Causal effects on employment after first birth - A dynamic treatment approach

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Abstract: The effects of employment breaks on labor market outcomes are of great relevance in particular for females in relation with childbirth. In this paper we estimate the effect of the timing of women's first birth on later labor market outcomes, more precisely the effect on employment. In order to investigate the treatment effect of having the first childbirth now or waiting, we employ the dynamic treatment approach in the spirit of Sianesi (2004, 2008). We combine this approach with the inverse probability weighting (IPW) as in Busso et al. (2009) for better performance than matching with respect to bias and variance in finite samples with good overlap. Finally, we assess effect heterogeneity by estimating ex post outcome regressions as in Abadie, Imbens (2006). We implement this novel approach on a monthly basis by using the German SOEP data set from 1991 to 2008. The results show that there are very strong employment effects around childbirth which decline over time, but remain significantly negative over the whole considered period of five years after childbirth. Further, the treatment effect patterns display only some modest heterogeneity across different age groups. Finally, we find that the facilitation of part-time work as introduced in 2001 did increase maternal employment rates to a significant extent.

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1 Introduction

Childbirth has often been found to largely reduce female labor supply which in turn has adverse effects on later labor market outcomes. Due to their close correlation, the important decisions about career objectives and whether and when to start a family have to be considered jointly which usually occurs between the ages of 20 and 40 years. This joint nature of fertility and employment decisions is also at the heart of current policy reforms which aim at reducing negative effects of child-related employment breaks which have been found particularly for female wage growth and careers (see Ejrnaes and Kunze, 2004; Beblo et al., 2009; Kunze, 2002; Budig and England, 2001). As the opportunity costs of childbirth vary with acquired human capital, e.g. higher work experience or education (Görlich and de Grip, 2007), the timing of first birth has an important impact on future career outcomes (Herr, 2007). Put differently, in a certain phase of their lives couples can decide on whether to have a child now or wait – where the expected negative career effects vary with age. This raises two research questions which are analyzed in this study: 1) Does the *timing* of first birth have an impact on post-birth employment? 2) Is this impact in line with human capital theory, i.e. is the negative treatment effect weaker for women with more human capital?

To address these questions it is important to recognize that post-birth employment behavior affects future employment chances because longer breaks and lower working hours reduce the amount of human capital (Lefebvre et al., 2009). This in turn is of highest relevance in the current debate on poverty among families and single parents. Clearly, the employment behavior in the first years after childbirth depends on the private environment (for instance the partner's income), legislation (for instance daycare availabilities, see Kreyenfeld and Hank, 2000 and van Ham and Büchel, 2004) but also on pre-birth employment and the individual education level. Thus, post-birth employment is in turn part of the fertility decision in as far as preferences for employment after childbirth are already known before birth.

In general, motherhood can have effects on various labor market outcomes, such as wages, labor force participation, tenure, and working hours. There are several studies which analyze the wage effects of childbirth. One of the first is by Taniguchi (1999) for the U.S. who finds that the wage penalty of childbirth is lower for higher educated mothers. However, it remains unclear whether the reasons have to do with labor force attachment or family friendly jobs. This question is addressed explicitly by Beblo et al. (2009) who match individuals within identical firms. They find that the wage effect of entering motherhood in Germany of about 16-19% is driven by selection of mothers-to-be into firms and jobs with a flatter wage profile. This could be in line with a small wage dip which is found already before birth by Ejrnaes and Kunze (2004) also for Germany. They also find that effects vary for different educational groups and work experience levels. Another study for Germany by Buligescu et al. (2009) finds effects of a slightly lower magnitude (10-14%), but they stress the dynamics as the child grows. More precisely, they find that wages have caught up five years after first birth. In a very interesting paper for the U.S., Herr (2007) uses miscarriages and contraceptive failures for the identification of the effects of first childbirth. Her analysis of wage effects shows that it does "pay to delay", because delaying first childbirth by one year raises wage growth after 15 years by 3-5%. This effect is driven by labor force participation and working hours. We will therefore focus in this study on effects on labor force participation. Troske and Voicu (2009), studying the simultaneity of the fertility and employment decision, find for the U.S. that delaying childbirth leads to higher levels of labor market involvement before first childbirth and reduces the negative effect on employment thereafter. This effect abates as the child grows as is also found by Sommerfeld (2009) for Germany. This as well as other studies find that German mothers reduce their labor supply dramatically in the context of childbirth, although maternal labor supply increased over the past decades in Germany as in most other industrial countries.[1] Geyer and Steiner (2007), for example, find that mothers' employment rate drops by almost 60 percentage points in the year of giving birth. This appears huge in comparison to 30 percentage points in the UK, 16 percentage points in Denmark and six percentage points in Italy (ibid. p. 17). As one reason for this dissimilarity, the generosity of the German maternity leave policy has repeatedly been cited (Sonderhof, 2007; Schönberg and Ludsteck, 2007; Beblo et al., 2009).

On the one hand, women with higher human capital or higher labor force attachment have the most to loose from an employment break in terms of human capital and thus career opportunities. From this we would expect older and higher educated first-time mothers to catch up more quickly to the control group than younger or less educated first-time mothers. On the other hand, for older first-time mothers who have potentially established a career already the control group also consists of highly career-oriented women which could amplify the treatment effect. Thus the age effect of first childbirth on employment is an empirical question.

Answering these questions empirically necessitates solutions with regard

[1] See for example Jaumotte (2003); Gornick et al. (1998); Geyer and Steiner (2007) and for data on Germany: Statistisches Bundesamt (1999).

to the endogeneity of fertility and employment decisions. We estimate the average treatment effects on the treated (ATTs) in the framework of a timing-of-events approach which is usually employed in the evaluation of active labor market policies (Sianesi, 2004, 2008; Fitzenberger et al., 2010). In our application individuals can decide whether to have a child now or wait. After conditioning on labor market history and personal circumstances and values, the exact timing of birth is random which is the basis of our identification strategy. Conditioning on future birth would result in a highly selective control group and for this reason we instead allow control individuals to have a child later. We align treatment and control group very precisely by age in months which permits us to estimate a monthly treatment effect. This is done by inverse probability weighting (IPW) based on the estimated propensity to have a child now. Inverse proabibility weighting is preferred on bias and variance grounds in finite samples with very good overlap (cf. Busso et al., 2009) if the weights are normalized to sum to one (in contrast to Frölich, 2004). Finally, in order to allow for effect heterogeneity, we employ ex-post outcome regressions in the spirit of Abadie and Imbens (2006) to analyze heterogeneity with respect to educational levels in more detail.

For the analysis we make use of the German SOEP data from 1991 to 2008 as it provides not only detailed monthly information on employment, but also a large variety of control variables. In order to avoid selectivity of the control group, we limit our analysis to women who bear their first child between the ages of 22 and 33. Our results confirm the sharp employment drop at childbirth. The average treatment effects on the treated slowly approach zero, implying that employment rates recover as the child grows, but we also show that within the observation period of five years employment rates of treated and controls do not reach equality. Although the employment effects differ depending on the age at first childbirth the trends are not ordered by age. This is because older mothers display higher employment rates, but the same holds for the corresponding control group. As expected from human capital theory first-time mothers with a university degree display lower treatment effects, but this is significant only from age 30 onwards.

The paper proceeds as follows. First, the German institutional background is described in section 2. The following section explains the econometric approach in some detail, because we combine the dynamic treatment approach (section 3.1) with inverse probability weighting (3.2) and ex post outcome regressions (3.3). The data preparation has to correspond to this novel approach and is described in section 4. Then, our first results are discussed before concluding with some final remarks in section 6.

2 Institutional Background

German maternity institutions are relatively generous in an international comparison. For instance, it is possible to take maternity leave for up to three years which is one of the longest leave periods in western countries. In the economic literature it is often discussed that this long period is one of the reasons for the low labor market participation of German mothers (see e.g. Beblo et al., 2009; Sonderhof, 2007; Schönberg and Ludsteck, 2007; Ondrich et al., 1999). In order to give incentives for shorter realized maternity leave breaks, in 2007 the German government introduced the so called parenting benefit (Elterngeld). It allows parents to receive 67 percent of their pre-birth net labor income (at maximum 1,800 Euro per month) for up to 14 months.[2] Because our observation period covers the time period before parenting benefit was introduced we give a short overview about the institutions which existed during the considered period.

In Germany there are two different laws regulating when mothers have to or are allowed to take maternity leave breaks.[3] Both laws relate to job protection and regulate financial benefits, but apply at different time periods. First the "Maternity Protection Law" (Mutterschutzgesetz) requests women to take a break from employment six weeks before and eight weeks after birth in order to protect her and the baby's life.[4] During this period the mother receives her full net labor income which is financed partly by the employer and partly by the health insurance.

Second, the "Parental Leave Law" (Elternzeitgesetz) regulates the job protection period and financial compensation during the parental leave period.[5] Job protection guarantees the parents to return to a *comparable* job at the previous employer, which need not be the old workplace. The job protection period is three years of which up to twelve months may be de-

[2]The income basis is the average of the last 12 months before birth. If only one parent takes maternity leave parenting benefit is paid for 12 months and if each parent takes at least 2 months maternity leave parenting benefit is paid for 14 months.

[3]A precise summary of the German regulation can be found in Kreyenfeld (2001) and Schönberg and Ludsteck (2007). The effects of the prolonged maternity leave period on the child are analyzed by Dustmann and Schönberg (2008).

[4]The post-birth break is obligatory whereas for the pre-birth period a women can apply for an exemption. In the case of giving birth to twins the after-birth period is extended to twelve weeks.

[5]Parents can partition maternity leave as they want. Since after-birth maternity protection is accumulated, the maximum leave period for fathers is three years minus maternity protection after birth.

	1. child	2. child	3. child	4. child and more
1992 - 1995	35.79	66.47	112.48	122.71
1996	102.26	102.26	153.39	178.95
1997 - 1998	112.48	112.48	153.39	178.95
1999	127.82	127.82	153.39	178.95
2000 - 2001	138.05	138.05	153.39	178.95
2002 - 2008	154.00	154.00	154.00	179.00

Table 1: Development of child allowance in Euro

layed until the child reaches the age of eight.[6] According to the "Parental Leave Law", parental benefits are paid if the parent on leave does not work more than the allowed amount of hours, lives in the same household and cares predominantly by him- or herself for the child. Before 2001 a parent on parental leave was allowed to work 19 hours a week at maximum. In January 2001 this upper bound was extended to 30 hours. Until January 2007, if eligible the parent on leave had the choice between receiving 300 Euro per month during the first 24 months or 450 Euro per month during the first 12 months. This maternity benefit was means-tested, i.e. it was paid to families with an annual net income less than 30,000 Euro in two-parent households and 23,000 Euro in single-parent households. Six months after birth these boundaries decreased to 16,500 (13,500) Euro plus 3,140 Euro for each additional child. In addition to maternity benefits, families receive a monthly child allowance (Kindergeld) for dependent children of nowadays 184 Euro for the first and second child, 190 Euro for the third child and 215 Euro for the fourth and all other children. Note that this child benefit is paid until the children have completed their education. Table 1 shows the development of the child allowance during the observation period. Instead of the child allowance families can choose a tax exemption for dependent children which is more attractive for high income families.

Mothers who are working in the labor market are in need of formal or informal daycare for their children. In Germany the availability of formal daycare for children younger than three years was extended in the last years but still there is a lack of facilities. On average around 14 percent of all children in this age group attend formal or institutional daycare.[7] From age three on every child in Germany has a legal claim for a place in formal

[6]Some companies voluntarily extend the job protection period to four years.

