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The Effects of the Timing of Childbirth on Female Labour Supply:

An Analysis using the Sequential Matching Approach

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The Effects of the Timing of Childbirth on Female Labour Supply: An Analysis using the Sequential Matching Approach[†]

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Abstract

In this study, we estimate the effects of childbirth on female labour supply by using Japanese data. The novel contributions of our study are twofold. Firstly, we include the effects of unobserved preferences on female labour supply. Secondly, we apply a dynamic version of the sequential matching approach to analyse the causal effects of childbirth on female labour market outcomes. The estimated results show that childbirth decreases current employment outcomes (participation in regular and non-regular work) and that this decrease is larger for regular employees than for non-regular employees. On the timing of childbirth, while the negative effects of childbirth on regular work increase by delaying the age at childbirth, these negative effects on non-regular employment slightly decrease by delaying the age at childbirth. On future employment outcomes, childbirth does not affect the probability of choosing non-regular work in the next period regardless of childbearing age. By contrast, delayed childbirth decreases the probability of choosing regular work in the next period significantly.

Keywords: Dynamic Treatment Approach; Sequential Matching Method

JEL classification codes: J21, C21, C25

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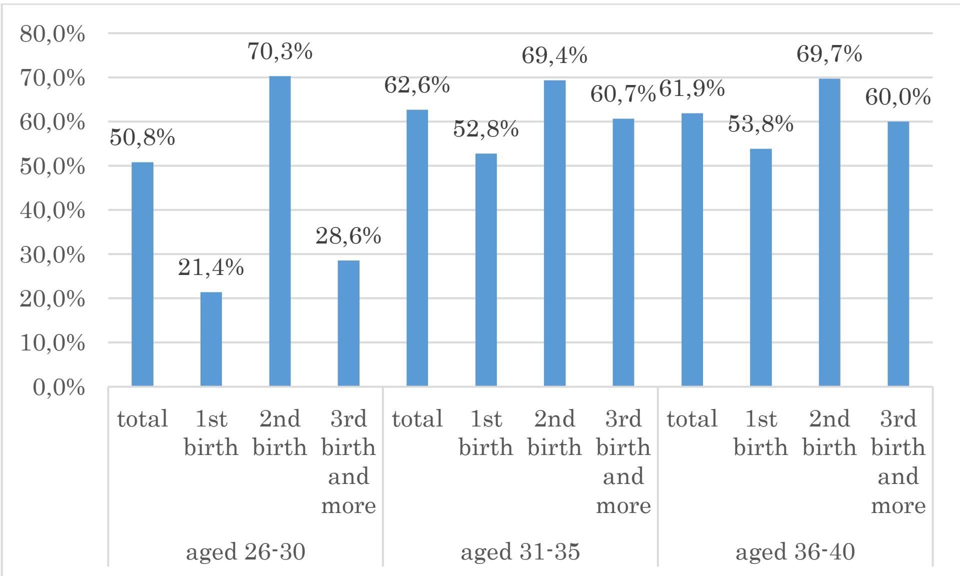
1 Introduction

The declining birth rate in Japan is one of the causes of late marriage and subsequently late childbirth. According to the Cabinet Office, Government of Japan (2014) report, the mean age of mothers giving birth to their first child increased from 26.4 years in 1980 to 30.3 years in 2012. However, while late childbirth might decrease the *future* labour force, it increases the *current* labour force. Figure 1 shows the job continuity rate by childbearing age and birth order. The delay in the timing of childbirth from 26–30 years to 31–35 and 36–40 years increases the job continuity rate by 11.8 and 11.1 percentage points, respectively. Further, birth order affects the job continuity rate non-linearly: compared with the first birth and third birth, the suppressing effects of the second birth on job continuity are minor.

