts to enact the infertility mandate since 1990 (Audit and of the Virginia General Assembly, 2008), as well as Florida that holds the second largest number of infertility clinics in the US (the Centers for Disease Control and Prevention, 2000). Other such states also include Nebraska, Michigan, Maine, Pennsylvania, Arizona, Maryland, Missouri, Kansas, Michigan and Oklahoma.

The existence of these states with unsuccessful attempts highlights the potential importance of several factors other than the demand for infertility treatment, namely the opposition forces from the health insurance providers as well as the concerns among the policymakers regarding the moral ethics involved with the infertility treatment. Indeed, the two case studies in the state of Illinois and Nevada carried out by Fulwider (2009) reveal that the main debates among the policymakers regarding the passing of the infertility insurance bill involved the potential cost of the mandate towards the health insurance providers and employers. In addition, some senators raised the issues of moral and ethical dilemma associated with infertility treatment. These policymarkers argued that ART procedure resembles that of selective abortion, since it involves selections of eggs for the purpose of implantation

to introduce a new mandate for mental health.

and abortions in the case of multiple pregnancies.

The background information strongly suggests that the infertility mandates were exogenously introduced. Nonetheless, Table 2.7 presents results from a placebo test, where Eq. (2.7) is estimated using the pre-policy period 1970-1985 PSID data. Looking at a pre-introduction period assures similarities in fertility behavior between the affected and not affected groups and hence ensures the robustness of the identification strategy. Since all except for West Virginia introduced their mandates after 1985, the period of observation presents women's fertility behavior in the absence of the policy interventions. Moreover, by the year 1985, various infertility treatments were already available. The selected period, therefore, allows us to see if highly educated women had differential preferences towards their birth timings when various treatments could be purchased without the health insurance coverage. Since West Virginia had already introduced the mandate in 1977, it is excluded from the estimation. The result from column (1) are reassuring with regard to the exogeneity of the policy introductions as the coefficient on the $Mandate$ dummy is small and statistically insignificant. In addition, Figure 2.7 plots the predicted hazard functions by treatment status and clearly indicates that these two groups exhibited a very similar trend in the absence of mandates.

Although the robustness checks so far seem to indicate no differences in the timing of birth between mandated and non-mandated states, estimated results in Section 2.8 raised a concern that there may be underlying differences between women residing in states with weak mandates and the others. If these women were indeed inherently different from the others, it is likely to observe the differential birth trends even before the introduction of the mandates. In order to test this, policy impacts are separately estimated for differential coverage. However, even when the policy variables are estimated separately by those living in "Mandate to cover" and "Mandate to offer", no evidence of differences prior to the introductions of mandates are found (column (2)). Moreover, estimated results are generally small, positive and insignificant and thus does not indicate any differences between the states with and without the IVF coverage(column (3)). Additionally, column (4) presents results for states that regulated all firms to follow the mandated states vs those that excluded some insurance firms. Again, there are no differential timing of birth prior to the introduction of these mandates.

2.9.3 Test for the assumptions in the empirical specification

Individuals who are found to be in the initial period at the age of 20 were likely to have faced different hazard rates compared to those who were aged 25 at the start of the observation period. In this chapter, it is assumed that such differences are controlled for by the inclusion of the initial age variable. In other words, the differences in the initial condition are assumed to be reflected in the proportional alteration of the baseline hazard. At the same time, this implies an additional assumption that the differences in the initial condition can be controlled for solely by observed characteristics.

In order to test for these assumptions, only individuals who enter the sample at the age of 20 are included. This makes sure that every woman is found to be in the initial period under the same condition. However, this reduces the sample size. As a result, observations with one or two missing years during the sample period are still included, filling these missing observations as long as their region of residence before and after the missing years are the same. This is likely to reduce the size of the estimates if women were moving during these unobserved years for reasons other than the mandates. On the contrary, this may amplify the size of the estimates if people moved to take advantage of the mandates.

Although the lack of observations restricts our analysis only to those who were affected from the age of 20, the results presented in Table 2.8 confirm the conclusion drawn in Section 2.8. Just as the results in Table 2.5, the coefficients in the second column, which show the differences in the hazard rate in each period between those who are affected and unaffected, indicate a delay of birth until the 5th period. Moreover, just as before, the differences are statistically significant in periods 2 and 4. Additionally, we now observe a significant reduction in the hazard also in the 5th period. Figure 2.8 presents the survival functions which are plotted using the estimates from Table 2.8. This figure suggests approximately a year delay at the median. The size of the delay is similar to the result in the main analysis for those who were affected from the age of 20 (see the left side figure in Figure 2.5).

2.10 Conclusions

This chapter investigates the impact of the US infertility state mandates on the timing of first birth. A discrete-time proportional hazard model is estimated allowing for a flexible nonparametric baseline hazard as well as gamma unobserved heterogeneity.

In contrast to the past literature, which has focused on how these mandates affected older women, the present paper looks at policy impacts on younger women. In other words, while women who undergo infertility treatment are generally older, it proposes the existence of a potential effect on younger cohorts of women who were likely to have been planning to have a child in the future. Facing the difficulties in balancing work and life, these women may have incorporated the availability of cheap and thus more accessible infertility treatment into their life cycle plan. If this is the case, we should observe a delay in the time to first birth among the affected women.

The results from the discrete-time proportional hazard model indicate an insignificant effect of the mandates when the entire sample is included and the effect is assumed to be the same across educational group. However, a significant negative effect of these mandates on the timing of first birth is observed among white women with more than 13 years of education. Moreover, when separate baseline hazard functions are estimated, evidence suggests that individuals affected by the mandates for at least two years were delaying birth. Moreover, the size of the delay depended on the age at which these women became exposed to the mandates. For example, at the median of the survival function, affected white women are estimated to have delayed their first birth for 1 year if they were exposed to the legislation for two years by the the age of 20 or 25. The size of delay becomes even larger when they were affected at the age of 30. In particular, these women are observed to have delayed their first birth for 2 years. The estimated policy impact translates to approximately a 14% increase in the number of women who face infertility. This implies an increase of approximately 0.37 million infertile women.

There are two potential explanations for why we observe stronger impacts among the women exposed to the mandates at older ages. Firstly, the older childless women had already delayed birth possibly for career or educational reasons and thus are likely to be the sample of women who had a stronger incentives to delay birth in order to balance work and life. Secondly, the notion of pregnancy and timing of birth is likely to be more of a serious issue for women who were at the age of 28 than those who were younger.

Results broken down by the level of coverage indicated that women in weaker mandated states seem to be responding more strongly by delaying birth. This raises a concern as it may indicate an underlying cause of the delay observed other than the state-level infertility health insurance mandates. However, the small sample size, reflected in the large standard errors, raises a concern over the precision of the estimates.

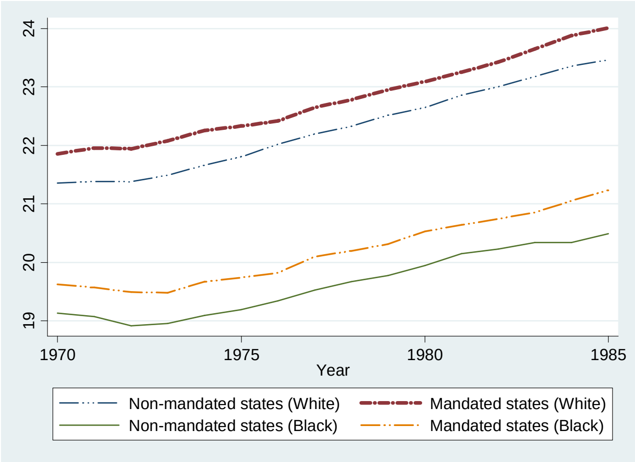
In order to confirm no differential trends between the affected and unaffected groups, robustness checks were carried out using the pre-policy period (1970-1985) data. If affected women were different from the other women, such differences are likely to be observed prior to the introduction of the mandates. However, no matter how we divide the sample, we observe no differences between the two groups of women and thus indicating the robustness of the delaying effect found in this chapter.

Two further assumptions regarding the initial conditions of individuals in the sample are tested by using only those women who turned 20 at the beginning of the observation period. Although the smaller sample size only allows us to study the effect among individuals who were affected for two years by the age of 20, the results from this sample draws the same conclusion as those in the main analysis.

This chapter demonstrate that the introduction of infertility insurance state mandates not only affected those who are directly targeted, but had a wider policy impact on the timing of birth. Further research is needed in order to uncover how the timing of second birth was affected by these mandates. Due to the delay of first birth, women may have had their second child significantly after the age of 35 further increasing the health risks for both mothers and children. Moreover, such an analysis would inform us as to whether the infertility health insurance mandates affected the total fertility rate.