[7]Due to historical reasons the daycare coverage is 37 percent in East and only 8 percent in West Germany in 2007 (see Mühler, 2008).

daycare and the attendance rates increases to 89 percent in this age group of children.[8] However, in existing daycare facilities the opening hours are often not sufficient (Boll, 2009) and attendance is often on a part-time basis. Apart from the Parental Leave legislation explained above, this lack of suitable childcare facilities is another reason which leads a large fraction of mothers to leave the labor market for three years or more.

3 Econometric Approach

Our goal is to estimate the average treatment effect for the treated (ATT) of the treatment 'first childbirth at a certain age' on employment. We propose estimating the treatment parameter of childbirth now versus waiting based on discrete time data, as suggested by Sianesi (2004, 2008) in the context of estimating the effects of active labor market programs. We assume a dynamic conditional independence assumption and we apply inverse probability weighting (IPW) based on the estimated propensity score by age (Hirano et al., 2003). As suggested by Busso et al. (2009), we implement the IPW estimator for the ATT by normalizing the weights of the members in the comparison group such that the weights sum up to one for each treated individual in the estimation of the counterfactual nontreatment outcome. We summarize the ATT estimates and we analyze effect heterogeneity using weighted outcome regressions based on IPW analogous to the approach suggested by Abadie and Imbens (2006) for matching estimators. The next subsection introduces and motivates the treatment parameter to be estimated. Then, we describe our implementation of the IPW estimator. Finally, we introduce the weighted outcome regression based on IPW.

3.1 Childbirth now versus waiting

The treatment effect we estimate is the parameter of the effect of treatment versus waiting as introduced by Sianesi (2004, 2008). Thus, for a certain age, we estimate the ATT of having a first child at this age versus not having a child at this age. The group of nontreated women consists of women who do not have a first birth at this age and who may or may not have a first child in the future. Treated and nontreated women have not had a child before the considered age and therefore the group of nontreated women shrinks with

[8] For more information on German daycare provision see Mühler (2008).

rising age because the treated women are excluded as soon as they give birth to their first child. The treatment effect we estimate can be viewed as an example for the timing-of-events approach to treatment effects estimation as applied in the context of program evaluation to assess active labor market policy by Sianesi (2004, 2008) for Sweden or by Fitzenberger et al. (2010) for Germany. These countries have a comprehensive system of active labor market policies implying that with some probability unemployed individuals who have not been treated by a certain point of time may receive treatment later. If one constructs a control group based on nontreated individuals during the observation window (e.g. by matching on observable characteristics), one runs the likely risk of conditioning on future outcomes because individuals do not participate in treatment in the future because they find a job earlier than having started a later treatment. Put differently, individuals eligible for treatment at a certain point of time but only receiving treatment later are excluded from the control groups. These effects may introduce a bias in static treatment effects estimates based on treatment status observed during a certain observation period as discussed by Sianesi (2004, 2008). This view is rationalized in the context of a competing risks model where exits from unemployment to employment and entry into program participation can be view as two competing risks.

In our application, for women giving first birth at a certain age, we do not exclude the alternative of giving first birth at a later age. Using solely a control group of women who do not give first birth until a much later age or who never have a child would bias the control group towards women with a low propensity of having a child. This bias is likely to be correlated with labor market outcomes (e.g. women with a strong unobserved career orientation are more likely to have a higher labor market attachment and lower fertility rates). Strictly speaking, this problem cannot be cast into a competing risk framework. However, after the age of first birth considered, there may be labor market shocks and shocks to personal circumstances (relationships, household characteristics) with differential effects on fertility and labor market outcomes. It is not too far fetched to consider the possibility that positive labor market shocks are negatively correlated with future fertility or that personal shocks which reduce the fertility in the future are also correlated with labor market outcomes.[9] For the purpose of our analysis, these effects are similar in nature to the competing risks approach prevalent in the timing-of-events analysis of active labor market policy. Therefore, a likely bias is implied if we exclude women who give first birth later than the

[9]An extreme example may be a work accident of a women which prevents her both from work and from having a child in the future.

age considered. This argument assumes that we cannot control unobserved heterogeneity of the treated women if we only consider nontreated women who do not give birth in the future (during an observation period). Instead, we follow Sianesi (2004, 2008) and assume that women giving birth to their first child are comparable before the gestation lag, i.e. at the time before they get pregnant, to women who have not given birth until some time after the age considered. This approach allows for the anticipation of birth during the gestation period but assumes that women do not know the exact age of first birth before the gestation period (although women may know the probability of having a first birth now versus later and they may act upon the determinants of this probability). Assuming a no-anticipation condition with respect to the precise date of pregnancy before the gestation period allows us to match treated and nontreated women at this date.

Specifically, the treatment group in our analysis consists of women between the ages of 22 and 33 who have their first child and who are not retired or disabled. The control group consists of women who have not had a child at the precise age of the treated mother at the time of birth.[10] Hence, we assign to each treated woman an individual-specific control group imposing an exact alignment of the age in months. We measure both the treatment effect and the age of women at a monthly frequency.[11] For example, there should be little difference between a women who has her first child at age 25 years and 12 months or at 26 years and 1 month. Our analysis estimates for each treated women an individual-specific, average counterfactual outcome based on the control group of nontreated individuals so far.

3.2 Inverse probability weighting

To control the selection of treated women, we estimate a sequence of quarterly propensity scores.[12] The probability of having the first child within the next year is modeled as a function of human capital and employment history, which is particularly important if childbirth decisions are taken jointly with decisions regarding the labor market career. Moreover, we control for status of the relationship with the partner and for the self reported importance of

[10]Precisely, the condition is that the control women remain childless for at least three months after the respective age month of birth for the treated women considered.

[11]However, for the purpose of presenting our results succinctly, we will later group the month-specific treatment effects into groups defined by years of age, see section 3.3.

[12]The size of the treatment sample is not sufficient to go down to a monthly frequency when estimating the propensity scores. Furthermore, we think that the selection into childbirth does not change strongly from month to month.

having a family.[13]

We assume that the rich set of covariates used allows us to capture differences in the propensity to have a first child at a certain age. Specifically, we assume that given the duration in childlessness and given the covariates, having a first birth within the next year is random, i.e. a dynamic conditional independence assumption (DCIA as discussed in Fitzenberger et al., 2010) is likely to hold. Put differently, as Busso et al. (2008, p. 1) state: "Intuitively, these assumptions allow for treatment to covary with observed characteristics, but require that there be some unexplained variation in treatment assignment left over after conditioning and that the unexplained aspect of treatment resembles an experiment." Furthermore, we assume that the timing within the year considered, i.e. the month of birth, is unrelated to the selection into first birth.

Given the unconfoundedness of the treatment and the perfect overlap we observe in the propensity score, Busso et al. (2009) conclude that in small samples with unknown propensity score, a modified inverse probability estimator (IPW) performs best in comparison to various matching estimators. This result stands in contrast to the conclusions obtained by the Monte Carlo study in Frölich (2004). The crucial modification of the IPW estimator involves the normalization of weights for the nontreated women. According to the results of Busso et al. (2009), the poor performance of IPW reported by Frölich's (2004) Monte Carlo study is due to the fact that Frölich does not normalize the IPW weights. Doing so strongly improves the performance of the estimator as found by Busso et al. (2009), who suggest to estimate the ATT as follows (Busso et al., 2009, eq. (7)):

$$(1) \qquad \hat{\theta}_{BDM} = \frac{\sum_{i=1}^{n} T_i Y_i}{\sum_{i=1}^{n} T_i} - \frac{\sum_{j=1}^{n}(1-T_j)\hat{W}_j Y_j}{\sum_{j=1}^{n}(1-T_j)\hat{W}_j}$$

$$\text{with weights} \quad \hat{W}_j = (1-T_j)\hat{p}(X_j)/(1-\hat{p}(X_j)) \; .$$

Furthermore, T_i, T_j denote the treatment dummy variables for treated individuals i and nontreated individuals j, respectively, and $\hat{p}(X_j)$ denote the estimated propensity score by age year as a function of covariates X_j. In

[13]In fact, in contrast to matching, the reweighting estimator we will use later requires that the propensity score be a conditional probability (Busso et al., 2009) which is evident in our application. Due to the observation that overlap is satisfied we do not require any trimming.

contrast to Busso et al. (2009, eq. (7)), we use a simplified expression for the weights W_j for the nontreated women omitting the treatment dummy T_j which does not change the estimated average nontreatment outcome. The weights reweight the nontreated women according to the odd–ratio of having a child within the next year. According to the Monte Carlo results in Busso et al. (2009), it is crucial that the weights $(1-T_j)\hat{W}_j/\sum_{j=1}^{n}(1-T_j)\hat{W}_j$ are normalized such that they sum up to one, i.e. the sum of the weights in the numerator corresponds to the sum in the denominator in the second term on the right–hand-side of equation 1.

For our application, we need to adjust this estimation to the fact that for the estimation of treatment versus waiting the group of eligible comparison women changes by age month and the alignment between treated and non-treated observation changes as well by age. The higher the age, the smaller the 'eligible' control group for first birth. Thus, we modify the estimator in equation (1) to

$$(2) \qquad \hat{\theta} = \frac{\sum_{i=1}^{n} T_i \left\{ Y_i - \frac{\sum_{j=1}^{n}(1-T_j)\hat{W}_{i,j} Y_{j,age(i)}}{\sum_{j=1}^{n}(1-T_j)\hat{W}_{i,j}} \right\}}{\sum_{i=1}^{n} T_i}$$

$$\text{with weights} \quad \hat{W}_{i,j} = E_{i,j}(1-T_j)\hat{p}(X_j)/(1-\hat{p}(X_j))\ .$$

$E_{i,j}$ is a dummy variable which take the value of one if woman j can be used as a control woman for treated women i, i.e. if j remains childless for at least three months after the respective age month of birth for the treated woman i. $E_{i,j}$ is set to zero, if woman j has a child earlier than the three months after birth for i or if woman i is not having a child. $Y_{j,age(i)}$ is the outcome of control woman j aligned to the age of birth for treated woman i.

This IPW or reweighting estimator has the advantage of not relying on a tuning parameter such as the number of nearest neighbors in nearest neighbor matching or a bandwidth in kernel matching. Moreover, it is generally very easy to implement and the standard errors are readily obtained from bootstrapping. Busso et al. (2009) show that this estimator is preferred on bias and variance grounds in finite sample settings with unknown propensity score but stress, that this holds only under good overlap and when misspecification of the propensity is not a concern. The estimate is to be preferred to the IPW estimator discussed by Frölich (2004), who does not normalize the weights for the nontreated observations such that they sum up to one.

3.3 Outcome regression based on IPW

As a simple way to summarize the treatment effects across different age years and to assess effect heterogeneity in observable characteristics, we follow Abadie and Imbens (2006) who suggest to estimate ex post outcome regressions after matching and adjust their approach to the IPW case. We estimate weighted linear regressions of the individual outcomes on an intercept, a treatment dummy and various covariates (calender year, observable characteristics) plus their interactions with the treatment dummy. We use a weight of 1 for the treated women and a weight of $(1-T_j)\hat{W}_{i,j}/\sum_{j=1}^{n}(1-T_j)\hat{W}_{i,j}$ for the nontreated individuals with outcome $Y_{j,age(i)}$ aligned in age to treated woman i. Thus, for the outcome regression, we have to include a nontreated woman as many times as she is 'eligible' for treatment, each time with the age of the outcome measurement aligned to the treated woman i considered.