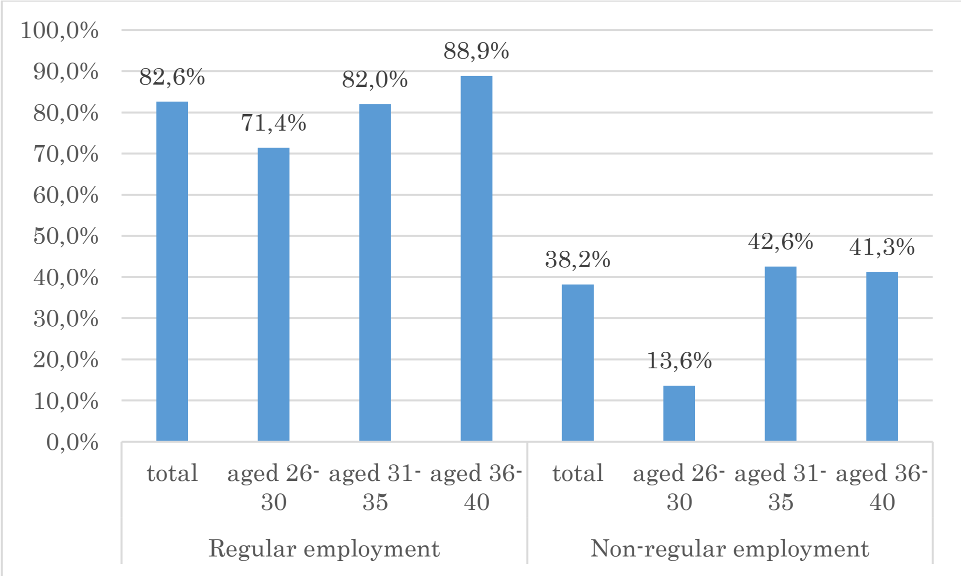
Figure 1. Job continuity rate by childbearing age and birth order



Source: Ministry of Health, Labour and Welfare (2015)

Figure 2 shows the job continuity rate by employment status, namely regular and non-regular employees. Since the job separation of regular employees because of childbirth imposes a considerable cost on both employers (e.g. training costs) and employees (e.g. opportunity costs for regular employees), the job continuity rate is expected to be higher for regular employees than for non-regular employees. As shown in Figure 2, the job continuity rate of regular employment is indeed twice that of non-regular employment.

Figure 2. Job continuity rate by employment status

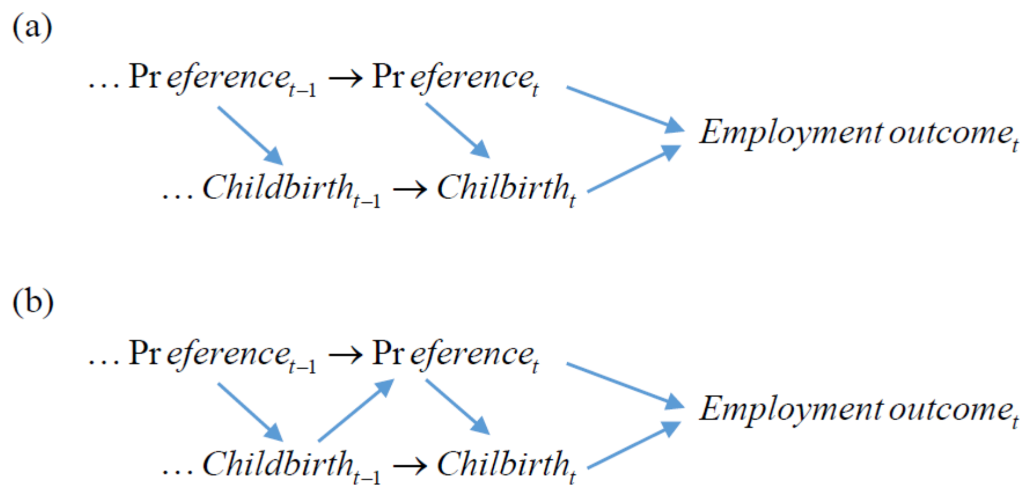


Source: Ministry of Health, Labour and Welfare (2015)

To implement policy targeting the falling birth rate in Japan, we must understand the causal effects of the timing of childbirth on employment outcomes. However, when analysing the effects of female fertility decisions on labour market outcomes, the endogeneity of such decisions is an issue for econometricians. A large number of studies have tackled this issue by applying the instrumental variable method (Angrist and Evans (1998)) or by modelling fertility and labour supply jointly (Troske and Voicu (2010)).

On the contrary, the dynamic treatment approach allows the identification, estimation, and inference of the causal effects among endogenous variables (Robins (1986)). In particular, this approach addresses the presence of time-varying confounding factors between the treatment and outcome variables. Consider the case that married women's labour force participation is influenced by childbirth and their preference for having a child. In this setting, if past preferences affect childbirth but the reverse does not, an appropriately specified regression model provides a consistent estimator of the causal effects of childbirth on labour force participation by controlling for preference history (Figure 3(a)). However, if preferences are also affected by past childbirth, a regression model conditioning on preference history-related variables is unsuitable because the causal effects of childbirth *through these preferences* is blocked (Figure 3(b)). In this case, the preference confounds the causal effects of childbirth on labour force participation.