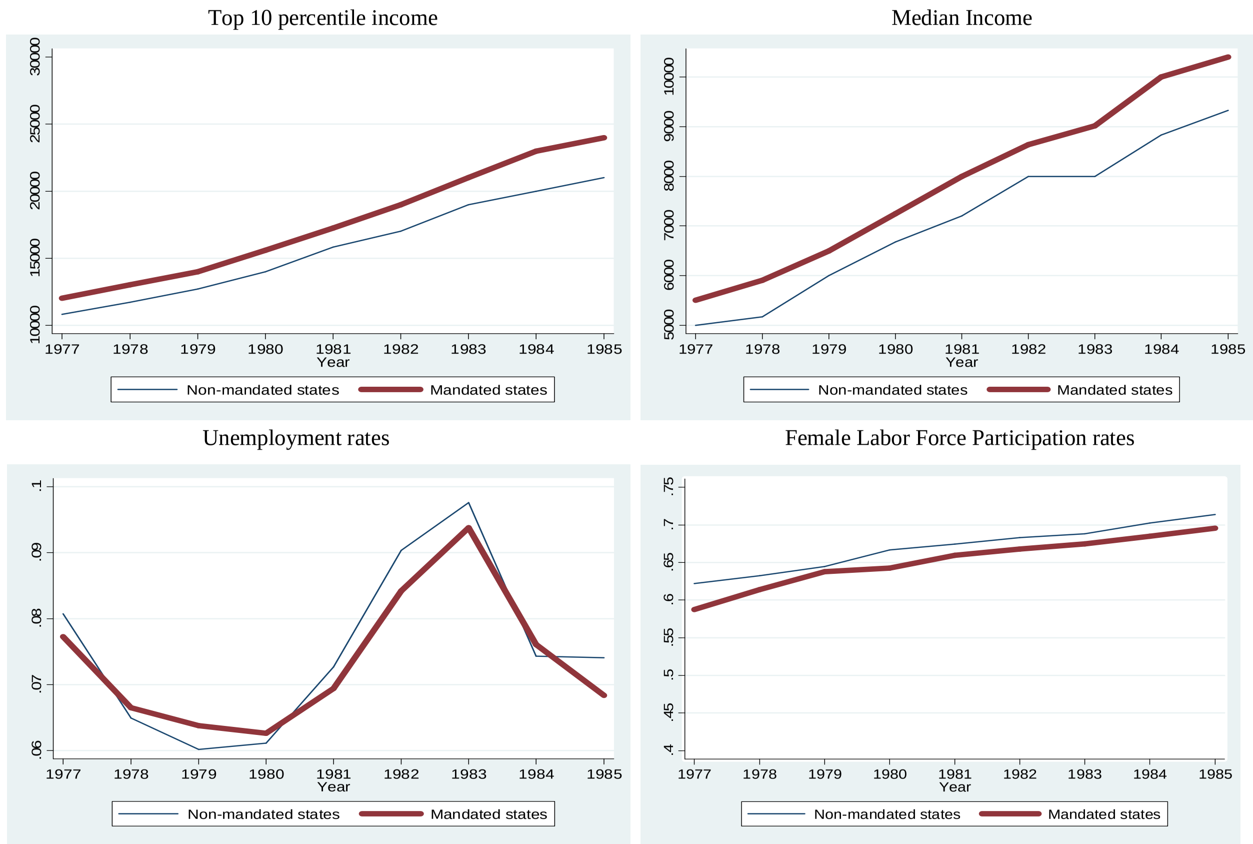
2.11 Chapter Two: Figures and Tables

Figure 2.1: Trends of mean age at first birth by mandate status and race



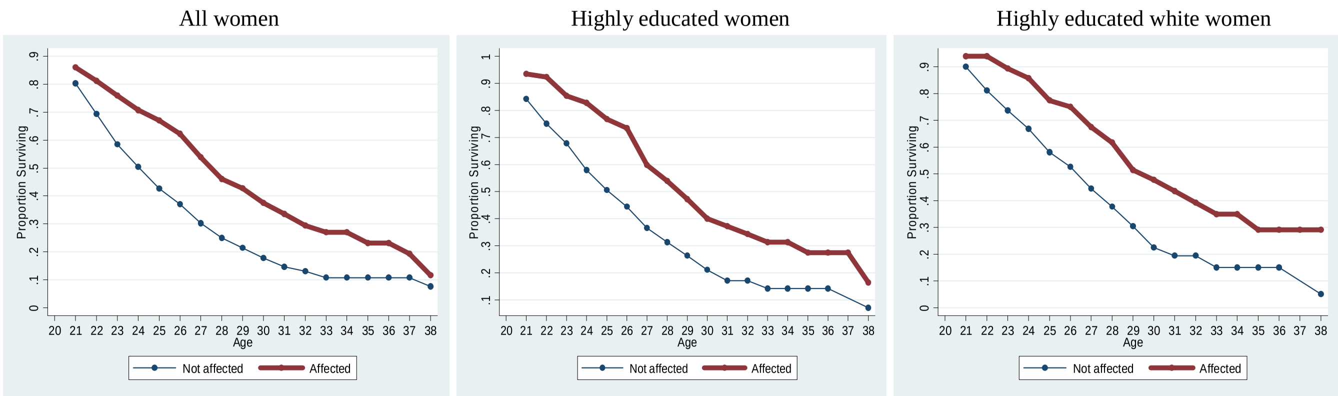
Notes: This figure presents the trends of age at first birth by race and mandates status. Statistics are calculated for the period prior to the introduction of mandates (1970-1985). The ages at first birth are computed using the NCHS's Vital Statistics Natality Birth Data.

Figure 2.2: Trends of economic characteristics by mandate status



Notes: These four figures present pre-policy period economic characteristic trends by mandate status. Data is taken from the 1977-1985 Current Population Survey. In each figure, the thin and thick lines show the trends experienced by women residing in the non-mandated and mandated states respectively.

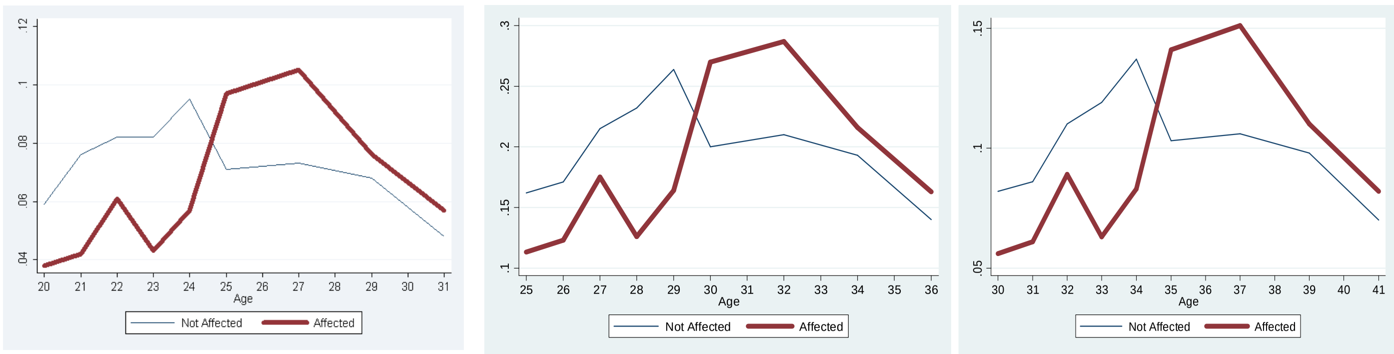
Figure 2.3: Life table survival functions



Note: In each of these figures, the life table estimates of the survival functions are plotted for two groups (i.e. affected and not affected groups) separately. The left figure shows the survival functions when all women are included. On the other hand, the middle and left figures each presents the graph for highly educated women and highly educated white women respectively. Data employed is the 1980-1997 Panel Study of Income Dynamics. Every point on these lines displays the proportion of women remaining childless until a particular age. The thin lines show survival rates for women who are unaffected by the mandates whilst the thick lines present that for women who were exposed to the mandates for at least two years by the age of 20.

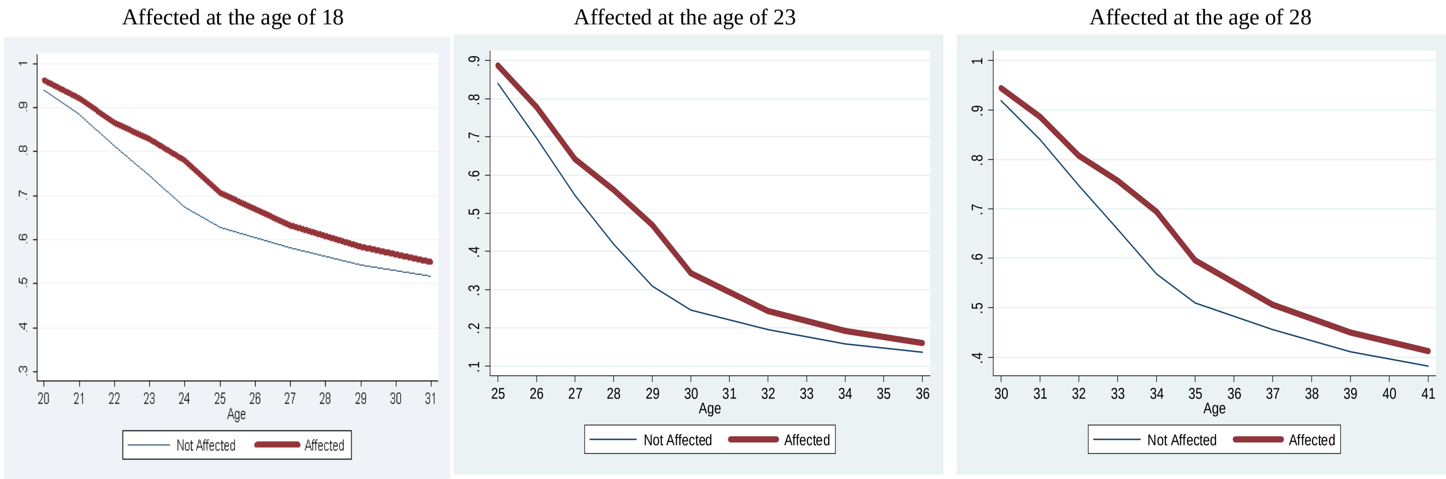
Figure 2.4: Predicted hazard functions(White and highly educated women)

Affected at the age of 18 | Affected at the age of 23 | Affected at the age of 28



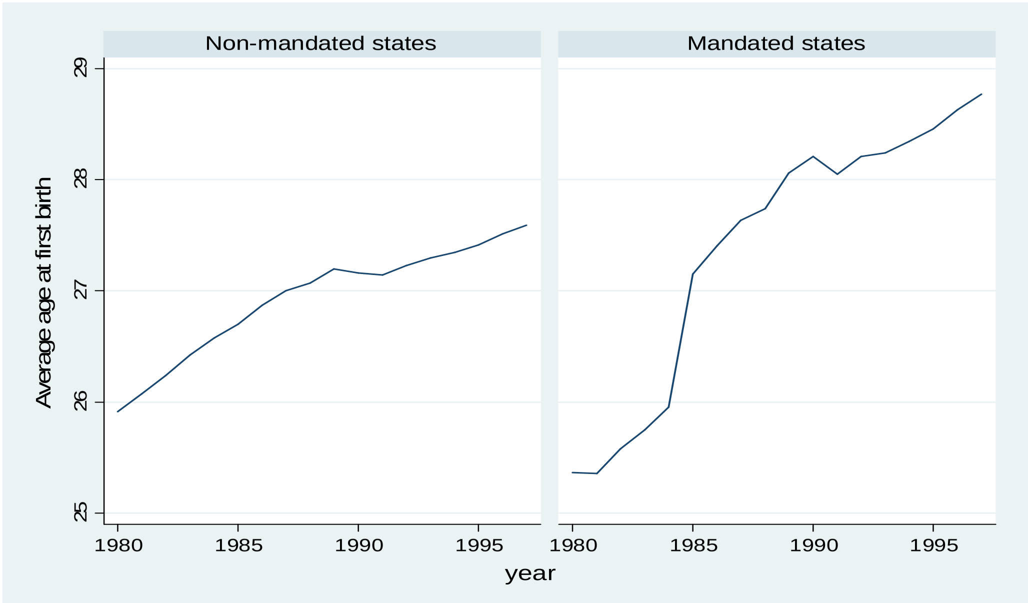
Notes: The predicted hazard functions of white highly educated women are plotted for two groups separately. Each figure compares the survival rates of unaffected women and women who were exposed to the mandates for at least two years by the age of20, 25 and 30 respectively. Every point on these lines displays the conditional probability of having a first child at a particular age. The thin lines show the predicted hazard for women who are unaffected by the mandates whilst the thick lines present the predicted hazard for women who were exposed to the mandates for at least two years. These probabilities are estimated using the discrete-time proportional hazard estimates with piece-wise constant baseline hazard and gamma unobserved heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. The estimates for the baseline hazard and covariates are included in Table 5 and Table 9.