Specifically, we run the following regression of the average outcome Y after the beginning of treatment

$$(3) \quad Y_{j',age} = \alpha + x_j\beta + \gamma T_j + T_j(x_j - \bar{x})\delta + u_j \ ,$$

where x_j denote the observable characteristics considered, including year dummies. The outcome measurement $Y_{j',age}$ is recorded once for a treated woman and carries a weight of 1. The outcome measurement $Y_{j',age}$ for a nontreated woman is recorded as many times as there are treated women where the nontreated woman can be used as a control observation, each time aligned in age to the specific treated individual. Each single measurement carries the weight $(1-T_j)\hat{W}_{i,j}/\sum_{j=1}^{n}(1-T_j)\hat{W}_{i,j}$ as described above and the weights of eligible control women sum to one for each treated woman.

If $\delta = 0$ in the regression (3), then there is no linear effect heterogeneity by the level of covariates. The estimate for γ corresponds to the ATT estimate corrected for the mismatch in observable characteristics. The coefficients β control for the effect of the characteristics on the average outcome variable. The standard errors of the estimated regression coefficients are obtained through the bootstrap procedure for the IPW estimator by rerunning the regression (3) for all resamples.

4 Data

The analysis is based on data from the German Socio-Economic Panel (SOEP) which is a household panel with yearly interviews since 1984 of all persons above 15 living in a panel household. The wide range of questions includes employment related questions on hours, income etc. as well as questions on leisure, health, satisfaction and attitudes towards politics, values etc. Moreover, it contains information about the monthly employment status and income sources in the previous year allowing us to analyze our research question on a monthly basis.[14]

We use data from 1991 until 2008, because 1991 is the first year households from East Germany were included in the panel. Pensioners, disabled, and those for whom we have no pre-birth information are excluded from this study. However, we do not restrain the data to previously working women as this would introduce additional selectivity (Lauer and Weber, 2003). Furthermore, we only look at women between 22 and 33 years of age. Younger women are mainly in education and for older women the selection of the control group gets too strong, because the share of women in the control group who will have a child later decreases with age. Importantly, women who have children already are dropped from the data.

A woman without children can potentially have her first child in any particular month. Therefore, for every month we construct a different control group consisting of women who have not yet received their first child. Hence, our treatment group consists of women who receive their first baby at a particular age between 22 and 33. In contrast, the dynamically defined control group consists of women who do not have any child (so far). We try to avoid any conditioning on the future and therefore women in the control group are allowed to have children later. Still, the definition of non-treatment is crucial and involves the classification window which defines treatment and control group (see Stephan, 2009, for an application to active labor market policy). On the one hand, for a sharp differentiation between treatment and control group, the latter should not have their first child in the near future. On the other hand, the control group should be well comparable to the treatment group. As a compromise, we chose a window of three months during which control group individuals must not have their first child.

[14]Due to the wide range of questions this data set has been used before for analyzing maternal employment or the effects of maternity leave. See e.g. Bergemann and Riphahn (2009); Buligescu et al. (2009); Kuhlenkasper and Kauermann (2009); Sommerfeld (2009); Vogel (2009); Wrohlich (2004); Gustafsson et al. (2002); Kreyenfeld and Hank (2000).

The exact alignment of age in months is illustrated in figure 1. The first individual depicted by the top time line exemplifies a treatment observation who has her first child at age 25 years and 4 months. We will now consider the control group for this precise age. Individuals 1 and 2 are used as control group observations for this example and are therefore aligned by age (bottom panel). Note that the individual who has her first birth at 25 years and 6 months is not used for the treatment effect at age 25 years and 4 months, but will be in the treatment group at 25 years and 6 months.[15] However, the treatment effects will later be displayed pooled over two age years in order to smooth the effects. The size of the treatment and control group is depicted in figure 2.

For the estimation of the treatment effect it is essential that comparable women are matched together. This comparability is achieved by the exact alignment of age (in months) as has just been illustrated, and by a reweighting procedure is employed which is based on the propensity to have the first child at a certain age. The required pre-birth information should on the one hand, avoid Ashenfelter's Dip and therefore be collected at least 12 months before the birth. On the other hand, in order to use relevant information for the time of birth the interview should not have been longer than 15 months before the birth thus implementing a window width of three months. Reweighting on the propensity score is required to assure that the dynamic conditional independence assumption (DCIA) holds. Therefore, all relevant factors which jointly influence fertility and labor supply need to be captured. Hence, on the one hand we include human capital variables (degree of schooling and training education), the pre-birth labor market history (working hours, wage, job type, full time, part time and unemployment experience) as well as personal circumstances (partnership, marriage). Above that, we include the stated importance of the family for one's satisfaction and argue that this captures a mix of different values and attitudes which also add to the fertility decision (the probit estimates can be found in the appendix in table 2).

As can be seen from figures 3 to 6, some differences between the treatment and control group exist before birth with respect to the employment rate, educational degrees and partnership, particularly at younger ages. Also, the highest obtained educational degrees differ somewhat for younger women, as some of them are still in education.[16] These graphs also show that the

[15]Note that this individual forms part of the control group in every month before 25 years and 3 months.

[16]Recall that for the treatment effect at age 22 the training degree is usually measured at age 21 - an age where some training degrees are not completed, yet (particularly university).

weighting procedure works well to increase comparability between treatment and control group at the point in time before treatment starts. However, the employment rate of the treatment group before birth still remains slightly higher, i.e. young first-time mothers have a high labor force attachment before birth.

As outcome variable we use employment after the first birth. This does not further differentiate between full time or part time employment as part time work may be a stepping stone to higher working hours (Vogel, 2009). Moreover, in Germany labor force participation after first birth is very low (see e.g. Geyer and Steiner, 2007; Sommerfeld, 2009 particularly in full-time employment. Furthermore, there is no differentiation between homemaking and job search as these statuses cannot be separately identified reliably from the data. Finally, observing employment only on a binary scale has the advantage of abstracting from working time reductions around childbirth (Buligescu et al., 2009). We observe this information for 5 years starting from the month in which the treatment woman gives birth to her first child and is aligned to the treatment group with exactly the same age.

Figure 8 shows employment rates before and after birth with and without reweighting of the control group. Recall that matching takes place about one year before treatment start. It can be seen again, that the reweighting procedure works well for the alignment of the employment rate before birth, even though the treatment group still exhibits slightly higher employment rates. At birth, the employment rate of the treated observations drops to zero because of mandatory maternity leave for at least eight weeks after the birth. After this, the employment rate of first-time mothers recovers very slowly, while that of control women decreases slightly. This is due to the fact that women in the control group may also have their first child thus dropping out of employment at least temporarily.

To shed more light on this, figure 10 shows the rates at which treatment women have their second child or women in the control group give birth to their first one. It shows that after five years, about 60% of first-time mothers in all age groups have received a second child. Moreover, most women give birth to their first child between age 25 and 30 (cf. childbirth rates of the control group). Also, among the treatment group women who have their first child at age 30 or 31 have a second child rather quickly, whereas first-time mothers at age 24 or 25 tend to delay their second childbirth somewhat. The trend over age is not linear or simple, potentially indicating a large heterogeneity of fertility and employment histories.

5 Preliminary Results

5.1 Average Treatment Effects on the Treated for different age groups and its dynamics

In figure 11 - 16 the monthly treatment effects on the treated (ATT) for the five years after first birth are plotted. We calculate the effects by pooling two age groups, thus every figure refers to one of these groups. Within every figure the top left graph shows the pure effect where nothing but a time trend was controlled for. We will now discuss these results first before considering effect heterogeneity in the next subsection. An overview of these ATTs for all age groups is given in figures 17 and 18.

First of all note that the estimated ATTs are always negative, implying that within the observation window of five years the first-time mothers never catch up to the control group. This holds even though part time employment is also observed in our outcome variable and although women in the control group may also drop out of employment e.g. in relation with childbirth.

The developments of the ATTs over time are not always upward, but also downward movements are observed, which must be due to mothers who (re)enter the labor market after childbirth, but then drop out again. These downward movements seem not to be due to increases in the participation rate of the control group (figure 8). Note that all age groups but one display an almost monotonic decline in the employment rate of the control group, which is particularly pronounced for 26 and 27 year olds (figure 19). One exception is the group of 32 and 33 year old control women whose employment rate increases strongly starting about 3.5 years after treatment. This could be due to two reasons: First, the control women in this age group get their first child shortly after the treatment group and also have a high probability to return from maternity leave within five years after treatment. This reason is not plausible as figure 10 shows that the control women in this age group do not exhibit higher birth rates in the first years after treatment. Second, the birth rates of the control women in this age group are lower than in other groups especially several years after treatment which is confirmed in figure 10. Overall, the employment rate of the control group tends to increase with age.

In the following, we will focus on the dynamics of the ATTs. At point zero, namely the birth of a child, the absolute values of the ATTs are the highest because all mothers reduce their labor supply to zero by law. The different absolute values of the ATT between the age groups reflect the differences in the employment rates of the control group. Within the first year the ATTs for the different age groups are very similar, despite variation in the employment rates in the treatment and control groups. Put differently, in the treatment *and* control group, older women work more frequently than younger ones, but this cancels out in the estimation of the ATTs. Over the first year the ATT increases from nearly -90% to between -60% and -50%.

In the second year the ATTs continue catching up to between - 40% and - 30%. Exceptions are the groups of 22 amd 23 year and 32 and 33 year old first-time mothers for whom the value remains below - 40% in the second year. For the youngest group this result is driven by the low increase in the treatment group's employment rate in the first two years after birth. For the oldest group the employment rate increases relatively fast in the first ten months after birth but then stagnates until the end of the third year. Additionally, the high absolute value of the ATT is driven by a relative high employment rate of the control women in this age group.

Three years after birth we observe an upward jump in the ATTs and the employment rates of the treatment group which is due to the institutional changes which occur at that age (recall the job protection period and daycare availability.) This jump is particularly pronounced for the group of 32 and 33 year old first-time mothers, but the absolute value of the ATT is still high after the jump. This result contradicts expectations derived from human capital theory which led us to hypothesize that the ATT should be lower for older first-time mothers as they have already acquired a high level of human capital. Instead, this group displays the strongest ATT of about - 40% after four years and around - 30% five years after childbirth. The reason for a higher absolute value of the ATT of the oldest age group is the high participation rate of the control group. Also, the group of women who receive their first child at age 24 or 25 exhibits a huge jump in the ATT after around three years and the ATT gets closest to zero within the five years after treatment compared to other age groups. The jump can also be seen in the employment rate after first birth, reaching 50% after three years. Additionally, the employment rate of the control group is the lowest around 3-4 years after the treatment. This could be due to the fact that the average age at first childbirth is about 28 years in Germany, which likely affects the behavior of the control group 4 years after treatment start for the treatment group at age 24 and 25.

After five years the levels of the ATTs are rather similar at around -20% which is reflects the similarity of employment rates of the treatment and control groups. Again, exceptions are the results for the groups of 24 and 25 and 32 and 33 year old first-time mothers. The ATT of the former group is much lower in absolute values after five years which is partly due to a somewhat higher participation rate of the treatment group but mainly due to a relatively low participation rate of the control group.

Over the whole observation period the large negative effect for the group of 32 and 33 year olds stands out. This group displays a very high labor force attachment before birth and therefore is of great relevance to policy-makers. In fact, current reforms aim at boostering the labor force participation of previously employed women. The expectation is that the opportunity costs of maternity leave breaks are higher for later childbirth which should lead to higher participation rates after birth. Therefore it is striking that the ATTs for the oldest group that we observe (which is by far not the oldest to have a first child after having established a career) are lowest implying the largest observed employment losses.