Figure 3. Causal diagrams: (a) childbirth is sequentially confounded by preferences, (b) childbirth is sequentially confounded by preferences and preferences are affected by past childbirth.



This study's novel contribution to the literature is to address the endogeneity of fertility decisions by considering explicitly the *time-varying confounder* that sequentially confounds the causality from married women's fertility decisions to labour supply. We apply Lechner's (2008) sequential matching method to estimate the dynamic treatment effects of childbirth on female labour supply, using Japanese data. In the context of the Japanese labour market, although many studies have investigated the effects of family size on female labour market outcomes, little work has examined the marginal impact of childbirth on such outcomes.[1] We therefore estimate the impact of childbirth on female labour supply by using the matching method, which is a relatively new and innovative statistical approach.

The contributions of our study are twofold. Firstly, we include the unobserved preference as a time-varying confounding variable explicitly in the effects of childbirth on female labour supply. Although the confounding variable should be defined explicitly as well as the treatment and outcome variables, previous studies of female labour supply that have used the matching method have failed to provide a suitable definition (Fitzenberger et al. (2013a), Lechner (2009b)). Hence, by identifying the time-varying confounding variable explicitly, we can not only estimate the causal effects, but also infer

[1] Exceptions include Nakamura and Ueda (1999) and Kenjo (2005). Nakamura and Ueda (1999) investigate the determinants of the job continuity of married women facing their first childbirth. Kenjo (2005) analyses the impact of the first childbirth on subsequent employment status.

the mechanism driving these causal effects based on the sequential causal diagrams presented in Figure 3.

Secondly, we apply a dynamic version of the sequential matching approach to analyse the causal effects of childbirth on female labour market outcomes. The sequential matching approach is an extension of Robins' (1986) method, which was originally developed by Lechner and Miquel (2001). The distinctive features of the sequential matching approach are its modelling flexibility and ease of implementation. As Lechner (2009b) shows, this offers considerable patterns describing the space and timing of the treatment. Further, because it is in the propensity score matching family, the estimation is easily implemented by using a suitable software package.

The difference in the job continuity rate after childbirth between regular and non-regular employees shown in Figure 2 is partly because of the difference in the observed and unobserved characteristics of employment status. If these characteristics are sequentially correlated as shown in Figure 1, the "true" causal effects of childbirth on employment outcomes are difficult to infer by using standard regression analysis. However, adopting the sequential matching method can reveal the dynamic causal effects of childbirth on multi-status employment outcomes.

The rest of this paper is organized as follows. Section 2 reviews the dynamic treatment approach and Section 3 presents the sequential matching model used in this study. Section 4 describes our data set and the main variables. Section 5 shows the empirical findings. Finally, Section 6 concludes.

2 Dynamic Treatment Approach

The dynamic treatment model was originally developed in epidemiology to estimate the causal effects of a treatment (e.g. a drug) on health outcomes (e.g. a patient's health status) when the variables confound the causal effects of the former on the latter. Indeed, since the paper by Robins (1986), a number of studies have been published on this topic.[2]

In economics, the dynamic treatment model has developed along two dimensions: statistical and econometric (structural) approaches.[3] The former aims to deal with the time-varying confounding factor between the treatment and outcome variables

[2] Robins' (1986) approach is based on the sequential randomization assumption of the treatment. Robins and Hernan (2009) and Daniel et al. (2013) subsequently review methods for estimating the dynamic treatment effect in the context of epidemiology.

[3] Heckman (2005) criticizes this statistical approach as it does not fully specify the mechanism underlying the causal model. On Heckman's critique, see also Guo and Fraser (2015).

statistically, and this approach has mainly been applied to analyse the effects of active labour market policy (Sianesi (2004, 2008), Fredriksson and Johansson (2008), Lechner (2009a), Lechner and Miquel (2010), Fitzenberger et al. (2013a)). This approach is also applied to examine the causal effects of childbirth on married women's labour market outcomes (Lechner (2009b), Fitzenberger et al. (2013b)). For example, Lechner (2009b) demonstrates the sequential propensity score matching estimates of the effects of the timing and spacing of childbirth on female labour market outcomes, while Fitzenberger et al. (2013b) estimate the timing of the first birth on future employment status. On the contrary, the econometric approach aims to build a dynamic treatment model based on traditional discrete choice and duration models (Abbring and van den Berg (2003), Heckman and Navarro (2007), Abbring and Heckman (2007), Heckman et al. (2016)).