Figure 2.5: Survival functions (White and highly educated women)



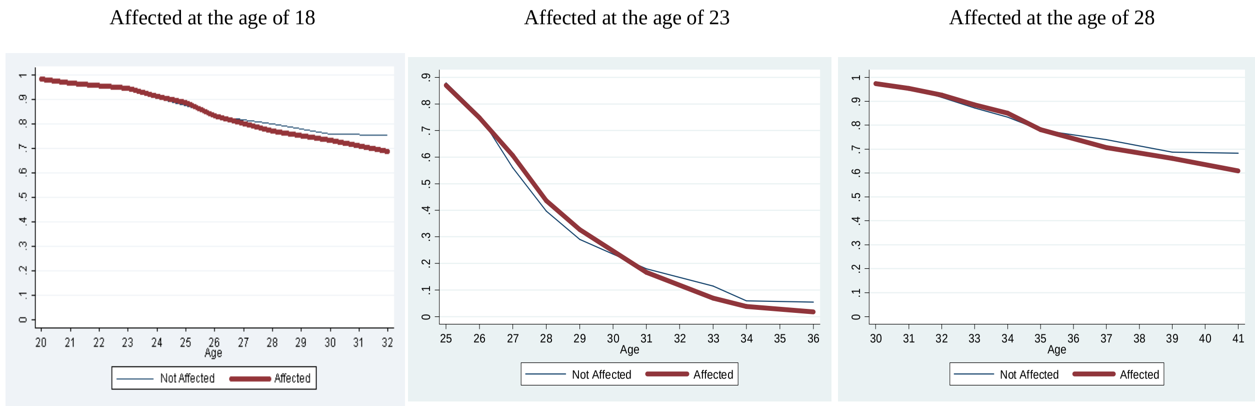
Notes: Above figures present the survival functions of white highly educated women. Each figure compares the survival rates of unaffected women and women who were exposed to the mandates for at least two years by the age of 20, 25 and 30 respectively. Points on these lines indicate the probabilities of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those women who were exposed to the policies for at least two years at a particular age. These probabilities are estimated using the discrete-time proportional hazard estimates with piece-wise constant baseline hazard and gamma unobserved heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The estimates for the baseline hazard and covariates are included in Table 5 and Table 9 in the appendix.

Figure 2.6: Trends of mean age at first birth by mandate status (White and highly educated women)



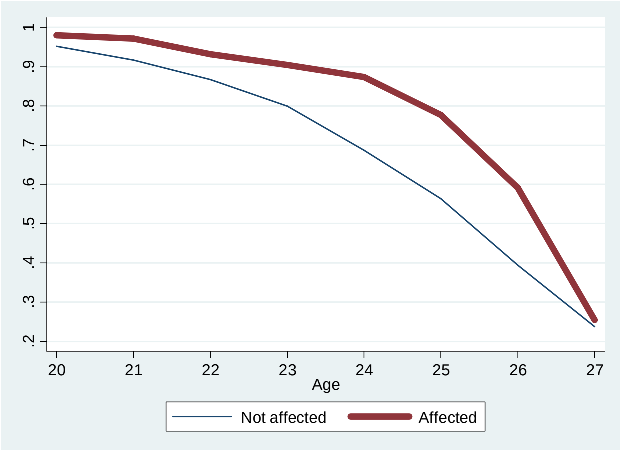
Note: These figures plot trends of age at first birth for the highly educated white women during the period of 1980-1997. The left figure plots the trends of the non-mandated states. The right figure, on the other hand, shows the trend of the mandated states. The states are included in right figure from the year in which they enact the mandates. For example, New York is defined to be a non-mandated state until 1989 and is included in the mandated state group only from 1990. Statistics are calculated using the NCHS's Vital statistics natality data.

Figure 2.7: Survival functions (Pre-policy period analysis)



Note: These figures display the survival functions calculated using the pre-policy period Panel Study of Income Dynamics data (1970-1985). For robustness check, an arbitrary year was chosen to assume the introduction of the mandates. Each figure compares the survival rates of unaffected and affected women at the initial age of 18, 23 and 28 respectively. Every point on each line indicates the probability of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those for women who were assumed to have been exposed to the pseudo policies. These probabilities are estimated using the discrete-time proportional hazard estimates with piece-wise constant baseline hazard and gamma unobserved heterogeneity.

Figure 2.8: Robustness check: survival functions (White and highly educated women)



Notes: Above figure presents the survival functions of white highly educated women when only those who turned 20 are included in the sample. The figure compares the survival rates of unaffected women and women who were exposed to the mandates for at least two years by the age of 18. Points on these lines indicate the probabilities of remaining childless until a particular age. The thin lines show these probabilities for women who were unaffected by the mandates and the thick lines indicate those women who were exposed to the policies for at least two years at a particular age. These probabilities are estimated using the discrete-time proportional hazard estimates with piece-wise constant baseline hazard and gamma unobserved heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The estimates for the baseline hazard and covariates are included in Table 8 and Table 9.

Table 2.1: Treatment options, success rates and costs

Treatment	Description	Success rates	Cost	Multiple births
Fertility Drugs	Regulate reproductive hormones and trigger the release of more eggs per cycle.	20-60 percent (often with IUI)	Clomipihene: Minimum $50 per cycle Gonadotropins:$2000-$5000 including tests, drugs and medical check ups.	Yes (8-10 percent twin rate for Clompihene, 15 percent for Gonadotrophin)
Surgery	Unblocking the fallopian tubes or removing endometrial scarring, fibroids, or ovarian cysts.	40-60 percent (if treated for endometriosis and scar tissues) 10-90 percent (if treated for blocked fallopian tubes)	$3,000- $10,000	
Intrauterine insemination (IUI)	A concentrated dose of sperm is injected into the uterus or fallopian tubes with a catheter.	5 to 20 percent	$300-$700 ($1,500-$4,000 including medication and ultrasound monitoring).	Yes if fertility drug is also used in conjunction to this method.
In vitro fertilisation (IVF)	Eggs removed from the ovaries are fertilised with sperm in a laboratory, and the resulting embryos are transplanted back to the uterus.	28 and 35 percent	$8,000-$15,000 per cycle $50000 until success or $44,000 and $211,940 (Neumann, Gharib, and Weinstein (1994))	Yes(20-25% chance)
Gamete intrafallopian transfer (GIFT)	Eggs and sperm are harvested and mixed together in a lab. The mixture is surgically injected into the fallopian tubes so fertilisation can happen naturally inside the body.	25 to 30 percent	$8 000 - $15 000	Yes
Intracytoplasmic sperm injection (ICSI)	A single sperm is injected into a single egg and the resulting embryo is transplanted into the uterus.	35 percent	$10,000 - $17,000 per cycle	Yes
Donor sperm	Donated sperm is used during an IUI treatment. IVF techniques can also be carried out using donor sperm.	20 to 26 percent (when used with IVF)	$200-$3000 per unit of semen	(Yes, if other treatment is used together)
Egg (or embryo) donation	An egg (or embryo) donated by another woman is mixed with sperm and implanted in the recipient's uterus.	43 percent (when used with IVF)	$4,000 -$5,000	(Yes. 20-25% chance)
Surrogacy	Another woman carries a couple's embryo, or a donor embryo, to term.	Not Available	$15,000- $50,000	
Zygote Intrafallopian Transfer (ZIFT)	Similar to GIFT but the doctors make sure the egg is fertilized before implanting it into the womb.	25 to 30 percent	$8 000 -$15 000	Yes

Source:Getting Pregnant (2009) *Sperm Donation*, last revised 2009, Retrieved August 20, 2009 from http://www.wdxcyber.com/sperm_donation.html

BabyCenter (2009) *Fertility treatment: Your options at a glance*, last revised 2009, Retrieved August 20, 2009 from http://www.babycenter.com/0_fertility-treatment-your-options-at-a-glance_1228997.bc?page=1

Table 2.2: States with mandate coverage

State	Year law passed	Mandate to		IVF is		Law applies to			Upper age limit
		cover	offer	included	excluded	All firms	Non-HMOs	Only HMOs	
Arkansas	1987	Y	N	Y	N	N	Y	N	
California	1989	N	Y	N	Y	Y	N	N	
Connecticut	1989	2005 onwards	Before 2005	Y	N	N	Y	N	Below 40 (2005~)
Hawaii	1987	Y	N	Y	N	Y	N	N	
Illinois	1991	Y	N	Y	N	Y	N	N	
Louisiana	2001	Y	N	N	Y	Y	N	N	
Maryland	1985	Y	N	Y	N	Y	N	N	
Massachusetts	1987	Y	N	Y	N	Y	N	N	
Montana	1987	Y	N	N	Y	N	N	Y	
New York	1990	Y	N	N	Y	N	Y	N	21-44 (2002~)
New Jersey	2001	Y	N	Y	N	Y	N	N	Below 46
Ohio	1991	Y	N	Before 1997	1997 onwards	N	N	Y	
Rhode Island	1989	Y	N	Y	N	Y	N	N	25-40 (2006~)
Texas	1987	N	Y	Y	N	Y	N	N	
West Virginia	1977	Y	N	N	Y	N	N	Y	

Sources: Bitler (2008), Resolve (2008), and The New York Times (2002)
Notes: This table presents the states that had implemented the state-level mandates and summarizes the extent of their coverage. Mandate "to cover" is a type of mandate that requires insurance companies to cover the infertility treatment cost regardless of the insurance policies purchased. On the other hand, mandate "to offer" simply regulates insurance providers to offer infertility insurance policies to customers.

Table 2.3: Summary statistics

	1970-1980				1980-1997			
	Treatment		Control		Treatment		Control	
	Mean	S.D	Mean	S.D	Mean	S.D	Mean	S.D
Age in the first period	21.65	2.90	21.09	3.05	22.47	3.16	21.90	2.71
Birth (1 if birth observed)	0.13		0.13		0.10		0.18	
State-level economic indicators								
Median annual income	7033.63	1593.85	6229.25	1646.40	14668.93	3806.48	13440.55	3859.00
Top 10 percentile annual income	17621.79	4195.27	16032.85	4106.10	38772.77	9735.18	35580.74	9585.53
Female labor force participation rate	0.46	0.04	0.46	0.05	0.54	0.04	0.54	0.05
Female unemployment rate	0.07	0.02	0.07	0.02	0.07	0.02	0.07	0.03
Ethnicity dummies								
White	0.92		0.87		0.90		0.88	
Black	0.07		0.13		0.09		0.11	
Education dummies								
Highest grade attended 1-5	0.001		0.004		0.01		0.01	
Highest grade attended 6-8	0.001		0.01		0.00		0.01	
Highest grade attended 9-12	0.43		0.47		0.30		0.39	
Highest grade attended 13 or more	0.56		0.52		0.70		0.59	
Region of Residence dummies								
New England	0.19		0.00		0.17		0.02	
Mid-atlantic	0.20		0.20		0.20		0.23	
Mid-west	0.27		0.37		0.21		0.26	
South Atlantic	0.03		0.18		0.04		0.22	
East South	0.00		0.09		0.00		0.09	
West South	0.11		0.03		0.14		0.03	
Mountain	0.00		0.07		0.00		0.08	
Pacific	0.20		0.05		0.24		0.07	
Starting year dummies								
1 if the observation enters in the sample in 1970/1980	0.38		0.43		0.40		0.34	
1 if the observation enters in the sample in 1971/1981	0.06		0.05		0.05		0.04	
1 if the observation enters in the sample in 1972/1982	0.11		0.08		0.06		0.05	
1 if the observation enters in the sample in 1973/1983	0.08		0.08		0.05		0.06	
1 if the observation enters in the sample in 1974/1984	0.07		0.08		0.07		0.05	
1 if the observation enters in the sample in 1975/1985	0.07		0.08		0.06		0.06	
1 if the observation enters in the sample in 1976/1986	0.09		0.07		0.04		0.06	
1 if the observation enters in the sample in 1977/1987	0.06		0.03		0.05		0.04	
1 if the observation enters in the sample in 1978/1988	0.05		0.03		0.02		0.04	
1 if the observation enters in the sample in 1979/1989	0.03		0.05		0.05		0.05	
1 if the observation enters in the sample in 1980/1990	0.01		0.01		0.06		0.05	
1 if the observation enters in the sample in 1991					0.01		0.04	
1 if the observation enters in the sample in 1992					0.02		0.03	
1 if the observation enters in the sample in 1993					0.02		0.03	
1 if the observation enters in the sample in 1994					0.02		0.02	
1 if the observation enters in the sample in 1995					0.01		0.02	
1 if the observation enters in the sample in 1996					0.01		0.01	
1 if the observation enters in the sample in 1997					0.01		0.01	
Year dummies								
1 if observed in 1970/1980	0.07		0.07		0.06		0.05	
1 if observed in 1971/1981	0.07		0.07		0.06		0.05	
1 if observed in 1972/1982	0.07		0.07		0.00		0.00	
1 if observed in 1973/1983	0.06		0.07		0.06		0.05	
1 if observed in 1974/1984	0.08		0.09		0.06		0.05	
1 if observed in 1975/1985	0.09		0.10		0.05		0.06	
1 if observed in 1976/1986	0.10		0.10		0.06		0.06	
1 if observed in 1977/1987	0.11		0.10		0.06		0.06	
1 if observed in 1978/1988	0.11		0.10		0.05		0.05	
1 if observed in 1979/1989	0.13		0.12		0.06		0.06	
1 if observed in 1980/1990	0.12		0.12		0.07		0.06	
1 if observed in 1991					0.06		0.06	
1 if observed in 1992					0.06		0.06	
1 if observed in 1993					0.07		0.07	
1 if observed in 1994					0.07		0.07	
1 if observed in 1995					0.05		0.06	
1 if observed in 1996					0.06		0.07	
1 if observed in 1997					0.04		0.05	
Number of observations	2103		3552		3997		6832	
Number of individuals	586		1015		1001		1684	