5.2 Heterogeneity of the Average Treatment Effects on the Treated

In the previous chapter we discussed differences in the ATTs between age groups without taking other sources of heterogeneity into account. Probably, the timing of first birth depends on the current employment experience and on certain career steps a woman has already reached. As the years of education greatly affect the career progression at a certain age this should influence the timing of first birth and thus the ATTs. In Germany young employees with a vocational training degree generally enter the labor market in the early twenties at the latest. Academics usually complete their university degree between 25 and 30 years of age. Figure 7 shows the timing of first births for the different education groups. Obviously, the age of first birth depends strongly on the educational level. Thus, it is important to take these differences into account when calculating the ATTs for different age groups. Especially, the outstanding results of the 32 and 33 year olds are expected to differ between the education groups as particularly academics start having children at an older age.

In figures 11 - 16 the top right graphs show the ATTs where heterogeneity in educational degrees is taken into account. Since the reference group are women with vocational training the graph shows the ATTs for this group. In the following graphs the impact of university degree, no training degree and still being in education on the ATTs with effect heterogeneity (left hand side) as well as the coefficients of the education dummies (right hand side) are shown.

In order to get insights into the employment behavior of the different age groups we first consider the coefficients of the education dummies (graphs in the right column). The conditional participation rate of women who are still in education is low at the beginning of the five year observation period but increases strongly during this time period. Young women in education never catch up to the group with vocational training but the negative coefficient becomes insignificant for older women most likely because there are only few women in education, anymore. Women without a training degree exhibit low and decreasing conditional employment rates, where this pattern is particularly pronounced in the youngest and the two oldest age groups. The coefficients for women with university degree are positive and slightly increasing in age in almost all cases, but not significantly different from the group of women with a vocational training degree.

Now we discuss the impact of education on the ATTs. First of all note that the ATTs for the group with vocational training (which is the largest group) hardly differ from the ATTs without accounting for heterogeneity. The most interesting group are women with university degree since we expect to find an impact of higher opportunity costs for employment breaks. Until the age of thirty there is hardly a difference in the ATTs between women with vocational training and women with university degree. In the age group of 30 and 31 years there is a positive effect of having a university degree on the ATTs in the first months after birth. This impact increases in value and significance for the age group of 32 and 33 year olds and also is rather high for the first three years after birth. Obviously, first-time mothers with a high education level and some work experience exhibit a higher attachment to the labor market in the first three years after birth compared to medium educated first-time mothers.[17] This result is very well in line with human capital theory.

[17]Note again that part-time work is also included in the analysis.

6 Conclusions

In a critical phase of one's life crucial career and fertility decisions have to be taken jointly. It has to be taken into account that the impacts of childbirth on career outcomes vary greatly with human capital and thus also with age. Delaying the first childbirth to a later age could pay off positively in terms of career development (Herr, 2007). Therefore, this paper studies the question whether and to what extent it makes a difference to have the first child at an earlier or later age.

To answer this question we start from the individual decision whether to have the first child now or wait. This is modeled in a dynamic treatment effects framework in analogy to Sianesi (2004, 2008) and Fitzenberger et al. (2010). We then reweight these dynamically constructed control groups by inverse probability weighting (IPW) based on the finding that this procedure has better performance with respect to bias and variance if the weights are normalized to sum to one as found by Busso et al. (2009). Finally, the ex-post outcome regressions proposed by Abadie and Imbens (2006) allow us to explicitly consider effect heterogeneity for different educational groups.

Our results show generally large and persistent negative average treatment effects on the treated (ATTs) for first childbirth at a certain age on employment. Even though we document large dynamics in the observation period of five years since childbirth, the treatment effects never reach zero within that period, implying that the employment rates of treatment and control group do not equate. In other words: children cost employment. The strong negative effects for the oldest considered age groups of 32 and 33 year olds stand out, as they contradict expectations from human capital theory according to which these women face the highest opportunity costs of leaving the labor market. This age group is of high political relevance, but it is by far not the oldest to have the first child after having established a career. When considering effect heterogeneity with respect to educational degrees it becomes clear that university graduates face weaker ATTs, particularly from age 30 onwards, which reconciles with human capital theory.

In a next step we are planning to consider effect heterogeneity with respect to education levels in more detail. Moreover, potential effect heterogeneity regarding calendar year could allow us to evaluate a law change which facilitates part-time employment from 2002 on. This law change together with current reforms in parental leave as well as the debate on poverty among families emphasize the policy relevance of further research on this topic.

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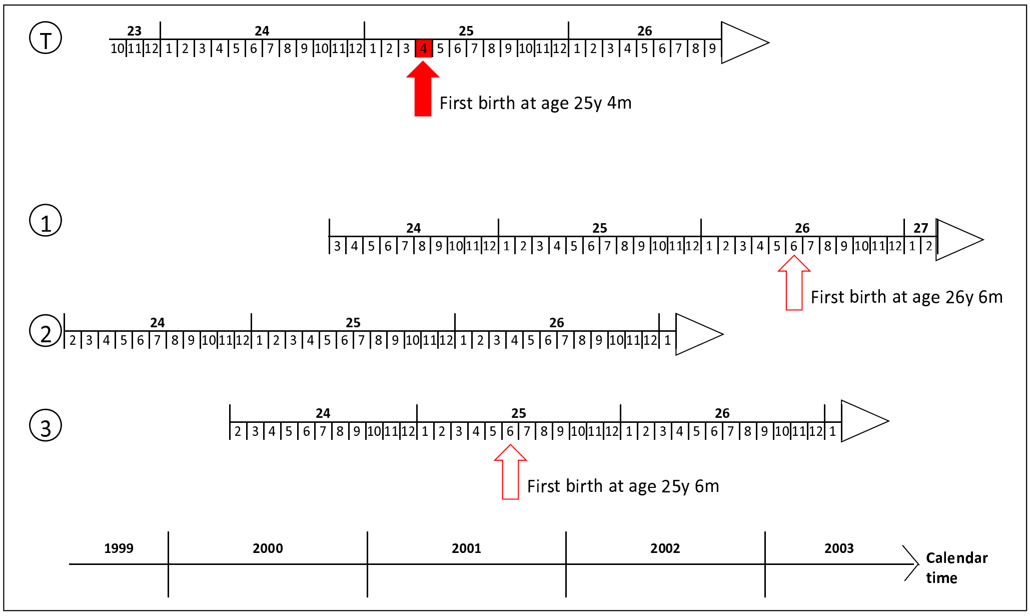
Appendix

Figure 1: Data Alignment

Before Alignment



After Alignment

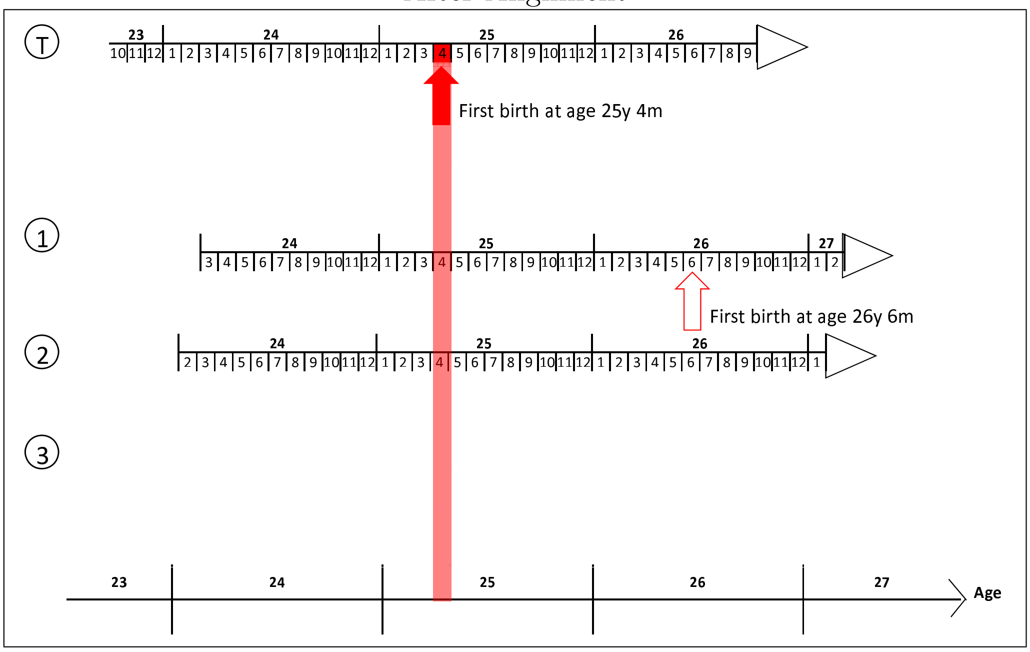
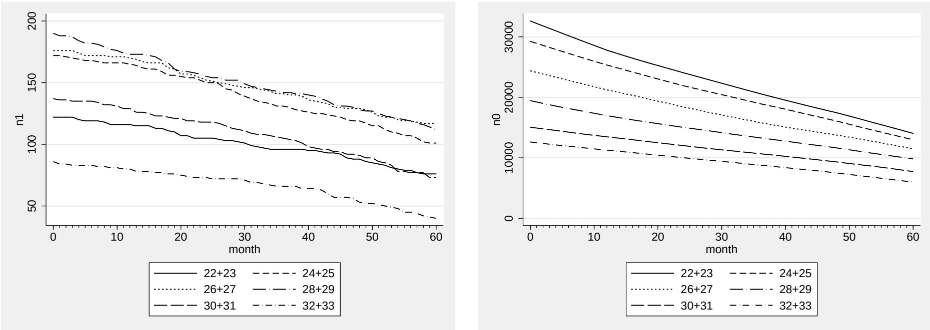


Table 2: Propensity for having the first child (probit regression results)

Variable	Coefficient	(Std. Err.)
Wage	-0.001	(0.003)
Actual working hours	0.002	(0.001)
Part time	-0.004	(0.060)
Blue collar	0.034	(0.045)
Self-employed	-0.013	(0.090)
Civil servant	0.081	(0.061)
Employer mainly publicly owned	-0.025	(0.042)
Experience full time	-0.020	(0.063)
Experience part time	-0.033	(0.096)
Experience unemployment	0.209	(0.161)
Experience full time squared	-0.002	(0.001)
Experience part time squared	-0.002	(0.003)
Experience unemployment squared	0.012	(0.007)
No training degree	-0.220	(0.331)
University degree	-0.727	(0.423)
In education	-1.146	(0.447)
Living in East	0.100	(0.036)
Partnership	0.186	(0.037)
Age*Importance of family very high	-0.002	(0.002)
Age*Importance of family high	-0.006	(0.003)
Age*Importance of family low or very low	-0.017	(0.010)
No. of years since when importance of family was last asked * ...		
...Importance of family very high	-0.109	(0.068)
...Importance of family high	-0.126	(0.109)
...Importance of family low or very low	-0.020	(0.629)
...Age*Importance of family very high	0.004	(0.002)
...Age*Importance of family high	0.006	(0.004)
...Age*Importance of family low or very low	0.001	(0.023)
Age 20	0.057	(0.127)
Age 21	-0.040	(0.070)
Age 22	-0.027	(0.067)
Age 24	0.024	(0.063)
Age 25	-0.098	(0.069)
Age 26	0.037	(0.069)
Age 27	-0.009	(0.076)
Age 28	-0.011	(0.086)
Age 29	-0.124	(0.102)
Age 30	-0.066	(0.118)
Age 31	-0.336	(0.146)
Age 32	-0.283	(0.170)
Age 33	-0.272	(0.198)
Age*Married	0.016	(0.001)
Age*Experience full time	0.002	(0.003)
Age*Experience part time	0.001	(0.004)
Age*Experience unemployment	-0.010	(0.006)
Age*No training degree	0.007	(0.013)
Age*University degree	0.026	(0.015)
Age*In education	0.038	(0.019)
After law change	-0.441	(0.076)
Intercept	-2.267	(0.088)