In this study, we follow Lecher's (2008) sequential matching approach to analyse the effects of the timing of the childbirth on female labour force participation. As explained in the Introduction, because our conceptual framework, which suggests that married women's preferences confound the causal effects of the treatment (i.e. childbirth) on employment outcomes (i.e. labour force participation of regular and non-regular workers), is borrowed from epidemiology, it has high compatibility with the matching method.

3 Empirical Model

Because our interest lies in the effects of the timing of the childbirth on employment outcomes, it is appropriate to use the woman's age as an index of time period t.We consider childbirth at age t as treatment S_t and the potential outcomes are defined as $\{Y_t^0, Y_t^1\}$, where Y_t^1 represents the labour market outcomes of married women who experience childbirth at age t and Y_t^0 represents the outcomes of the untreated population.

Our goal is to estimate the following average treatment effects:

$\theta_t(\underline{S}_{t-1}, X_t) = E(Y_t^1 \mid \underline{S}_{t-1}, X_t) - E(Y_t^0 \mid \underline{S}_{t-1}, X_t)$, where $\underline{S}_{t-1} = \{S_{t-1}, \ldots, S_1, S_0\}$ represents the treatment history.

Lechner's (2008) sequential matching estimator is based on propensity score matching. The peculiarity of sequential matching is the inclusion of past treatment in the conditioning set. Indeed, to estimate the propensity score by using a logit model, the past treatment variables are included as well as the socio-economic characteristics.

Suppose that there is an initial period and two subsequent periods (period 1 and period 2) in which childbirths occur. To calculate the matching estimator of the treatment in period 1, we estimate the propensity scores $p(S_1 = 1 | S_0, X_1)$ and $p(S_1 = 0 | S_0, X_1)$ and compute $E(Y_1^1 | S_0, X_1) - E(Y_1^0 | S_0, X_1)$ for the matched sample. Similarly, to calculate the matching estimator of the treatment in period 2, we estimate the propensity scores $p(S_2 = 1 | S_1, S_0, X_2)$ and $p(S_2 = 0 | S_1, S_0, X_2)$ and compute $E(Y_2^1 | S_1, S_0, X_2) - E(Y_2^0 | S_1, S_0, X_2)$ for the matched sample.

To identify the treatment effects, the childbirth is assumed to be randomly distributed conditional on the socio-economic characteristics and past treatment; hence, the dynamic conditional independence assumption (DCIA) holds.[4] Our approach to address the DCIA is explained in the next section.

4 Data

We use a pooled sample taken from the Japanese Panel Survey of Consumers (JSPC). This survey consists of Japanese women who belong to four cohorts:

- Cohort A: 1500 women 24–34 years in 1993;
- Cohort B: 500 women 24–27 years in 1997;
- Cohort C: 836 women 24–29 years in 2003; and
- Cohort D: 636 women 24–28 years in 2008.

We use the sample of married couples surveyed from 1993 to 2012. Because the time lag between planning to have a child and childbirth is normally over a year (see Lechner (2009b)), we define the time period unit as two years. As time is indexed by age, we therefore use the sample from period 0 (26–27 years) to period 3 (32–33 years; Table 1). The baseline sample is married women 26–27 years. First, we merge the baseline sample with the sample of 28–29 years (merged data set: *sample 26–29*) based on individual ID. Next, by merging *sample 26–29* with the sample of 30–31 years, we create the sample (merged data set: *sample 26–31*) used to estimate effects of birth at 30–31 years. Lastly, we merge *sample 26–31* with the sample of 32–33 years (merged data set: *sample 26–33*)

[4] Lechner and Miquel (2001) show that if the potential outcomes $\{Y_t^0, Y_t^1\}$ are independent of treatment S_t conditional on the history of treatment $\underline{S}_{t-1}$ and the characteristics including the confounding variables X_t, the weak DCIA holds. This allows us to identify the average treatment effects. See also Fitzenberger et al. (2013a).

to estimate the effects of birth at 32–33 years.