Note: This table reports the averages and standard deviations of variables taking account of the survey data structure of PSID. Treatment group includes women who were residing in states that introduced mandates sometime during the observation period. The first two columns report the summary statistics of variables from the pre-policy period data (i.e.1970-1980) while the third and fourth columns show that of the post-policy period data (i.e. 1980-1997).

Table 2.4: Estimates of mandates effect

	(1)	(2)	(3)	(4)
	All Women	By education		
		10≤Education≤12	13≤Education	
	All race	All race	All race	White
Mandate (Policy×After)	0.03	0.30*	-0.38**	-0.54**
	(0.12)	(0.17)	(0.17)	(0.22)
Policy	0.14	0.12	0.16	0.08
	(0.42)	(0.33)	(0.46)	(0.61)
LR test of gamma variance	12.40***	2.28*	1.60	15.30***
Number of women observed	2685	1339	1180	839
Observations	10829	4794	4925	3662

Notes: This table displays key policy impact variables from discrete-time proportional hazard estimates with heterogeneity. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed in a piece-wise constant baseline hazard and gamma unobserved particular year and 0 otherwise. The estimates for the baseline hazard and covariates are included in Table 9 in the appendix. Covariates included are: age of individuals in the first year of observation and its squared term, race, education and region of residence dummies, state-level characteristics, year fixed effects and start year dummies. The flexible baseline hazard is assumed to be common between the treatment and control groups.
Column (1) shows regression results when all women in the sample are included.
Column (2) shows results estimated using a sample of women with 10 to 12 years of education.
Column (3) shows results for women with more than 13 years of education
Column (4) shows results for white women with more than 13 years of education.
Standard errors are bootstrapped to take account of state-level clustering and are shown in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: Estimated baseline hazard

Periods (t)	Coefficients	Periods(t)	Coefficients
		Mandate_period1	-0.58
			(0.46)
period2	0.06	Mandate_period2	-0.55***
	(0.13)		(0.20)
period3	0.32*	Mandate_period3	-0.42
	(0.18)		(0.36)
period4	0.40*	Mandate_period4	-0.87***
	(0.23)		(0.31)
period5	0.55*	Mandate_period5	-0.73
	(0.30)		(0.48)
period6/7	0.25	Mandate_period6/7	0.14
	(0.33)		(0.27)
period8/9	0.27	Mandate_period8/9	0.19
	(0.44)		(0.39)
period10/11	0.20	Mandate_period10/11	-0.07
	(0.57)		(0.23)
period12/15	-0.16	Mandate_period12/15	-0.02
	(0.63)		(0.37)

Notes: This table displays the baseline hazard estimates from discrete-time proportional hazard model with piece-wise constant baseline hazard and gamma unobserved heterogeneity. The first column shows the piece-wise constant baseline hazard for the unaffected individuals whereas the second column includes the difference in hazard between the affected and unaffected individuals. Number of individuals in the sample is 1180 contributing binary responses of 4925. LR test of gamma variance reports a chi squared statistics of 1.713*. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. The estimates for the covariates are included in Table 9 in the appendix. Standard errors are bootstrapped to take account of the state-level clustering and are reported in parentheses. p<0.01, ** p<0.05, * p<0.1 ***

Table 2.6: Policy impacts by differential coverage

	Highly educated women only				
	(1)	(2)	(3)	(4)	(5)
	Cover vs Offer	IVF vs no IVF	All firms vs Not all firms	IVF vs no IVF (excluding New York)	all firms (excluding New York)
Mandate_Cover	-0.43*				
	(0.26)				
Mandate_Offer	-0.33				
	(0.21)				
Policy_Cover	0.1				
	(0.47)				
Policy_Offer	0.41				
	(0.73)				
Mandate_IVF covered		-0.18*		-0.18*	
		(0.11)		(0.11)	
Mandate_IVF not covered		-0.71		-0.35	
		(0.44)		(0.29)	
Policy_IVF covered		-0.01		-0.02	
		(0.50)		(0.17)	
Policy_IVF not covered		0.47		0.22	
		(0.68)		(0.28)	
Mandate_All insurance firms			-0.13		-0.12
			(0.15)		(0.12)
Mandate_Not all insurance firms			-1.01**		-0.67**
			(0.40)		(0.31)
Policy_All insurance firms			0.09		0.04
			(0.44)		(0.20)
Policy_Not all insurance firms			0.31		0.01
			(0.62)		(0.32)
LR test of gamma variance	3.60**	1.53	3.55**	1.34	2.51*
Number of women observed	1180	1180	1180	1110	1110
Observations	4925	4925	4925	4635	4635

Notes: This table displays key policy impact variables estimated separately by the characteristics of the mandate.
These results were estimated using the dicrete-time proportional hazard model with piece-wise constant baseline hazard
and gamma unobserved heterogeneity. The dependent variable is a dummy which equals to one if birth observed 0 otherwise.
The estimates for theh baseline hazard and covariates are included in the appendix (Table 9)

Table 2.7: Test for differencial trends between the mandated and non-mandated states

	Highly educated women only			
	(1)	(2)	(3)	(4)
	13≤Education	Cover vs Offer	IVF vs no IVF	All firms vs Not all firms
Mandate	-0.01			
	(0.22)			
Policy	0.02			
	(0.19)			
Mandate_Cover		-0.14		
		(0.28)		
Mandate_Offer		0.07		
		(0.26)		
Policy_Cover		0.03		
		(0.21)		
Policy_Offer		-0.14		
		(0.28)		
Mandate_IVF covered			-0.14	
			(0.26)	
Mandate_IVF not covered			0.12	
			(0.29)	
Policy_IVF			-0.00	
			(0.23)	
Policy_IVF not covered			0.06	
			(0.30)	
Mandate_All insurance firms				0.14
				(0.27)
Mandate_Not all insurance firms				-0.12
				(0.33)
Policy_All insurance firms				-0.10
				(0.26)
Policy_Not all insurance firms				0.22
				(0.29)
LR test of gamma variance	47.01***	46.18***	46.51***	55.68***
Number of women observed	939	939	939	939
Observations	4257	4257	4257	4257

Note: This table displays regression results from the robustness analyses, which are estimated using the pre-policy 1970-1985 PSID. The estimates shown are the key variables from the discrete-time proportional hazard model with piece-wise constant baseline hazard and gamma unobserved heterogeneity. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. Covariates included are: age of individuals in the first year of observation and its squared and cubed terms, race, education and region of residence dummies, state-level economic indicators, state fixed effect and start year dummies. In column (1), policy impacts are estimated for all highly educated women. Column (2) presents results of differential policy impacts among highly educated women affected by "mandate to cover" and "mandate to offer". Column (3) show results for states with and without the IVF coverage and Column (4) includes that for women residing in mandates which regulated all health insurance firms and those that excluded some firms.
Standard errors are bootstrapped to take account of the state-level clustering and are reported in parentheses. *** p<0.01, **p<0.05, p*<0.1

Table 2.8: Test for the assumptions regarding the sampling scheme

Periods (t)	Coefficients	Periods(t)	Coefficients
		Mandate_period1	-0.51
			(0.507)
period2	-0.27	Mandate_period2	-0.99***
	(0.180)		(0.343)
period3	0.13	Mandate_period3	0.15
	(0.181)		(0.254)
period4	0.53**	Mandate_period4	-0.66*
	(0.245)		(0.399)
period5	1.15***	Mandate_period5	-1.15***
	(0.245)		(0.402)
period6/7	1.42***	Mandate_period6/7	-0.17
	(0.314)		(0.268)
period8/9	1.98***	Mandate_period8/9	0.11
	(0.449)		(0.340)
period10/11	2.48***	Mandate_period10/11	0.73**
	(0.644)		(0.306)
period12/15	2.97***	Mandate_period12/15	-0.14
	(0.794)		(0.413)

Notes: This table shows results estimated by using a sample of women who were 20 in the initial period. The first column shows the piece-wise constant baseline hazard for the unaffected individuals whereas the second column includes the difference in hazard between the affected and unaffected individuals. LR test of gamma variance reports a chi squared statistics of 10.82***. Data employed is the 1980-1997 Panel Study of Income Dynamics. The dependent variable is a dummy which equals one if birth observed and 0 otherwise. The estimates for the baseline hazard and covariates are included in Table 9 in the appendix. Number of individuals in the sample is 1101 contributing binary responses of 4125. Standard errors are bootstrapped to take account of the state-level clustering and are reported in parentheses. p<0.01, ** p<0.05, * p<0.1 ***

Conclusions

This thesis presents empirical evidence on how financial incentives affect the timing of birth.

The first chapter looks at the effect of the UK WFTC policy on the timing of first and subsequent birth using a multiple-spell discrete-time proportional hazard model with normal unobserved heterogeneity. The policy impact is identified using a difference-in-differences estimator and by comparing the timing of birth of women with different levels of educational attainment. The main data employed in the analysis is the 1991-2003 BHPS.

The findings from this chapter suggest that apart from the first birth, second and third birth timings were only insignificantly shortened, suggesting limited impacts of WFTC and other pronatalist benefits. The significant observed delay of the first birth, which suggests a delay of approximately 1.7 years, is likely to be due the following reasons. Firstly, WFTC may have been less effective as the marginal cost of having a first child is likely to be higher than that of the subsequent births. In fact, women with partners with at least one child responded positively by shortening the timing of birth, albeit insignificantly. Secondly, other labour market policies that were introduced during this period of time seem to have particularly affected the childless women's labour supply. Such an increase in the female labour supply is likely to have deterred women from entering motherhood.

The second chapter investigates the impact of the US infertility state mandates on the timing of first birth. A discrete-time proportional hazard model is estimated allowing for a flexible nonparametric baseline hazard as well as gamma unobserved heterogeneity. By employing the 1980-1997 PSID, this chapter identifies the impact of the mandates by a difference-in-differences estimator and exploiting the variation in the cost of infertility treatment across states and years.