Year dummies have been controlled

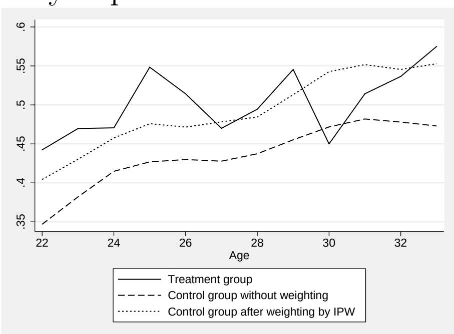
Figure 2: Size of treatment (left) and control group (right) by age



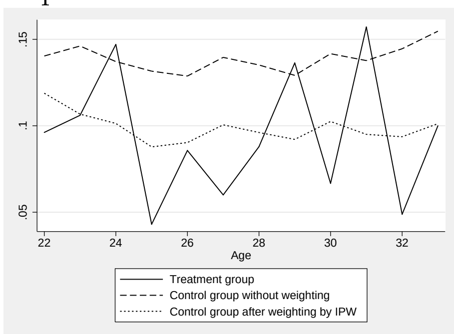
Descriptive Statistics one year before birth stratified by age

Figure 3: Importance of family for satisfaction one year before birth (shares)

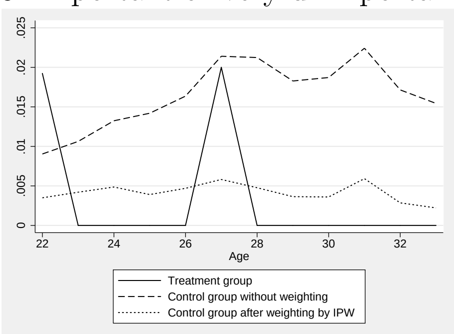
Very important



Important



Unimportant or very unimportant



Missing

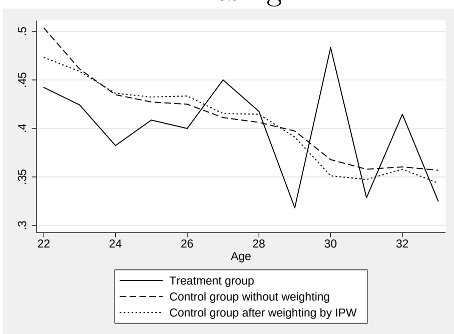


Figure 4: Partnership
one year before birth

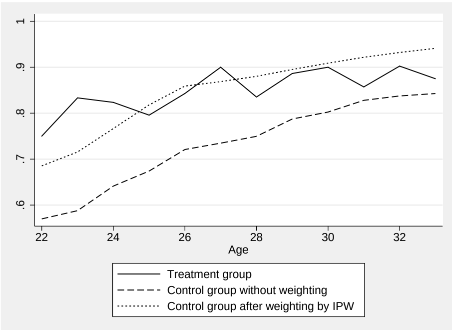


Figure 5: Employment
one year before birth

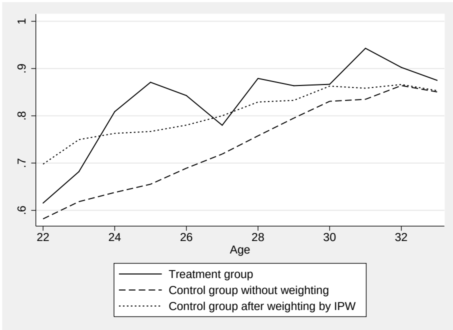
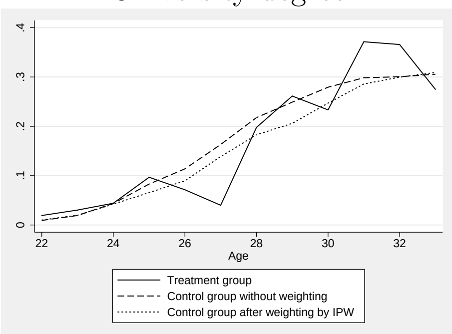
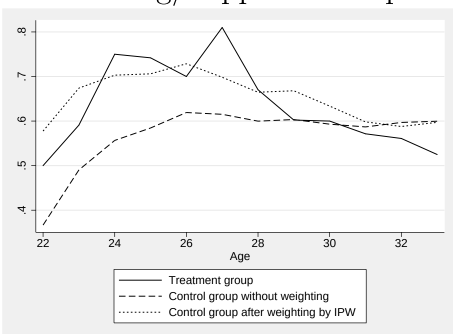


Figure 6: Completed training degrees one year before birth

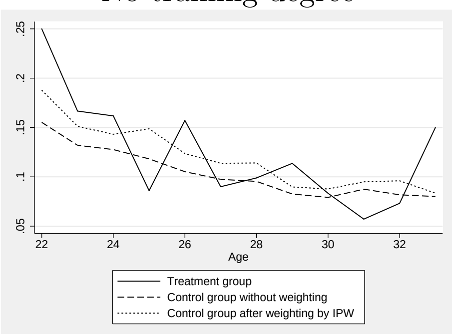
University degree



Training/ Apprenticeship



No training degree



In education (or missing)

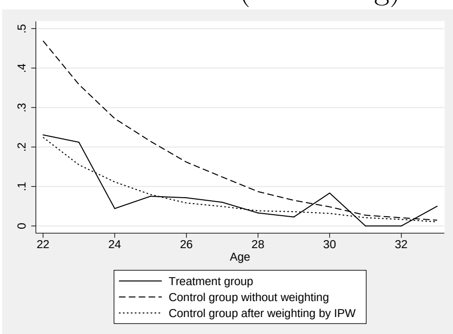


Figure 7: Age at first birth by training degrees

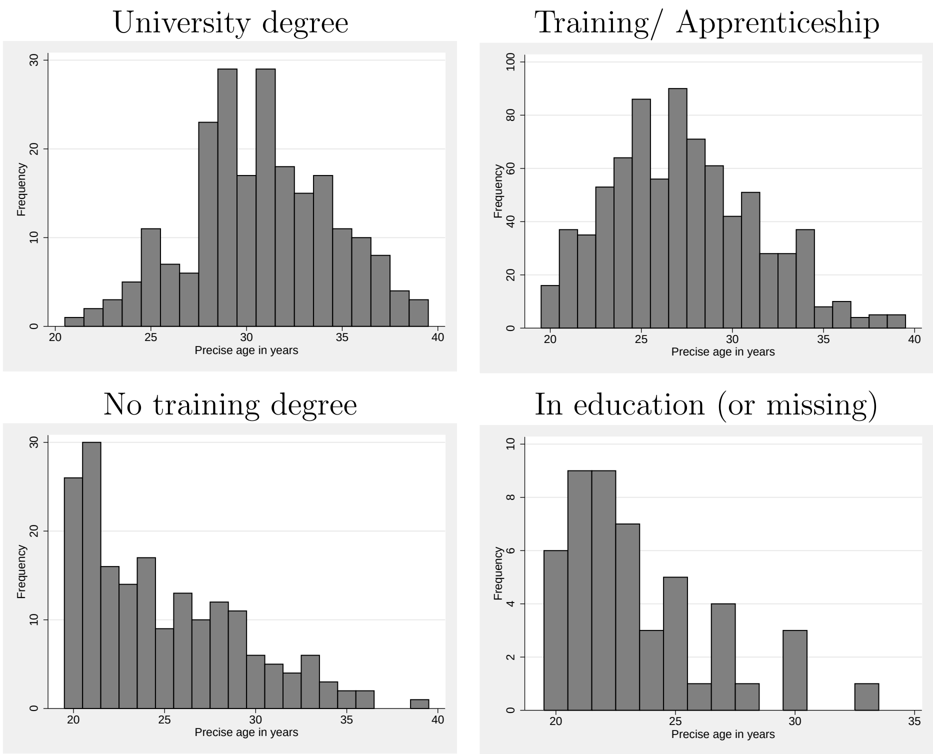


Figure 8: Employment before and after reweighting

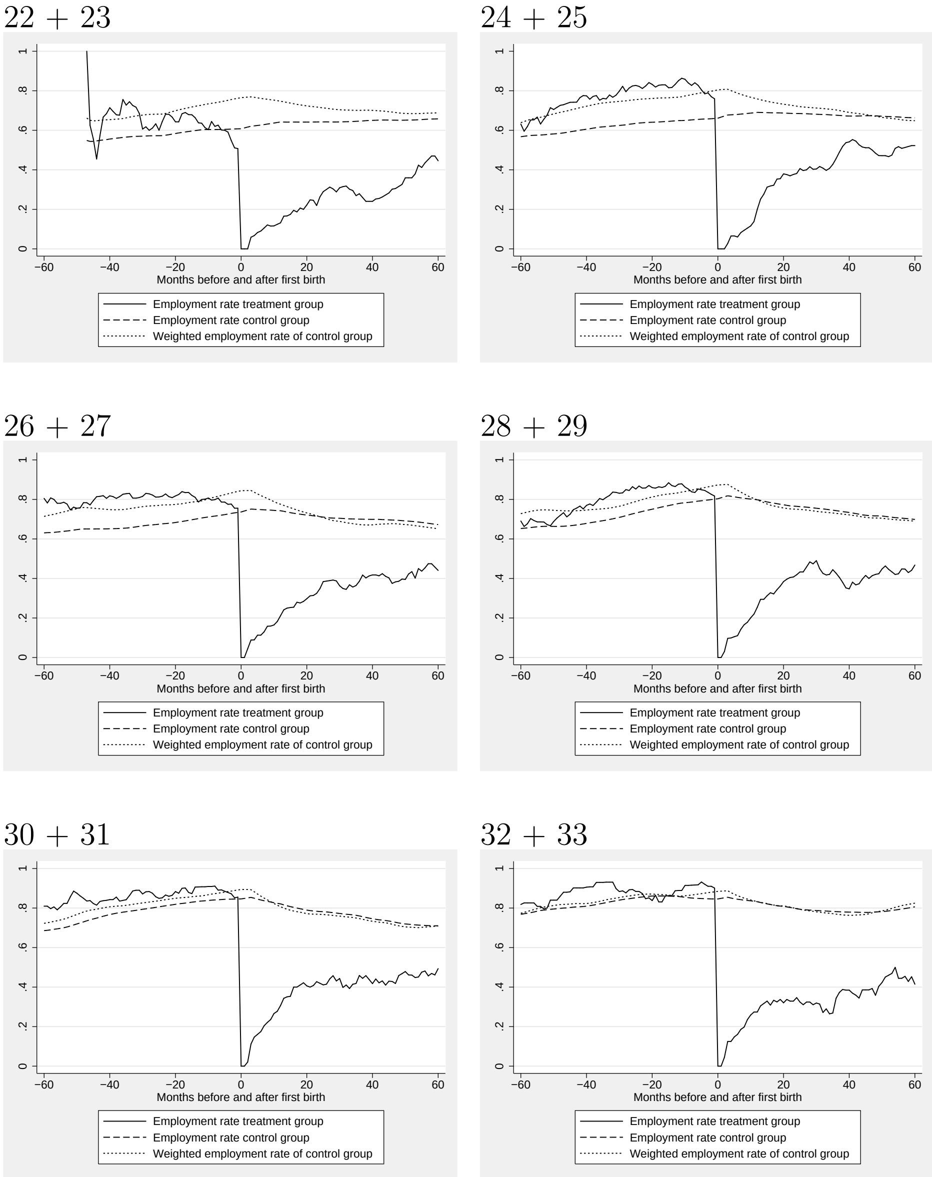
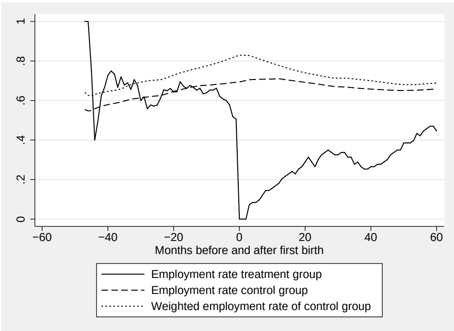
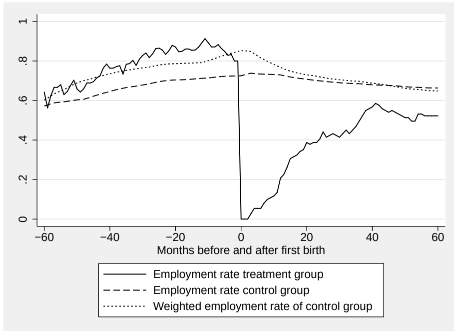