The fertility (treatment) variable is defined as the occurrence of childbirth (treatment = 1 if a child is born within two years). Crucially, whereas employment outcomes are the information as of September in the survey year, the fertility variable is an event that occurred during the past year. Consider the case of the effects of birth at 28–29 years on the employment outcomes at 28–29 years. The effects are split into three parts: (i) the effects of birth from October at 27 years to September at 28 years on the employment outcomes in September at 28 years, (ii) the effects of birth from October at 27 years to September at 28 years on the employment outcomes in September at 29 years, and (iii) the effects of birth from October at 28 years to September at 29 years on the employment outcomes in September at 29 years. In cases (i) and (iii), the periods from childbirth to employment outcomes are at most one year; on the contrary, the maximum length is two years in case (ii).

Further, because until 2005, only regular workers were eligible for parental leave in Japan, most mothers in non-regular employment find it difficult to continue working after childbirth in case (i) and (iii). As a result, the negative effects of childbirth on employment outcomes are expected to be more obvious for non-regular working mothers than for regular working mothers. Hence, we use three employment outcome variables: *Participation* to represent working for both years, *Regular* to represent being a regular employee for both years, and *Non-regular* to represent being a non-regular employee for both years.

As the DCIA holds, all the covariates that affect the treatment assignment and potential outcomes should be included as covariates to calculate the propensity score. Many earlier studies made the DCIA by controlling for a variety of socio-economic characteristics (see Fitzenberger (2013b)). By contrast, we try to identify and control for the confounding variables explicitly by considering the unobserved preference for having a child as a key variable for assuming the DCIA. The JSPC survey includes the question, “Do you want (more) children in the future?” Based on the answers to the three choices to this question, we define the dummy variable for the preference for having a child.[5]

The other covariates include her and her husband’s real annual income, co-residence with parents (living with her or her husband’s parents = 1), utilization of parental leave (if there is a parental leave system in the wife’s company and she is eligible to use it = 1), hours that her husband spends on housework or childcare per week, educational

[5] The three choices to the questions are as follows: three choices: “I do want”, “It depends on the condition”, and “No, I don’t”. In this paper, we define the dummy variable for answered the first choice as a proxy variable of preference for having a child.

attainment (junior high school, high school, special school or special training college, junior college, university), and non-regular work experience post-school (if she experienced part-time or temporary employment or joblessness after graduating = 1).

To avoid the endogeneity of childbirth, we use the one-period lagged value of the covariates that are assumed to influence childbirth. In the sequential matching approach, past treatment sequences are allowed as explanatory variables to predict the current treatment assignment. Thus, we include treatment history to estimate the propensity score. For example, to predict the treatment assignment of birth at 30–31 years, the dummy variable of childbirth at 28–29 years is included as a covariate. Similarly, in the case of birth at 32–33 years, the dummy variables of childbirth at 28–29 years and 30–31 years are included.

5 Empirical Results

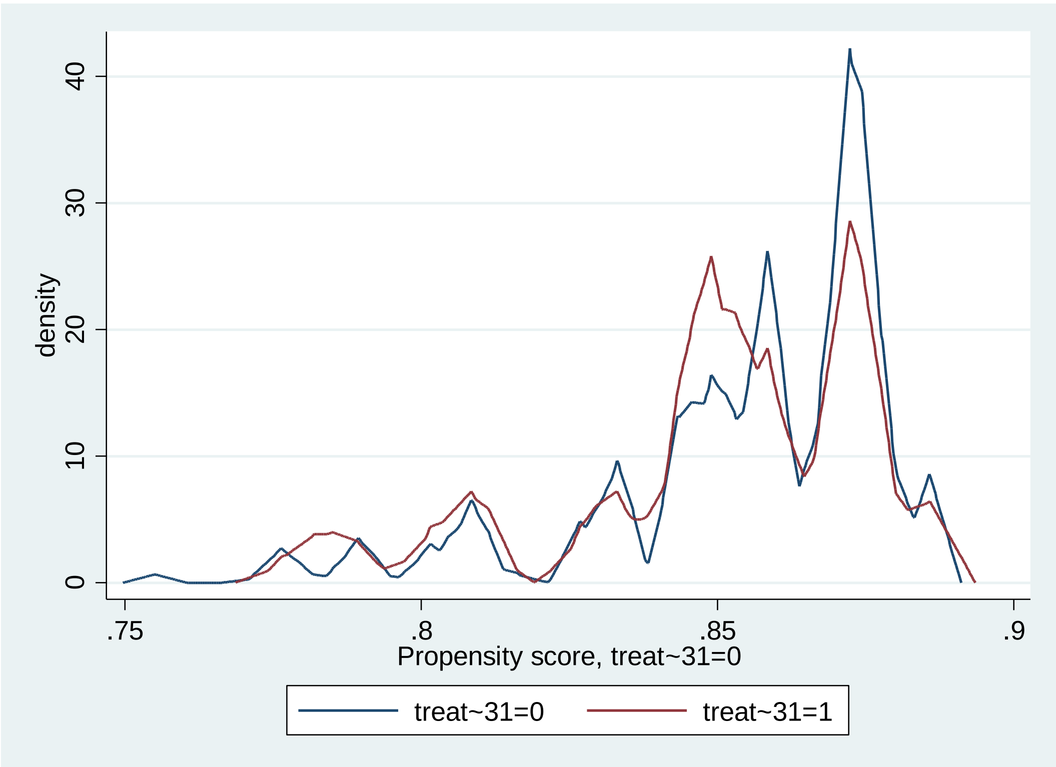
Table 2 shows the characteristics of the treatment and control groups for each subsample. In all age groups, the participation ratio (i.e. employed as either a regular or a non-regular worker) is higher in the control groups. By contrast, average income is higher in the treatment groups, especially for childbirth at 32–33 years. For the fertility-related variables, the results on the hours of the husband's housework are mixed. Contrary to our expectations, the ratio of living with parents is higher in the control groups.