Results indicate no delaying of birth when the entire sample of women is used, but significant delaying effect is found among the highly educated individuals with more than 13 years of education. The size of delay depended on the age at which these women became exposed to the mandates. For example, at the median of the survival function, affected white women are estimated to have delayed their first birth for 1 year if they were exposed to the legislation for two years by the age of 20 or 25. The size of delay becomes even larger when they were affected at the age of 30. In particular, these women are observed to have delayed their first birth for 2 years. The estimated policy impact translates to approximately 14% increase in the number of women who face infertility. This implies an increase of approximately 0.37 million infertile women.

Although it is not easy to summarise these findings from the two very different policies that affected different demographic groups of women, the evidence from this thesis suggests the existence of unintended consequences of these policies. In addition, the larger impact found in the second chapter seems to reconfirm the importance of female labour supply in determining women's fertility behaviour. The women affected by the UK WFTC and the other policies during this period were encouraged to increase their labour supply. This is likely to have made it difficult for women to balance work and life. On the other hand, the US women faced fewer dilemmas as a result of the mandates. By relaxing the biological constraints, the mandates are likely to have made it easier for women to pursue their careers. The future labour market policy planning as well as the empirical analysis of female labour supply, therefore, must take into consideration the potential unintended side effects on fertility. Moreover, an effective fertility related policy must combine the reduction in the tension women face between work and life.

A Appendix

Table A.1: Chapter1: All estimates from Tables 1.3, 1.4, and 1.5

VARIABLES	(1) birth	(2) birth	(3) birth	(4) birth	(5) birth	(6) birth	(7) birth
Piecewise constant baseline hazard							
Spell1							
period1	1.24***			1.09**	0.38	1.25***	1.00***
	(0.205)			(0.426)	(0.267)	(0.208)	(0.204)
period2	1.26***			1.34***	0.52*	1.27***	1.09***
	(0.215)			(0.421)	(0.273)	(0.217)	(0.211)
period3	1.66***			1.59***	0.85***	1.68***	1.47***
	(0.217)			(0.418)	(0.279)	(0.220)	(0.213)
period4	1.69***			1.76***	0.82***	1.71***	1.43***
	(0.229)			(0.418)	(0.296)	(0.232)	(0.228)
period5	1.58***			1.87***	0.74**	1.60***	1.28***
	(0.245)			(0.421)	(0.315)	(0.247)	(0.246)
period6	1.90***			1.93***	1.07***	1.94***	1.65***
	(0.248)			(0.427)	(0.320)	(0.251)	(0.247)
period7	2.17***			1.91***	1.42***	2.21***	1.88***
	(0.254)			(0.439)	(0.327)	(0.257)	(0.254)
period8	2.34***			1.86***	1.46***	2.36***	2.09***
	(0.266)			(0.455)	(0.348)	(0.268)	(0.265)
period9	2.49***			1.71***		2.51***	2.15***
	(0.281)			(0.479)		(0.284)	(0.286)
period10	2.45***			1.61***		2.47***	2.11***
	(0.305)			(0.508)		(0.309)	(0.312)
period11	2.66***					2.68***	2.44***
	(0.317)					(0.321)	(0.319)
period12	2.86***					2.86***	2.59***
	(0.333)					(0.338)	(0.334)
period11_12				1.09**			
				(0.519)			
period13_15				0.24			
				(0.639)			
period13	2.49***					2.51***	2.36***
	(0.392)					(0.396)	(0.391)
period14	2.42***					2.40***	2.25***
	(0.434)					(0.441)	(0.433)
period15_16	2.07***					2.08***	1.98***
	(0.437)					(0.441)	(0.450)
period17_19	1.27**					1.30**	1.06*
	(0.605)					(0.607)	(0.607)

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

This table includes estimates that were not included in Tables 1.3, 1.4, and 1.5 .

Column (1): Coefficient estimates from Table 1.3, column (1).
Column (2): Coefficient estimates from Table 1.4, column (1).
Column (3): Coefficient estimates from Table 1.4, column (2).
Column (4): Coefficient estimates from Table 1.4, column (3).
Column (5): Coefficient estimates from Table 1.5, column (1).
Column (6): Coefficient estimates from Table 1.5, column (2).
Column (7): Coefficient estimates from Table 1.5, column (3)

VARIABLES	(1) birth	(2) birth	(3) birth	(4) birth	(5) birth	(6) birth	(7) birth
Spell 2							
period1	1.81***			0.80*	1.43***	1.74***	1.67***
	(0.226)			(0.452)	(0.304)	(0.235)	(0.222)
period2	2.91***			2.31***	2.46***	2.86***	2.74***
	(0.222)			(0.412)	(0.309)	(0.230)	(0.216)
period3	3.65***			2.72***	2.95***	3.59***	3.44***
	(0.240)			(0.408)	(0.336)	(0.247)	(0.236)
period4	3.45***			2.47***	2.89***	3.42***	3.21***
	(0.284)			(0.426)	(0.395)	(0.291)	(0.284)
period5	3.99***			2.48***	2.89***	4.01***	3.76***
	(0.313)			(0.442)	(0.463)	(0.320)	(0.312)
period6	3.50***					3.34***	3.06***
	(0.426)					(0.428)	(0.437)
period7	4.50***					4.37***	4.04***
	(0.417)					(0.425)	(0.434)
period6_7				1.58***	2.85***		
				(0.486)	(0.493)		
period8_9				0.25	3.83***		
				(0.803)	(0.694)		
period8_10	4.38***					4.41***	4.03***
	(0.513)					(0.515)	(0.500)
period11_17	4.51***					4.42***	4.02***
	(0.804)					(0.806)	(0.773)
Spell 3							
period1	0.91**			1.01*	-0.11	0.62	0.84**
	(0.371)			(0.541)	(0.608)	(0.403)	(0.368)
period2	1.67***			1.51***	0.74	1.38***	1.46***
	(0.346)			(0.519)	(0.569)	(0.379)	(0.346)
period3	2.07***			0.76	1.00*	1.78***	1.84***
	(0.357)			(0.654)	(0.590)	(0.390)	(0.358)
period4	2.15***			0.98	0.85	1.96***	1.98***
	(0.393)			(0.659)	(0.648)	(0.418)	(0.390)
period5_6	2.37***				0.80	2.13***	2.09***
	(0.386)				(0.638)	(0.417)	(0.382)
period5_8_spe				0.03			
				(0.664)			
period7_8	2.57***				1.37*	2.19***	2.59***
	(0.554)				(0.786)	(0.585)	(0.533)
period9_12	2.23***					1.86**	2.03***
	(0.709)					(0.733)	(0.709)

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

This table includes estimates that were not included in Tables 1.3, 1.4, and 1.5 .

Column (1): Coefficient estimates from Table 1.3, column (1).
Column (2): Coefficient estimates from Table 1.4, column (1).
Column (3): Coefficient estimates from Table 1.4, column (2).
Column (4): Coefficient estimates from Table 1.4, column (3).
Column (5): Coefficient estimates from Table 1.5, column (1).
Column (6): Coefficient estimates from Table 1.5, column (2).
Column (7): Coefficient estimates from Table 1.5, column (3)

VARIABLES	(1) birth	(2) birth	(3) birth	(4) birth	(5) birth	(6) birth	(7) birth
Quadratic baseline hazard							
period×spell1		0.18					
		(0.245)					
period×period×spell1		-0.01					
		(0.013)					
period×spell2			-0.05				
			(0.075)				
period×period×spell2			-0.01				
			(0.007)				
period×spell3			-0.14				
			(0.130)				
period×period×spell3			-0.01				
Race							
White	-1.61***	0.50	-0.42	-0.68***	-2.11***	-1.60***	-1.62***
	(0.147)	(0.636)	(0.573)	(0.262)	(0.188)	(0.150)	(0.140)
Black	-0.75	2.02	-0.80	-0.10	-1.31**	-0.77	-0.66
	(0.573)	(1.411)	(0.704)	(0.705)	(0.590)	(0.582)	(0.533)
Housing tenure							
Owned	-0.54***	-0.49	0.03	0.17	-0.56**	-0.53***	-0.51***
	(0.183)	(0.574)	(0.305)	(0.212)	(0.235)	(0.183)	(0.181)
Rented	0.39***	1.52***	-0.32**	0.08	0.56***	0.39***	0.39***
	(0.096)	(0.507)	(0.125)	(0.136)	(0.113)	(0.097)	(0.095)
Region of residence							
North West	-1.94***	-1.80	-0.31	0.34	-1.49***	-1.95***	-1.82***
	(0.209)	(1.216)	(0.310)	(0.265)	(0.231)	(0.211)	(0.202)
Yorkshire	-1.99***	-3.09**	-0.52	0.26	-1.27***	-2.01***	-1.92***
	(0.232)	(1.323)	(0.328)	(0.273)	(0.246)	(0.235)	(0.226)
East Midlands	-1.76***	-1.75	-0.05	0.02	-1.11***	-1.79***	-1.66***
	(0.231)	(1.238)	(0.338)	(0.312)	(0.243)	(0.233)	(0.222)
West Midlands	-1.95***	-3.85**	-0.40	0.17	-1.48***	-1.95***	-1.90***
	(0.233)	(1.526)	(0.337)	(0.287)	(0.243)	(0.236)	(0.224)
East of England	-2.17***	-0.62	-0.17	-0.79**	-1.79***	-2.22***	-2.05***
	(0.233)	(1.061)	(0.312)	(0.325)	(0.274)	(0.236)	(0.226)
London	-2.81***	-1.77*	0.75**	-0.04	-2.37***	-2.83***	-2.75***
	(0.245)	(1.006)	(0.332)	(0.295)	(0.307)	(0.248)	(0.245)
South East	-2.38***	-2.13*	-0.30	0.02	-1.85***	-2.39***	-2.31***
	(0.213)	(1.138)	(0.300)	(0.266)	(0.241)	(0.215)	(0.208)
South West	-1.84***	-1.28	-0.12	0.28	-1.33***	-1.83***	-1.74***
	(0.233)	(1.074)	(0.354)	(0.285)	(0.252)	(0.235)	(0.224)
Wales	-1.75***	-1.61	-0.25	0.32	-1.04***	-1.77***	-1.66***
	(0.222)	(0.990)	(0.315)	(0.269)	(0.247)	(0.225)	(0.214)
Scotland	-2.10***	-1.65*	-0.25	0.24	-1.43***	-2.10***	-1.94***
	(0.208)	(0.958)	(0.301)	(0.250)	(0.231)	(0.210)	(0.202)

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

This table includes estimates that were not included in Tables 1.3, 1.4, and 1.5 .

Column (1): Coefficient estimates from Table 1.3, column (1).

Column (2): Coefficient estimates from Table 1.4, column (1).

Column (3): Coefficient estimates from Table 1.4, column (2).

Column (4): Coefficient estimates from Table 1.4, column (3).

Column (5): Coefficient estimates from Table 1.5, column (1).

Column (6): Coefficient estimates from Table 1.5, column (2).