Figure 9: Employment before and after reweighting
Only for the subsample of those, who are still in the sample after 5 years

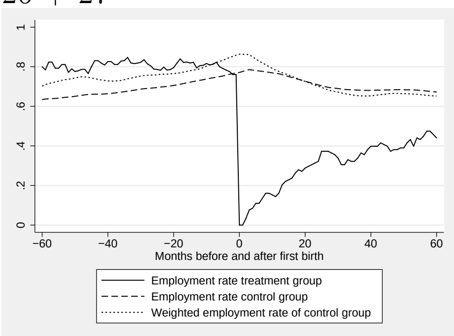
22 + 23



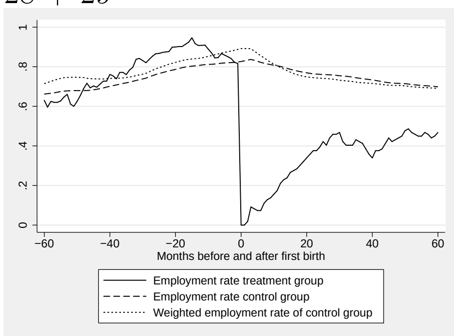
24 + 25



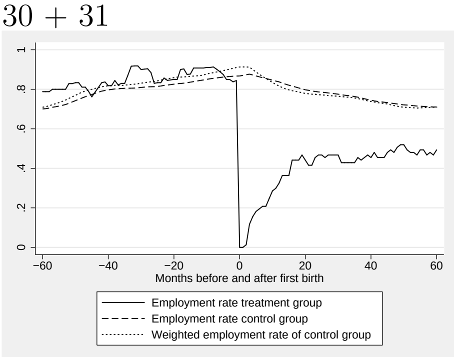
26 + 27



28 + 29



30 + 31



32 + 33

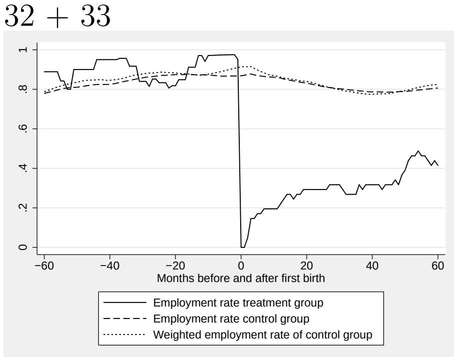
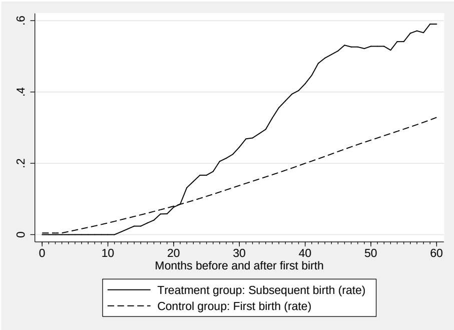
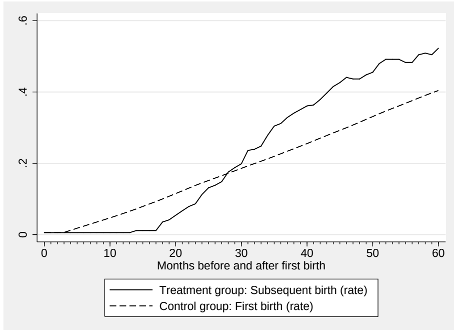


Figure 10: Childbirth rates for treatment and control group

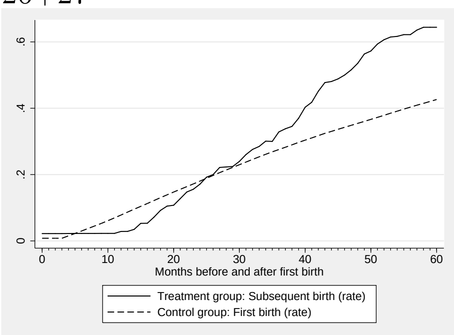
22+23



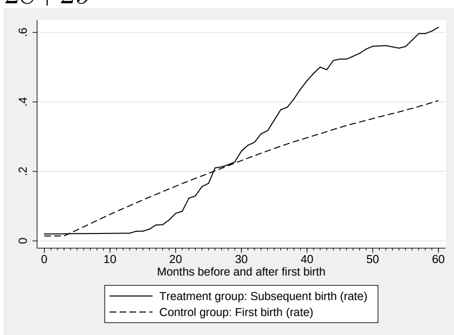
24+25



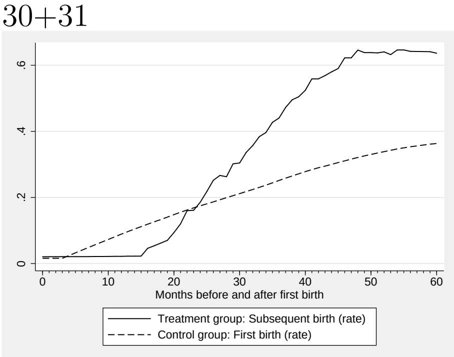
26+27



28+29



30+31



32+33

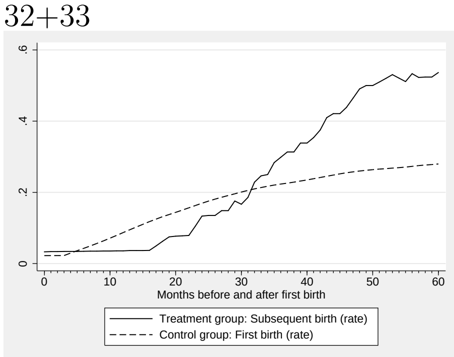
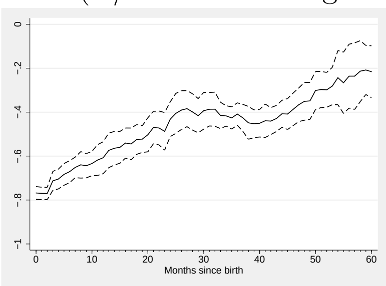
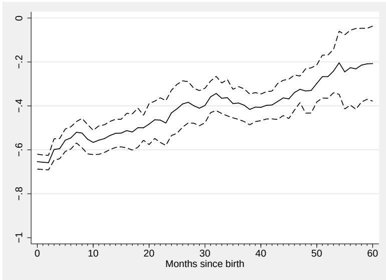


Figure 11: Age 22 + 23

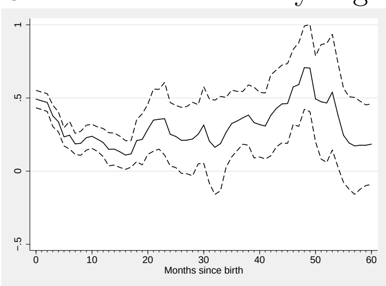
ATT (w/o effect heterogeneity)



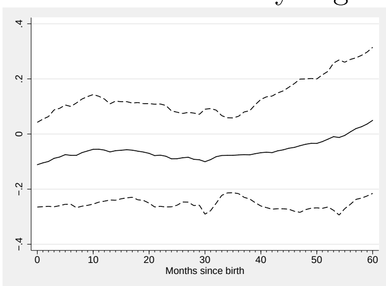
ATT with effect heterogeneity



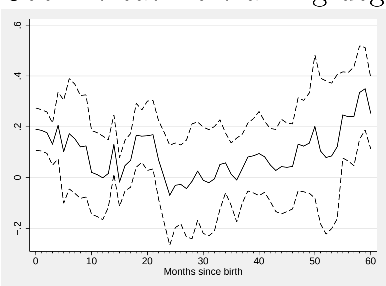
Coeff: treat*university degree



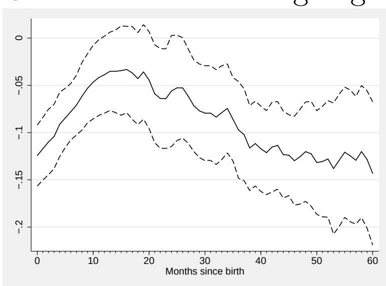
Coefficient university degree



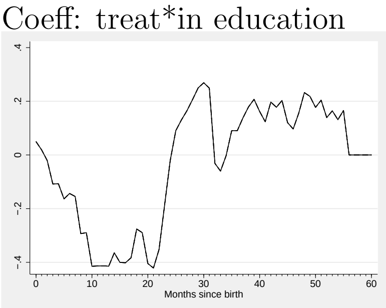
Coeff: treat*no training degree



Coefficient no training degree



Coeff: treat*in education



Coefficient in education

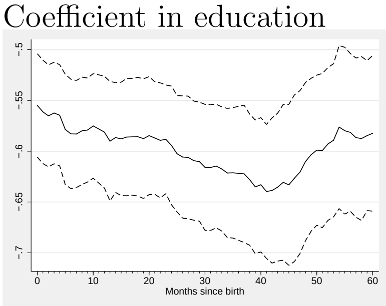
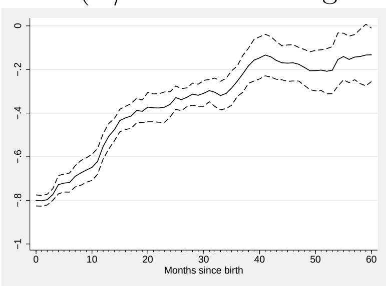
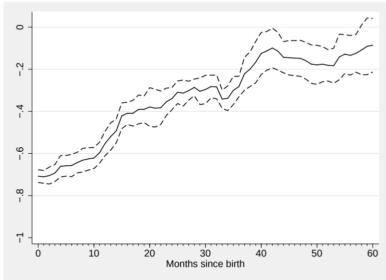


Figure 12: Age 24 + 25

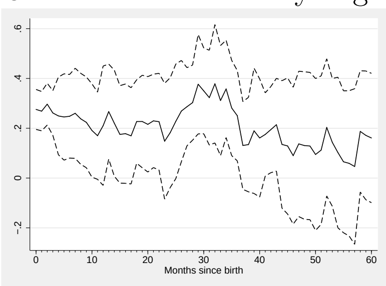
ATT (w/o effect heterogeneity)