Parental leave availability is higher in the treatment groups regardless of childbearing age. The average number of children of 0–2 years (3–5 years and 6–17 years) is higher in the treatment (control) groups. The higher educational attainment is more distributed for the treatment groups. Moreover, while the results of non-regular work experience are mixed, the preference for children is, as expected, higher in the treatment groups. Finally, the ratio of past childbirth in the treatment groups exceeds that in the control groups in all cases. These results suggest state dependency in childbearing behaviour.

The properties of propensity score matching should be evaluated by using a balance of covariates and an overlap of the common support region between the treatment and control subsamples. For the former, comparing the standardized differences in the mean and variance ratios between groups has been suggested (Rosembaum and Rubin (1985), Austin (2009)). We thus examine different combinations of the baseline covariates, including the cross-terms with the preference for childbearing/rearing variable until the standardized differences in the mean and variance ratios of all covariates are close to zero and one, respectively.[6]

Further, to avoid no overlap in the covariate distribution between the treatment and control subsamples, we follow the suggestion of Crump et al. (2009) to discard the units with estimated propensity scores outside [0.1, 0.9]. As shown in Table 3, the standardized differences in means range from -0.038 to 0 and standardized differences in variance range from 0.941 to 1.023. Notably, the preference for children as well as its cross-terms with the other variables are included in all matching.[7] Finally, Figures 4 and 5 show the estimated propensity scores of both groups for each matching pair. In all cases, the probability mass mostly overlaps with each other, thus indicating that the overlap assumption is not violated.

Figure 4. Overlap of propensity score, treatment at 30–31 years



[6] We follow Austin's (2011) approach that includes the cross-terms as covariates.
[7] We also estimate the model that includes past employment outcomes as a control variable, but find no combination of covariates that can balance past employment outcomes.