Column (7): Coefficient estimates from Table 1.5, column (3)

VARIABLES	(1) birth	(2) birth	(3) birth	(4) birth	(5) birth	(6) birth	(7) birth
Starting year dummies							
start86	-1.03***	-1.84	-0.70	-0.52**	-0.35*	-1.05***	-0.83***
	(0.236)	(1.281)	(0.480)	(0.254)	(0.201)	(0.239)	(0.216)
start87	-1.53***	-3.58**	0.07	-0.34	-0.95***	-1.58***	-1.37***
	(0.242)	(1.539)	(0.323)	(0.258)	(0.224)	(0.246)	(0.224)
start88	-1.51***	-6.74***	0.13	-0.22	-0.82***	-1.56***	-1.32***
	(0.261)	(2.596)	(0.262)	(0.248)	(0.241)	(0.265)	(0.243)
start89	-1.42***	-0.34	0.43*	-0.55	-0.89***	-1.46***	-1.17***
	(0.262)	(2.160)	(0.253)	(0.366)	(0.257)	(0.266)	(0.242)
start90	-1.60***	-1.49	-0.12	-0.45*	-1.31***	-1.70***	-1.42***
	(0.257)	(1.206)	(0.272)	(0.263)	(0.271)	(0.262)	(0.241)
start91	-1.40***	-3.34**	0.25	-0.56**	-1.04***	-1.49***	-1.29***
	(0.248)	(1.702)	(0.262)	(0.273)	(0.261)	(0.253)	(0.233)
start92	-1.47***	-3.67**	-0.28	-0.48*	-1.09***	-1.56***	-1.25***
	(0.252)	(1.529)	(0.272)	(0.287)	(0.269)	(0.256)	(0.235)
start93	-1.62***	-2.53**	0.11	-0.61**	-1.14***	-1.72***	-1.42***
	(0.266)	(1.252)	(0.331)	(0.256)	(0.299)	(0.271)	(0.252)
start94	-1.79***	-3.65**	-0.05	-0.67**	-1.13***	-1.90***	-1.47***
	(0.285)	(1.593)	(0.294)	(0.270)	(0.322)	(0.290)	(0.267)
start95	-1.70***	-3.04**	-0.37	-0.69***	-1.18***	-1.82***	-1.47***
	(0.279)	(1.442)	(0.399)	(0.261)	(0.334)	(0.285)	(0.266)
start96	-1.90***	-4.84***	0.04	-0.77**	-1.71***	-2.00***	-1.77***
	(0.294)	(1.720)	(0.327)	(0.304)	(0.417)	(0.300)	(0.285)
start97	-2.24***	-4.21***	0.07	-0.64**	-0.89**	-2.35***	-1.89***
	(0.297)	(1.581)	(0.321)	(0.272)	(0.356)	(0.303)	(0.282)
start98	-2.73***	-5.31***	-0.09	-0.64**	-2.52***	-2.84***	-2.27***
	(0.340)	(1.733)	(0.377)	(0.280)	(0.673)	(0.347)	(0.322)
start99	-2.36***	-4.06***	0.33	-0.54*		-2.43***	-2.18***
	(0.322)	(1.466)	(0.442)	(0.316)		(0.322)	(0.327)
start2000	-2.76***	-5.20***	-0.28	-0.59*		-2.84***	-2.37***
	(0.380)	(1.723)	(0.444)	(0.313)		(0.380)	(0.363)
start2001	-2.70***	-4.61***	0.94**	-1.23***		-2.78***	-2.32***
	(0.406)	(1.648)	(0.422)	(0.397)		(0.406)	(0.388)
start2002	-2.54***	-3.71***	0.14	-0.49		-2.62***	-2.14***
	(0.433)	(1.425)	(0.494)	(0.379)		(0.433)	(0.419)
start2003	-3.28***	-5.04***	-0.15	-0.21		-3.37***	-2.77***
	(0.612)	(1.818)	(0.570)	(0.400)		(0.612)	(0.596)
			(0.014)				

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

This table includes estimates that were not included in Tables 1.3, 1.4, and 1.5 .

Column (1): Coefficient estimates from Table 1.3, column (1).

Column (2): Coefficient estimates from Table 1.4, column (1).

Column (3): Coefficient estimates from Table 1.4, column (2).

Column (4): Coefficient estimates from Table 1.4, column (3).

Column (5): Coefficient estimates from Table 1.5, column (1).

Column (6): Coefficient estimates from Table 1.5, column (2).

Column (7): Coefficient estimates from Table 1.5, column (3)

VARIABLES	(1) birth	(2) birth	(3) birth	(4) birth	(5) birth	(6) birth	(7) birth
Age in the initial period			0.00	-0.23***			
			(0.050)	(0.041)			
Age in the initial period squared			-0.00	0.00***			
			(0.001)	(0.001)			
Treatment1×Age in the initial period			-0.32***	0.48***			
			(0.078)	(0.188)			
Treatment1×Age in the initial period squared			0.00***	-0.01***			
			(0.002)	(0.003)			
Constant	0.48***	1.65**	-12.85	-0.54*	-0.21	0.53***	0.22
	(0.161)	(0.714)	(22.631)	(0.315)	(0.264)	(0.157)	(0.180)
Observations	13,766	2,064	2,886	6,616	7,020	13,766	12,713
Number of women	1,486	433	356	831	1,043	1,486	1,486
LR test of unobserved heterogeneity	172.3***	13.78***	9.68e-05	16.48***	36.45***	181.6***	125.7***

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

This table includes estimates that were not included in Tables 1.3, 1.4, and 1.5 .

Column (1): Coefficient estimates from Table 1.3, column (1).
Column (2): Coefficient estimates from Table 1.4, column (1).
Column (3): Coefficient estimates from Table 1.4, column (2).
Column (4): Coefficient estimates from Table 1.4, column (3).
Column (5): Coefficient estimates from Table 1.5, column (1).
Column (6): Coefficient estimates from Table 1.5, column (2).
Column (7): Coefficient estimates from Table 1.5, column (3)

Table A.2: Chapter1: All estimates from Tables 1.6

VARIABLES	(1)	(2)	(3)
Age	0.33***	0.15***	-0.02
	(0.017)	(0.054)	(0.060)
Age squared	-0.01***	-0.00**	0.00
	(0.001)	(0.002)	(0.002)
Age cubed	0.00***	0.00	-0.00
	(0.000)	(0.000)	(0.000)
Race			
White	0.15***	0.10***	0.09***
	(0.008)	(0.013)	(0.012)
Black	0.05***	0.12***	0.07***
	(0.014)	(0.023)	(0.022)
Housing tenure			
Owned	-2.45***	-1.35**	0.12
	(0.150)	(0.529)	(0.605)
Rented/own	-2.62***	-1.59***	-0.16
	(0.150)	(0.528)	(0.605)
Rent	-2.66***	-1.63***	-0.14
	(0.150)	(0.528)	(0.605)
Region of residence			
York	-0.02*	0.00	0.03***
	(0.009)	(0.014)	(0.013)
Northwest	-0.02**	0.01	0.02
	(0.008)	(0.013)	(0.012)
East Midlands	0.01	-0.02	0.04***
	(0.009)	(0.015)	(0.013)
West Midlands	-0.00	0.00	0.03***
	(0.009)	(0.014)	(0.013)
East Anglia	0.03***	0.01	0.02
	(0.012)	(0.020)	(0.017)
London	0.02**	-0.09***	-0.08***
	(0.008)	(0.014)	(0.013)
South East	0.03***	-0.02	0.00
	(0.007)	(0.012)	(0.010)
South West	0.02**	-0.03*	0.04***
	(0.009)	(0.015)	(0.013)
Wales	-0.05***	-0.03**	0.01
	(0.011)	(0.017)	(0.015)
Scotland	-0.01	0.01	0.02
	(0.008)	(0.013)	(0.012)
Observations	37,086	20,398	25,879
R-squared	0.833	0.631	0.638

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

This table includes estimates that were not included in Tables 1.6 .

Column (1): Coefficient estimates from Table 1.6, column (1).

Column (2): Coefficient estimates from Table 1.6, column (2).

Column (3): Coefficient estimates from Table 1.6, column (3).

Table A.3: Chapter2: All estimates from Tables 2.4, 2.5, and 2.6

VARIABLES	(1) hazard	(2) hazard	(3) hazard	(4) hazard	(5) hazard	(6) hazard	(7) hazard	(8) hazard	(9) hazard	(10) hazard
Period										
Period 2	0.03	-0.01	0.06	0.28**		0.11	0.06	0.10	0.08	0.12
	(0.10)	(0.10)	(0.14)	(0.13)		(0.14)	(0.14)	(0.14)	(0.16)	(0.10)
Period 3	0.23	-0.01	0.36*	0.94***		0.45**	0.33*	0.43**	0.32	0.39**
	(0.17)	(0.19)	(0.21)	(0.22)		(0.19)	(0.18)	(0.19)	(0.22)	(0.18)
Period 4	0.38	0.19	0.38	1.08***		0.51*	0.35	0.49*	0.31	0.42
	(0.24)	(0.24)	(0.31)	(0.32)		(0.28)	(0.27)	(0.28)	(0.29)	(0.31)
Period 5	0.43	0.01	0.55	1.71***		0.72**	0.50	0.69**	0.50	0.64**
	(0.30)	(0.29)	(0.39)	(0.43)		(0.33)	(0.31)	(0.33)	(0.35)	(0.30)
Period 6	0.44	0.22	0.18	1.37***		0.39	0.13	0.36	0.12	0.30
	(0.35)	(0.38)	(0.44)	(0.50)		(0.38)	(0.36)	(0.37)	(0.43)	(0.45)
Period 7	0.71*	0.31	0.70	1.52***		0.94**	0.65	0.90**	0.65	0.84
	(0.40)	(0.48)	(0.50)	(0.57)		(0.43)	(0.41)	(0.41)	(0.46)	(0.54)
Period 8	0.75	0.49	0.42	1.64**		0.69	0.37	0.67	0.39	0.62
	(0.48)	(0.49)	(0.61)	(0.66)		(0.54)	(0.53)	(0.50)	(0.52)	(0.60)
Period 9	0.76	0.27	0.57	2.15***		0.87	0.51	0.85	0.54	0.80
	(0.55)	(0.63)	(0.65)	(0.71)		(0.57)	(0.54)	(0.54)	(0.57)	(0.49)
Period 10	0.90	0.47	0.63	2.02**		0.94	0.56	0.92	0.47	0.74
	(0.58)	(0.59)	(0.80)	(0.85)		(0.71)	(0.66)	(0.68)	(0.61)	(0.63)
Period 11	0.84	0.62	-0.02	1.47*		0.33	-0.09	0.32	-0.04	0.27
	(0.68)	(0.69)	(0.86)	(0.83)		(0.79)	(0.74)	(0.75)	(0.71)	(0.70)
Period 12	0.91	0.71	0.09	1.79**		0.45	0.02	0.42	0.07	0.38
	(0.74)	(0.86)	(0.96)	(0.85)		(0.85)	(0.75)	(0.79)	(0.74)	(0.70)
Period 13	1.21	0.30	0.72	2.71***		1.11	0.64	1.09	0.54	0.89
	(0.73)	(0.88)	(0.89)	(0.90)		(0.75)	(0.69)	(0.70)	(0.73)	(0.74)
Period 14	-1.57**		-1.54			-1.13	-1.62**	-1.16	-1.63	-1.27
	(0.78)		(1.03)			(0.85)	(0.82)	(3.44)	(1.20)	(0.85)
Period 15	0.63		-0.22			0.19	-0.30	0.18	-0.28	0.10
	(0.87)		(1.14)			(1.00)	(3.17)	(0.96)	(0.97)	(1.09)
Period 17	-0.00									
	(0.94)									
Period 18										