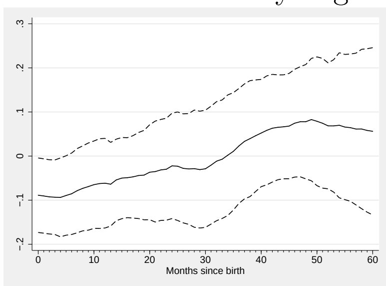
ATT with effect heterogeneity



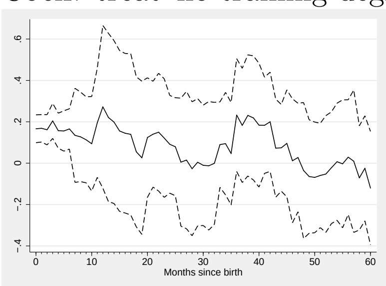
Coeff: treat*university degree



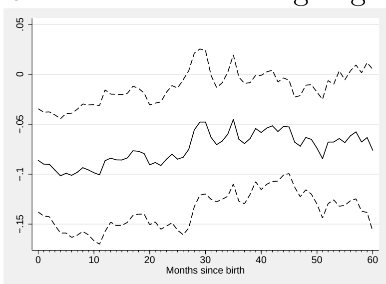
Coefficient university degree



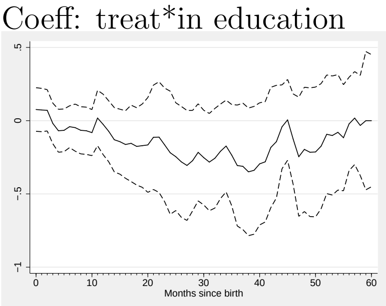
Coeff: treat*no training degree



Coefficient no training degree



Coeff: treat*in education



Coefficient in education

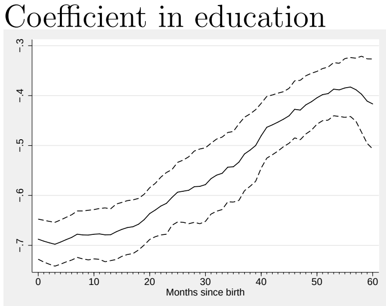
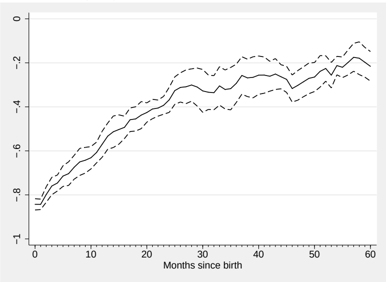
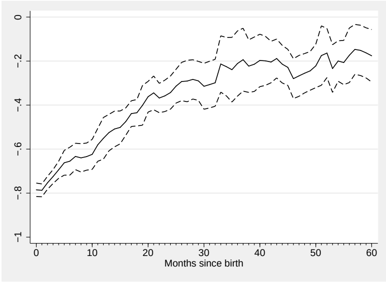


Figure 13: Age 26 + 27

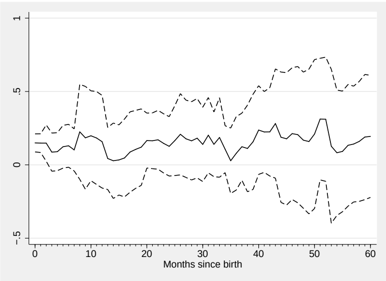
ATT (w/o effect heterogeneity)



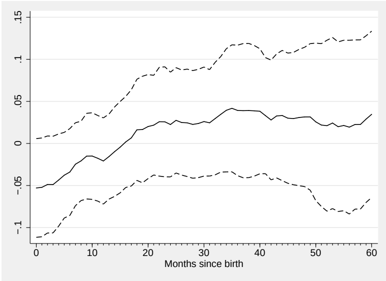
ATT with effect heterogeneity



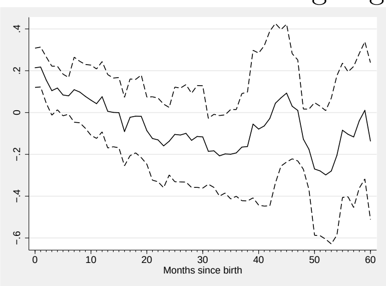
Coeff: treat*university degree



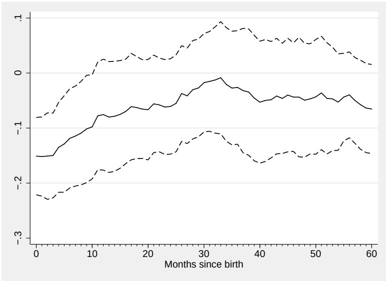
Coefficient university degree



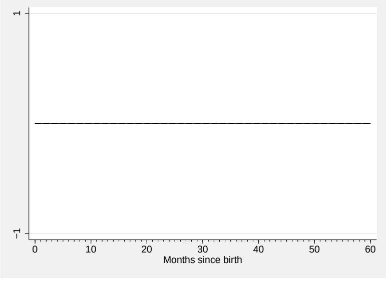
Coeff: treat*no training degree



Coefficient no training degree



Coeff: treat*in education



Coefficient in education

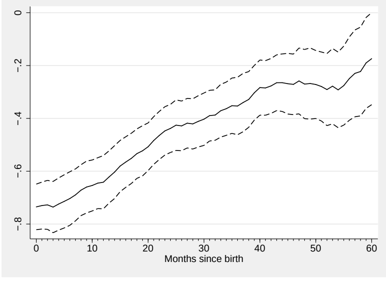


Figure 14: Age 28 + 29

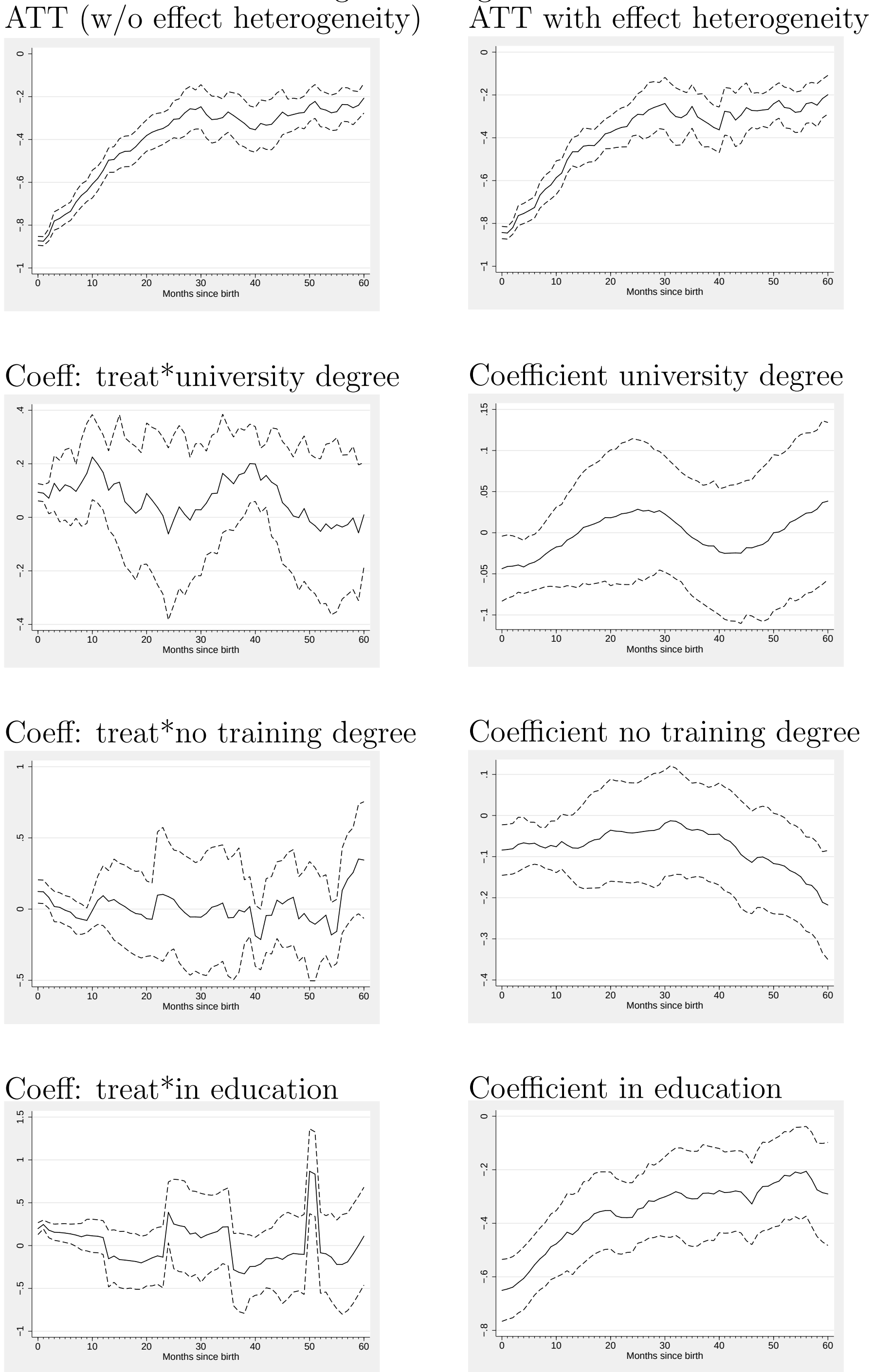
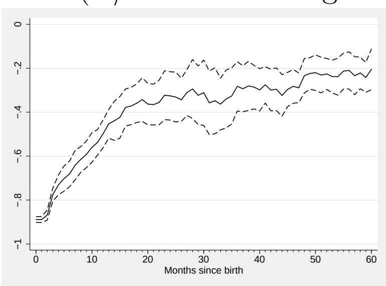
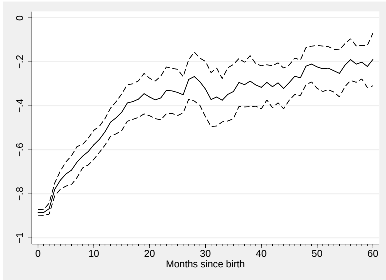


Figure 15: Age 30 + 31

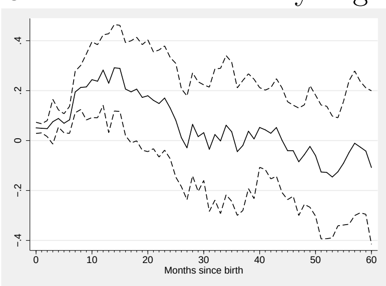
ATT (w/o effect heterogeneity)