Figure 5. Overlap of propensity score, treatment at 32–33 years

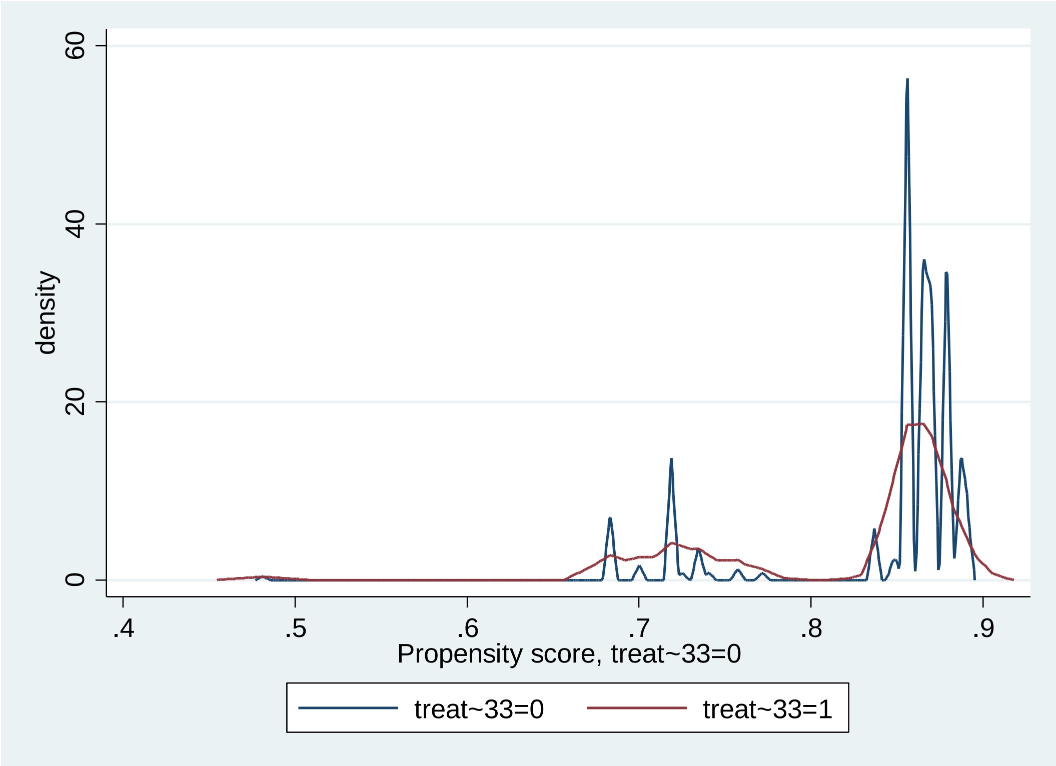


Table 4 shows the propensity score matching results.[8] We estimate the effects of childbirth on the next period (two years later) as well as on the current period. The effects on the current period are significantly negative in all cases. Contrary to our expectations, these negative effects are larger for regular workers than for non-regular workers. Because we adopt a matching approach that explicitly considers the sequentially confounding factors between the treatment and outcome variables, this might remove the spurious causation between childbirth and employment outcomes observed in macro-level data. Further, comparing the effects of delaying childbirth from 30–31 years to 32–33 years, while the negative effects of childbirth on regular work do increase by delaying the age at childbirth, these negative effects for non-regular workers decrease marginally with the age at childbirth.

The result that delayed childbirth leads to a lower participation in regular work is partly explained by the income effects of labour supply. As the husband's real annual income in the treatment group is higher than that in the control group for the 32–33 years group (see Table 2), the higher household income in the treatment group may decrease the labour supply of mothers. The difference between regular and non-regular work is especially

[8] To estimate the treatment effects, we use the *teffects* command in STATA.

prominent in the effects on the next period. Table 4 also shows the effects of childbirth at 30–31 years on the employment outcomes at 32–33 years as well as the effects of childbirth at 32–33 years on the employment outcomes at 34–35 years. This table shows that childbirth does not affect the probability of choosing non-regular work in the next period regardless of childbearing age. By contrast, whereas the negative effect of childbirth at 30–31 years on the probability of choosing regular work at 32–33 years is not significant, the negative effect of childbirth at 32–33 years does not clear away at 34–35 years.

A likely explanation for this difference in the effects on the next period between regular and non-regular work is the difference in the reservation wage when re-entering the labour force after childbearing/rearing. In general, the reservation wage is higher for regular employees than for non-regular employees. If the decrease in productivity in comparison with the reservation wage during the childbearing/rearing period is higher for regular workers than for non-regular workers, re-entering the labour force becomes difficult, especially for regular employees.

6 Conclusion

In this study, we estimated the effects of childbirth on female labour supply by using Japanese data. The novel contributions of our study are twofold. Firstly, we included the effects of unobserved preferences toward female labour supply in a dynamic treatment model, finding that unobserved preferences affect childbirth as well as labour force participation and that unobserved preferences and childbirth are sequentially correlated. Earlier studies that adopted the dynamic treatment approach did not model the confounding factors between the treatment and outcome variables explicitly. Secondly, we applied the sequential matching approach to analyse the causal effects of childbirth on female labour market outcomes. Few studies have applied the dynamical matching method to examine female labour supply.

The estimated results show that childbirth decreases current employment outcomes (participation in regular and non-regular work). Contrary to our expectations, the size of that decrease is larger for regular employees than for non-regular employees. On the timing of childbirth, while the negative effects of childbirth on regular employment are increased by delaying the age at childbirth, these negative effects on non-regular work decrease marginally by delaying. Moreover, on future employment outcomes, childbirth does not affect the probability of choosing non-regular work in the next period regardless of childbearing age. By contrast, the negative effect of childbirth at 32–33 years on the probability of choosing regular work at 34–35 years is significantly negative, whereas the negative effect of childbirth at 30–31 years on the probability of choosing regular work at 32–33 years is not significant. When we focus on the results for regular employees, delaying childbirth degrades not only current but also future employment opportunities. This micro-level evidence thus concurs with the macro-level evidence discussed in the Introduction but is at odds with the findings of previous studies that delayed childbirth leads to higher labour market outcomes (Miller (2011), Fitzenberger et al. (2013b)).