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

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Column (4): Coefficient estimates from Table 2.4, column (4).
Column (5): Coefficient estimates from Table 2.5.
Column (6): Coefficient estimates from Table 2.6, column (1).
Column (7): Coefficient estimates from Table 2.6, column (2)
Column (8): Coefficient estimates from Table 2.6, column (3).
Column (9): Coefficient estimates from Table 2.6, column (4).
Column (10): Coefficient estimates from Table 2.6, column (5).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard
Education dummies										
Highest grade attended 6-8	3.46									
	(4.44)									
Highest grade attended 9-12	3.26									
	(4.40)									
Highest grade attended 13 or more	2.84									
	(4.41)									
Ethnicity dummies										
White	-0.16	0.05	-0.44***		-0.43***	-0.46***	-0.44***	-0.46***	-0.50**	-0.49***
	(0.11)	(0.13)	(0.14)		(0.14)	(0.14)	(0.14)	(0.14)	(0.19)	(0.13)
Age in the first year of observation	1.68***	1.45***	1.88***	2.26***	1.89***	1.94***	1.88***	1.92***	1.92***	1.87***
	(0.16)	(0.27)	(0.22)	(0.27)	(0.23)	(0.22)	(0.23)	(0.24)	(0.21)	(0.32)
Age in the first year of observation squared	-0.03***	-0.03***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***	-0.04***
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Region of Residence dummies										
Mid-atlantic	0.17	0.19	0.03	-0.23	0.04	0.01	-0.13	0.08	-0.15	-0.17
	(0.58)	(2.43)	(0.64)	(0.69)	(0.65)	(0.66)	(0.71)	(0.69)	(0.89)	(0.31)
Mid-west	0.20	0.17	-0.03	0.06	-0.05	-0.02	-0.07	0.02	0.00	-0.07
	(0.40)	(2.33)	(0.54)	(0.55)	(0.52)	(0.57)	(0.58)	(0.60)	(0.33)	(0.23)
South Atlantic	0.19	0.17	-0.01	-0.26	-0.02	-0.03	-0.07	0.01	-0.06	-0.10
	(0.48)	(2.37)	(0.60)	(0.60)	(0.57)	(0.50)	(0.68)	(0.61)	(0.57)	(0.24)
East South	0.34	0.34	0.13	0.06	0.10	0.08	0.02	0.16	0.08	0.03
	(0.61)	(2.28)	(0.71)	(0.84)	(0.71)	(0.71)	(0.80)	(0.74)	(0.76)	(0.31)
West South	0.29	0.32	0.03	0.35	-0.00	-0.12	-0.04	0.10	0.06	-0.03
	(0.52)	(2.32)	(0.61)	(0.65)	(0.65)	(0.66)	(0.70)	(0.68)	(0.81)	(0.27)
Mountain	0.60	0.38	0.49	0.77	0.46	0.52	0.37	0.55	0.49	0.38
	(0.60)	(2.38)	(0.73)	(0.70)	(0.68)	(0.72)	(0.75)	(0.77)	(0.61)	(0.33)
Pacific	0.14	-0.15	0.12	0.06	0.11	-0.08	-0.13	0.17	0.14	-0.03
	(0.76)	(2.30)	(0.69)	(0.76)	(0.68)	(0.82)	(0.86)	(0.77)	(0.94)	(0.32)
State-level Economics Indicators										
Median annual income	-0.00***	-0.00*	-0.00	-0.00	-0.00	-0.00	-0.00**	-0.00	-0.00	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Top 10 percentile annual income	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female labor force participation rate	0.62	2.24	0.26	-2.92	0.36	-0.30	0.29	0.08	0.21	0.78
	(2.73)	(2.74)	(2.99)	(3.80)	(2.90)	(3.01)	(3.19)	(2.96)	(3.10)	(1.67)
Female unemployment rate	3.82	3.87	5.77**	11.33***	6.01**	6.21**	6.14***	5.53**	5.84*	6.19**
	(2.47)	(3.39)	(2.50)	(3.46)	(2.56)	(2.48)	(2.34)	(2.35)	(3.07)	(2.73)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

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Column (4): Coefficient estimates from Table 2.4, column (4).
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Column (6): Coefficient estimates from Table 2.6, column (1).
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Column (8): Coefficient estimates from Table 2.6, column (3).
Column (9): Coefficient estimates from Table 2.6, column (4).
Column (10): Coefficient estimates from Table 2.6, column (5).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard
Starting year dummies										
start81	0.49***	0.57***	0.42**	0.03	0.44***	0.43***	0.45***	0.45***	0.63*	0.49**
	(0.15)	(0.19)	(0.16)	(0.24)	(0.14)	(0.16)	(0.15)	(0.15)	(0.33)	(0.22)
start82	0.68***	0.64***	0.65***	0.89***	0.62***	0.71***	0.65***	0.72***	0.37	0.68**
	(0.17)	(0.21)	(0.19)	(0.29)	(0.17)	(0.15)	(0.16)	(0.17)	(0.32)	(0.27)
start83	0.65***	0.57**	0.61***	0.80**	0.57***	0.67***	0.59***	0.65***	0.30	0.59**
	(0.24)	(0.25)	(0.23)	(0.36)	(0.18)	(0.20)	(0.19)	(0.16)	(0.30)	(0.30)
start84	0.72***	0.85***	0.61**	0.40	0.55**	0.69***	0.59**	0.69***	0.33	0.56*
	(0.26)	(0.28)	(0.26)	(0.41)	(0.24)	(0.23)	(0.24)	(0.24)	(0.30)	(0.30)
start85	0.39	0.17	0.67*	0.62	0.56**	0.73***	0.66**	0.72***	-0.52	0.80***
	(0.30)	(0.34)	(0.37)	(0.39)	(0.26)	(0.28)	(0.28)	(0.25)	(0.40)	(0.29)
start86	0.66*	0.25	1.15***	1.58***	1.04***	1.28***	1.17***	1.27***	-0.22	1.09***
	(0.35)	(0.39)	(0.38)	(0.48)	(0.24)	(0.28)	(0.28)	(0.27)	(0.35)	(0.32)
start87	0.49	0.52	0.31	0.19	0.24	0.38	0.33	0.41	0.34	0.40
	(0.41)	(0.47)	(0.49)	(0.53)	(0.34)	(0.37)	(0.35)	(0.35)	(0.38)	(0.31)
start88	0.65	0.65	0.65	0.17	0.57	0.77*	0.68	0.80*	-0.00	0.63*
	(0.48)	(0.47)	(0.54)	(0.51)	(0.37)	(0.43)	(0.44)	(0.41)	(0.42)	(0.33)
start89	0.69	0.90	0.49	0.54	0.47	0.61	0.52	0.62	0.12	0.56*
	(0.48)	(0.57)	(0.60)	(0.58)	(0.36)	(0.44)	(0.43)	(0.40)	(0.47)	(0.31)
start90	1.38**	1.40**	0.96	1.16**	0.95**	1.13**	0.95*	1.16**	0.09	0.98***
	(0.60)	(0.62)	(0.66)	(0.58)	(0.44)	(0.49)	(0.50)	(0.48)	(0.52)	(0.34)
start91	0.83	1.18*	0.58	0.25	0.63	0.71	0.59	0.70	-0.25	0.55
	(0.58)	(0.61)	(0.66)	(0.68)	(0.44)	(0.49)	(0.45)	(0.46)	(0.57)	(0.35)
start92	1.15*	1.03	1.00	1.15	1.04**	1.19**	1.03**	1.24**	-0.31	1.07***
	(0.64)	(0.65)	(0.74)	(0.73)	(0.44)	(0.54)	(0.52)	(0.48)	(0.60)	(0.38)
start93	0.88	1.42**	0.59	0.28	0.69	0.75	0.62	0.79	-0.12	0.61*
	(0.71)	(0.72)	(0.81)	(0.78)	(0.48)	(0.58)	(0.58)	(0.52)	(0.64)	(0.34)
start94	0.52	0.90	-0.05	-0.09	0.09	0.08	-0.01	0.12	-0.70	-0.03
	(0.81)	(0.79)	(0.87)	(0.79)	(0.55)	(0.62)	(0.62)	(0.56)	(0.72)	(0.39)
start95	0.85	1.18	0.51	0.37	0.63	0.70	0.56	0.75	-0.16	0.55
	(0.80)	(0.78)	(0.99)	(0.94)	(0.59)	(0.74)	(0.74)	(0.67)	(0.75)	(0.42)
start96	0.52	0.53	0.27	0.45	0.42	0.44	0.33	0.43		0.26
	(0.86)	(0.85)	(1.03)	(0.93)	(0.57)	(0.75)	(0.69)	(0.67)		(0.48)
start97	0.22	0.59	-0.37	-1.23						
	(0.95)	(0.87)	(1.08)	(3.58)						