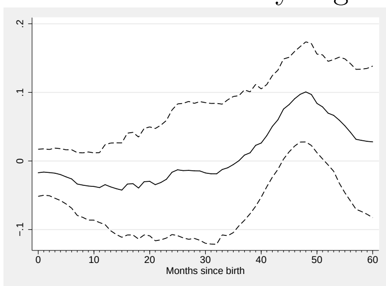
ATT with effect heterogeneity



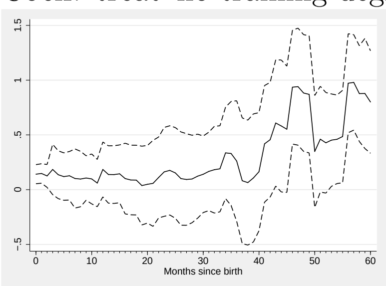
Coeff: treat*university degree



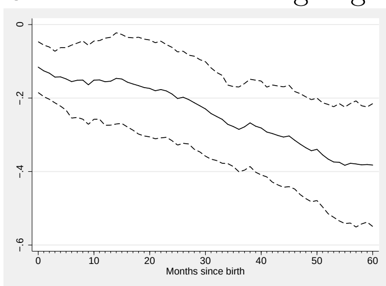
Coefficient university degree



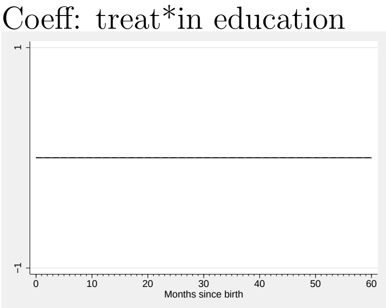
Coeff: treat*no training degree



Coefficient no training degree



Coeff: treat*in education



Coefficient in education

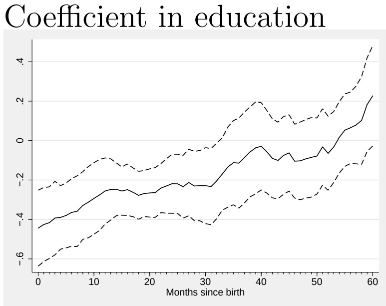


Figure 16: Age 32 + 33

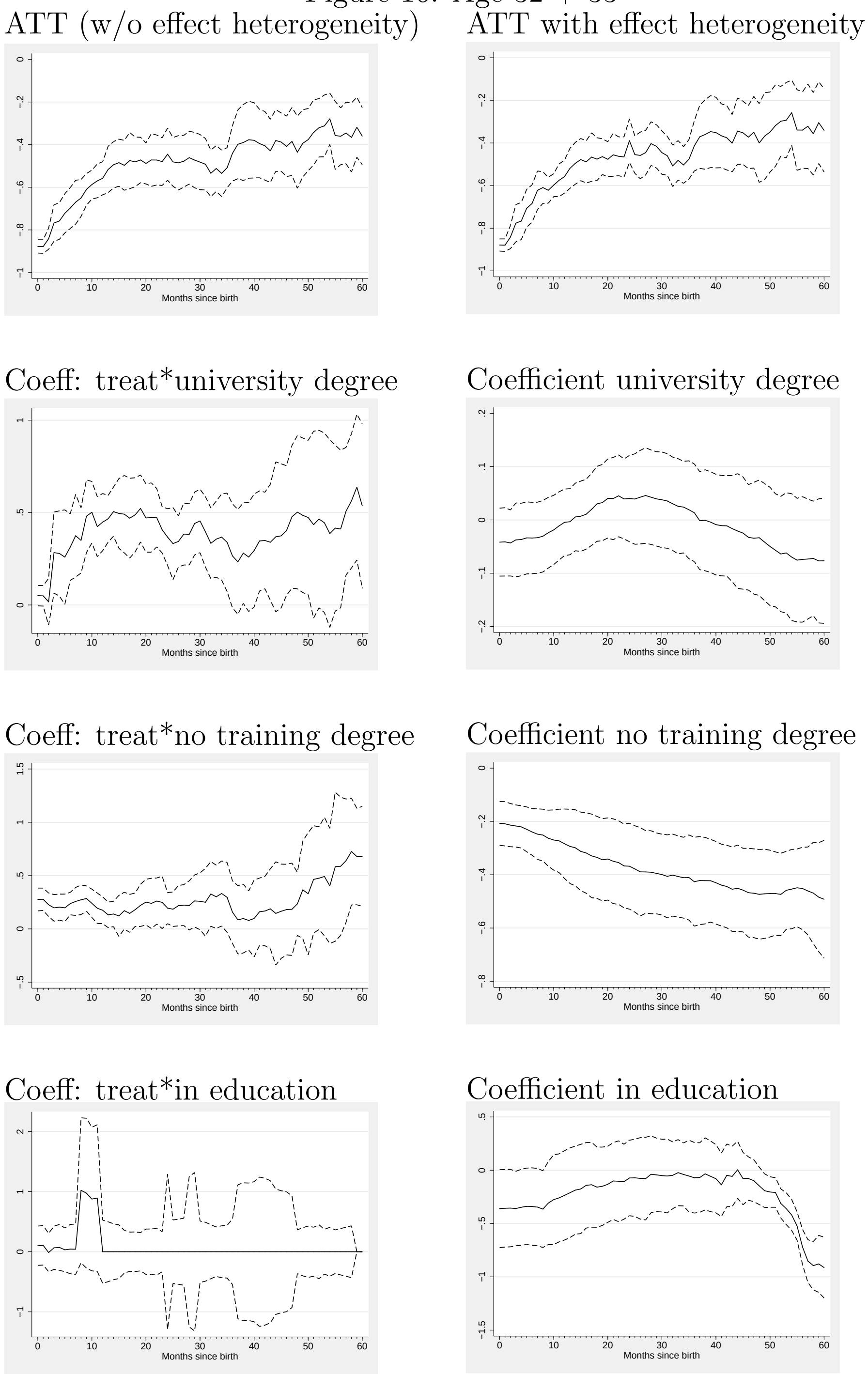


Figure 17: Overview: ATTs without effect heterogeneity

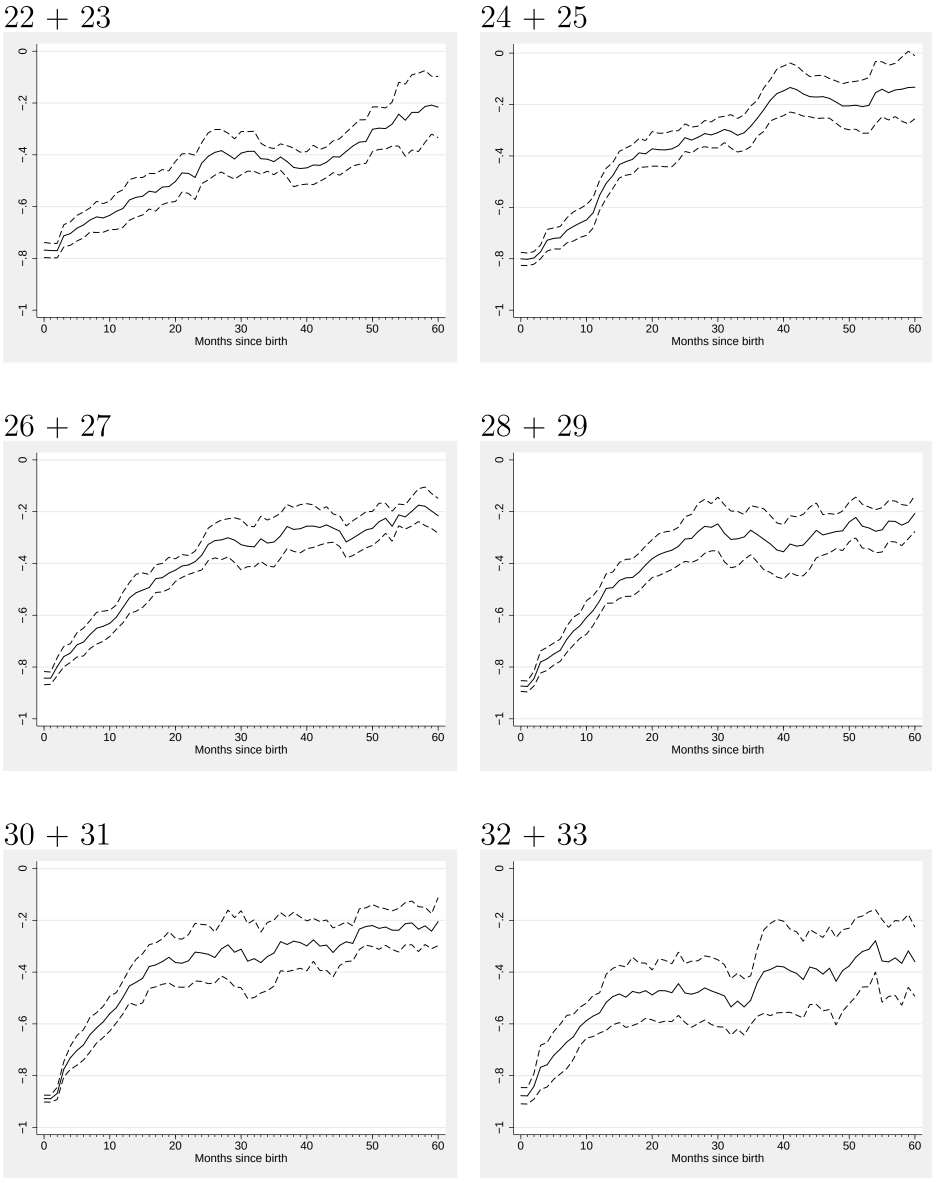


Figure 18: Average Treatment Effects on the Treated (ATTs)

ATT without effect heterogeneity

ATT with effect heterogeneity

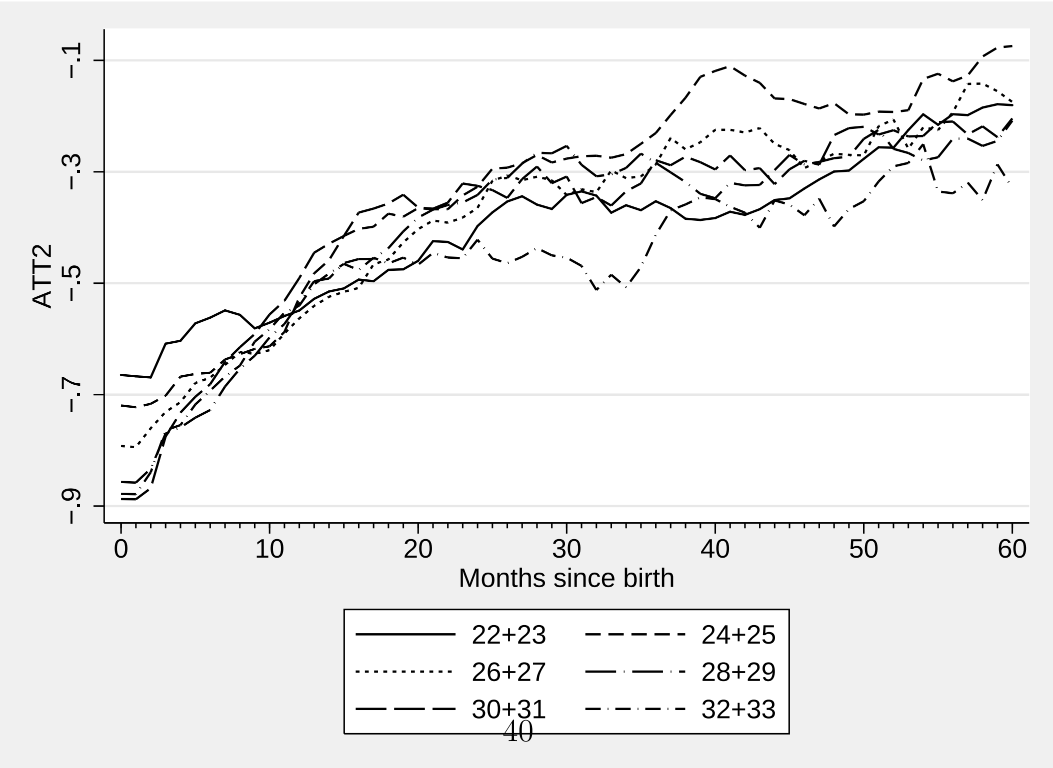


Figure 19: Employment rates

Treatment group

Control group