These results suggest ex post and ex ante policy implications. The ex post policy implication is that family policy must depend on the timing of childbirth, as this is an important factor in supporting the job continuity of working women. As Yamaguchi (2017) summarizes, earlier childcare and parental leave policies in Japan have not necessarily increased mothers' employment opportunities (Asai (2015), Asai et al. (2015), Yamaguchi (2016)). If there are mismatches between employment opportunities and childbearing/rearing mothers who differ in the timing of their childbirth, this might produce inefficient policy. Based on our empirical results, family policy should thus be intensified, especially for mothers that delay their age at childbirth.

The ex ante policy implication is based on the dynamic treatment regime. The significance of this regime in epidemiology is helping the doctor decide on the treatment timing for patients. Similarly, the insight into the effects of timing on current and future employment outcomes helps working women decide when to start a family. However, because our results are based on the average effect and have limited information about individual mothers, further research with more fertility-related confounding variables and an estimation method that incorporates heterogeneous treatment effects is recommended.

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Table 1

Time period for the analysis

Age	26–27	28–29	30–31	32–33
Period	0	1	2	3

Table 2

Characteristics of the Treatment and Control Samples

	Age of Birth			
	30-31		32-33	
	Treatment	Control	Treatment	Control
Outcome variables:				
Participation	0.078	0.225	0.081	0.336
Employed as regular worker	0.032	0.116	0.029	0.172
Employed as non-regular worker	0.028	0.082	0.037	0.127
Real annual income:				
Wife	6.716	6.529	6.807	5.384
Husband	11.132	11.439	10.285	9.591
Fertility related variables:				
Hours of husband's housework	438.372	618.341	468.950	447.556
Living with parents	0.226	0.311	0.234	0.299
Parent leave availability	0.296	0.236	0.280	0.228
Number of Children				
0-2 years old	0.371	0.292	0.402	0.302
3-5 years old	0.182	0.239	0.243	0.362
6-17 years old	0.094	0.136	0.131	0.315
Education:				
Junior high school	0.025	0.044	0.019	0.036
High school	0.245	0.353	0.262	0.344
Special school	0.201	0.196	0.159	0.187
Junior college	0.239	0.171	0.299	0.226
University	0.283	0.217	0.215	0.186
Graduate school	0.000	0.013	0.028	0.012
Non-regular work experience post school	0.176	0.190	0.196	0.151
Preference for children	0.692	0.496	0.654	0.404

Birth at aged 28-29	0.115	0.098	0.147	0.077
Birth at aged 30-31			0.221	0.114
Sample size	218	1914	136	885

Table 3
Balance Diagnostics

Age of First Birth: 30−31	Standardized Difference of the Mean	Variance Ratio
Treatment at aged 28-29	-0.017	0.966
Parent leave	-0.006	0.997
High School	-0.028	0.951
Junior College	0.017	1.023
Non-regular work experience	-0.038	0.941
Age	0.000	1.000
* Preference for Children		

Age of First Birth: 32-33	Standardized Difference of the Mean	Variance Ratio
Treatment at aged 28-29	-0.012	0.958
Age	0.000	1.000
Living with parents	-0.007	0.993
Parent leave	0.000	1.000
Preference for Children	0.000	1.000
Number of children aged 0-2	0.000	1.000
* Preference for Children		

Table 4

The Effects of Birth on Labour Market Outcomes

Dependent Variable		30-31	32-33
Participation	Current period	-0.200	-0.229
		(0.039)	(0.035)
	Next period	-0.031	-0.139
		(0.049)	(0.067)
Regular Work	Current period	-0.117	-0.162
		(0.031)	(0.024)
	Next period	-0.039	-0.104
		(0.031)	(0.035)
Non-regular Work	Current period	-0.066	-0.057
		(0.018)	(0.024)
	Next period	-0.020	-0.039
		(0.023)	(0.057)

Note. Standard errors in parentheses. Current period and next period represent the effects of childbirth on the employment outcomes in the period of childbirth and in the next period, respectively.

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