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

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Column (8): Coefficient estimates from Table 2.6, column (3).
Column (9): Coefficient estimates from Table 2.6, column (4).
Column (10): Coefficient estimates from Table 2.6, column (5).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard	hazard
Year fixed effects										
year1981	-0.10	-0.03	-0.31*	-0.13	-0.30*	-0.30**	-0.30*	-0.30*	-0.30	-0.31*
	(0.13)	(0.18)	(0.17)	(0.18)	(0.18)	(0.14)	(0.16)	(0.17)	(0.24)	(0.17)
year1982	-0.16	-0.10	-0.40*	-0.81***	-0.37*	-0.41**	-0.39**	-0.39*	-0.45*	-0.45***
	(0.16)	(0.18)	(0.21)	(0.24)	(0.22)	(0.19)	(0.19)	(0.21)	(0.26)	(0.14)
year1983	-0.01	0.16	-0.37*	-0.74***	-0.37*	-0.39**	-0.37*	-0.37*	-0.36	-0.36*
	(0.16)	(0.18)	(0.21)	(0.24)	(0.20)	(0.19)	(0.19)	(0.21)	(0.27)	(0.21)
year1984	-0.10	0.16	-0.56**	-0.89***	-0.54**	-0.57***	-0.54**	-0.57**	-0.50*	-0.52**
	(0.16)	(0.18)	(0.25)	(0.24)	(0.24)	(0.21)	(0.23)	(0.26)	(0.27)	(0.21)
year1985	-0.04	0.26	-0.54***	-1.03***	-0.52**	-0.56***	-0.52***	-0.56***	-0.45	-0.47*
	(0.18)	(0.25)	(0.21)	(0.32)	(0.23)	(0.20)	(0.20)	(0.20)	(0.29)	(0.25)
year1986	0.28*	0.55***	-0.39	-0.72*	-0.19	-0.41*	-0.36	-0.41*	-0.60**	-0.63
	(0.16)	(0.16)	(0.25)	(0.40)	(0.27)	(0.22)	(0.25)	(0.24)	(0.28)	(0.41)
year1987	0.35***	0.63***	-0.24	-0.42**	-0.11	-0.26*	-0.22	-0.27	-0.22	-0.26
	(0.12)	(0.15)	(0.16)	(0.17)	(0.16)	(0.14)	(0.17)	(0.17)	(0.26)	(0.26)
year1988	0.46***	0.68***	0.07	-0.29	0.23	0.05	0.10	0.03	0.10	0.06
	(0.08)	(0.15)	(0.15)	(0.28)	(0.16)	(0.16)	(0.16)	(0.15)	(0.24)	(0.29)
year1989	0.48***	0.58***	0.24*	0.04	0.40***	0.23*	0.23*	0.18	0.21	0.18
	(0.07)	(0.21)	(0.13)	(0.14)	(0.14)	(0.13)	(0.13)	(0.14)	(0.23)	(0.21)
year1991	0.32***	0.37**	0.15	-0.06	0.21	0.14	0.14	0.10	0.10	0.06
	(0.09)	(0.17)	(0.16)	(0.19)	(0.17)	(0.16)	(0.16)	(0.17)	(0.23)	(0.24)
year1992	0.35***	0.32**	0.01	-0.28*	0.11	-0.01	0.02	-0.06	-0.07	-0.12
	(0.12)	(0.14)	(0.19)	(0.17)	(0.20)	(0.20)	(0.19)	(0.19)	(0.24)	(0.23)
year1993	0.18*	0.11	0.08	-0.38*	0.23*	0.06	0.10	0.03	0.07	0.03
	(0.10)	(0.20)	(0.14)	(0.20)	(0.14)	(0.14)	(0.14)	(0.15)	(0.22)	(0.19)
year1994	0.39***	0.29	0.34***	0.22*	0.37***	0.33***	0.34***	0.32***	0.31	0.29*
	(0.10)	(0.21)	(0.11)	(0.11)	(0.12)	(0.11)	(0.11)	(0.11)	(0.20)	(0.15)
year1995	0.01	0.25	-0.40***	-0.51***	-0.38**	-0.39**	-0.40***	-0.41***	-0.47*	-0.48**
	(0.10)	(0.21)	(0.15)	(0.15)	(0.16)	(0.15)	(0.16)	(0.16)	(0.25)	(0.19)
year1996	0.19*	0.11	0.21	-0.03	0.22	0.21	0.20	0.20	0.13	0.13
	(0.11)	(0.23)	(0.15)	(0.16)	(0.16)	(0.13)	(0.15)	(0.15)	(0.21)	(0.25)
Constant	-26.56***	-20.81***	-25.86***	-30.67***	-25.91***	-26.35***	-25.77***	-26.25***	-25.84***	-26.15***
	(4.59)	(3.69)	(2.78)	(4.01)	(3.24)	(2.94)	(3.05)	(3.21)	(0.93)	(3.60)
LR test of gam	12.40***	1.60	2.276*	15.30***	1.713*	3.60**	1.53	3.55**	1.34	0.59
Observations	10829	4794	4925	4925	4925	4925	4925	4925	4635	4635

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.4, 2.5, and 2.6 .

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Column (4): Coefficient estimates from Table 2.4, column (4).
Column (5): Coefficient estimates from Table 2.5.
Column (6): Coefficient estimates from Table 2.6, column (1).
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Column (8): Coefficient estimates from Table 2.6, column (3).
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Column (10): Coefficient estimates from Table 2.6, column (5).

Table A.4: Chapter2: All estimates from Tables 2.7 and 2.8

VARIABLES	(1) hazard	(2) hazard	(3) hazard	(4) hazard	(5) hazard
Period					
Period 2	-0.02	-0.05	-0.04	0.09	
	(0.17)	(0.17)	(0.17)	(0.16)	
Period 3	0.43*	0.37	0.40*	0.66***	
	(0.23)	(0.23)	(0.23)	(0.21)	
Period 4	0.43	0.34	0.38	0.77***	
	(0.32)	(0.32)	(0.32)	(0.28)	
Period 5	0.18	0.06	0.12	0.62*	
	(0.41)	(0.41)	(0.41)	(0.35)	
Period 6	0.52	0.38	0.45	1.07***	
	(0.48)	(0.47)	(0.48)	(0.40)	
Period 7	0.44	0.27	0.35	1.08**	
	(0.56)	(0.56)	(0.57)	(0.48)	
Period 8	0.35	0.15	0.24	1.08**	
	(0.65)	(0.64)	(0.65)	(0.55)	
Period 9	0.00	-0.21	-0.12	0.86	
	(0.76)	(0.75)	(0.76)	(0.64)	
Period 10	0.40	0.16	0.26	1.34**	
	(0.82)	(0.81)	(0.82)	(0.68)	
Period 11	-1.93	-2.19*	-2.10	-0.89	
	(1.31)	(1.31)	(1.32)	(1.21)	
Period 12	-0.67	-0.94	-0.84	0.45	
	(1.07)	(1.06)	(1.08)	(0.93)	
Period 13	-1.64	-1.92	-1.83	-0.45	
	(1.38)	(1.37)	(1.39)	(1.27)	
Period 14	-1.52	-1.81	-1.71	-0.28	
	(1.41)	(1.40)	(1.42)	(1.29)	
Period 15	-1.35	-1.67	-1.55	-0.06	
	(1.44)	(1.43)	(1.44)	(1.31)	

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

Column (1): Coefficient estimates from Table 2.7 ,column (1).
Column (2): Coefficient estimates from Table 2.7 ,column (2).
Column (3): Coefficient estimates from Table 2.7 ,column (3).
Column (4): Coefficient estimates from Table 2.7, column (4).
Column (5): Coefficient estimates from Table 2.8.

VARIABLES	(1) hazard	(2) hazard	(3) hazard	(4) hazard	(5) hazard
Ethnicity dummies					
White	-0.39**	-0.37**	-0.38**	-0.53***	-0.99***
	(0.17)	(0.17)	(0.17)	(0.18)	(0.261)
Age in the first year of observation	2.41***	2.32***	2.37***	2.68***	
	(0.48)	(0.48)	(0.48)	(0.48)	
Age in the first year of observation squared	-0.05***	-0.05***	-0.05***	-0.05***	
	(0.01)	(0.01)	(0.01)	(0.01)	
Region of Residence dummies					
Mid-atlantic	0.10	0.12	-0.05	0.09	-0.19
	(0.34)	(0.33)	(0.38)	(0.38)	(0.935)
Mid-west	0.20	0.18	0.14	0.25	0.28
	(0.31)	(0.29)	(0.31)	(0.34)	(0.822)
South Atlantic	-0.16	-0.17	-0.24	-0.15	0.32
	(0.32)	(0.31)	(0.33)	(0.36)	(0.874)
East South	0.54	0.56	0.46	0.55	0.90
	(0.41)	(0.39)	(0.41)	(0.45)	(1.027)
West South	0.63*	0.74**	0.60*	0.69*	1.11
	(0.35)	(0.35)	(0.35)	(0.39)	(0.996)
Mountain	0.76*	0.74*	0.67	0.87*	0.60
	(0.42)	(0.41)	(0.43)	(0.46)	(0.873)
Pacific	0.25	0.42	0.04	0.37	0.55
	(0.32)	(0.35)	(0.40)	(0.36)	(1.019)
State-level Economics Indicators					
Median annual income	-0.00	-0.00*	-0.00	-0.00*	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.000)
Top 10 percentile annual income	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.000)
Female labor force participation rate	2.24	2.80	2.25	2.61	0.03
	(2.10)	(2.10)	(2.09)	(2.29)	(2.211)
Female unemployment rate	1.35	1.06	1.54	0.84	4.51
	(2.65)	(2.63)	(2.65)	(2.75)	(3.413)

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

Column (1): Coefficient estimates from Table 2.7 ,column (1).
Column (2): Coefficient estimates from Table 2.7 ,column (2).
Column (3): Coefficient estimates from Table 2.7 ,column (3).
Column (4): Coefficient estimates from Table 2.7, column (4).
Column (5): Coefficient estimates from Table 2.8.

VARIABLES	(1)	(2)	(3)	(4)		(5)
	hazard	hazard	hazard	hazard		hazard
Starting year dummies						
start71	0.60*	0.59*	0.59*	0.65*	start81	-0.56**
	(0.32)	(0.30)	(0.31)	(0.37)		(0.252)
start72	0.32	0.31	0.31	0.37	start82	-0.16
	(0.30)	(0.29)	(0.30)	(0.35)		(0.353)
start73	0.24	0.23	0.23	0.25	start83	-0.31
	(0.28)	(0.27)	(0.28)	(0.33)		(0.411)
start74	0.22	0.21	0.20	0.27	start84	-0.47
	(0.29)	(0.28)	(0.29)	(0.33)		(0.317)
start75	-0.67*	-0.67*	-0.68*	-0.63	start85	-0.72*
	(0.39)	(0.38)	(0.38)	(0.43)		(0.426)
start76	-0.45	-0.48	-0.47	-0.32	start86	0.09
	(0.36)	(0.35)	(0.36)	(0.38)		(0.380)
start77	-0.01	-0.04	-0.05	0.21	start87	-0.90**
	(0.40)	(0.39)	(0.40)	(0.41)		(0.389)
start78	-0.43	-0.49	-0.48	-0.13	start88	-1.04**
	(0.45)	(0.44)	(0.45)	(0.44)		(0.465)
start79	-0.33	-0.41	-0.40	0.02	start89	-0.50
	(0.50)	(0.49)	(0.50)	(0.48)		(0.475)
start80	-0.34	-0.43	-0.41	0.03	start90	-0.87*
	(0.54)	(0.53)	(0.54)	(0.51)		(0.476)
start81	-0.75	-0.79	-0.83	-0.39	start91	-0.15
	(0.59)	(0.58)	(0.59)	(0.57)		(0.536)
start82	-0.76	-0.82	-0.85	-0.35	start92_94	-0.24
	(0.62)	(0.61)	(0.62)	(0.59)		(0.590)
start83	-0.57	-0.65	-0.67	-0.12	start95_97	-0.46
	(0.67)	(0.65)	(0.67)	(0.63)		(0.709)
start84	-1.23*	-1.30*	-1.34*	-0.73		
	(0.74)	(0.72)	(0.74)	(0.69)		
start85	-0.67	-0.78	-0.78	-0.15		
	(0.77)	(0.75)	(0.77)	(0.72)		

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

Column (1): Coefficient estimates from Table 2.7 ,column (1).
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Column (4): Coefficient estimates from Table 2.7, column (4).
Column (5): Coefficient estimates from Table 2.8.

VARIABLES	(1)	(2)	(3)	(4)	(5)
	hazard	hazard	hazard	hazard	hazard
Year fixed effects					
year1981					-0.01
					(0.344)
year1982					0.29
					(0.358)
year1983					-0.10
					(0.472)
year1984					-0.17
					(0.416)
year1985					-0.05
					(0.423)
year1986					0.11
					(0.371)
year1987					0.04
					(0.260)
year1988					0.30
					(0.309)
year1989					0.49*
					(0.251)
year1990					0.39
					(0.280)
year1991					0.16
					(0.231)
year1992					0.30
					(0.185)
year1993					0.54***
					(0.185)
year1994					-0.18
					(0.166)
year1995					0.48***
					(0.155)
Constant	-32.21***	-31.30***	-31.69***	-35.38***	-3.17*
	(5.87)	(5.90)	(5.89)	(5.95)	(1.703)
LR test of gamma	47.01***	46.18***	46.51***	55.68***	10.82***
Observations	4257	4257	4257	4257	4125

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Standard errors in parentheses.

Notes:

-This table includes estimates that were not included in Tables 2.7 and 2.8 .

Column (1): Coefficient estimates from Table 2.7 ,column (1).
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