On Leave or Out of a Job?
The Contract Divide in Employment around Childbirth in Japan

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28 October 2026

Abstract

In Japan, most women stop working around childbirth, whatever their contract. Being out of work, however, can mean two very different things: a woman on leave still has a job to return to, while a woman who has lost her job must look for a new one. Most studies of Japanese child penalties measure earnings or employment in ways that cannot tell these two situations apart. Using the Keio Household Panel Survey and the Japan Household Panel Survey from 2004 to 2022, I distinguish the two and follow women through the years after the birth. Regular and non-regular workers stop working at similar rates, but regular workers are mostly on leave, whereas non-regular workers are about 42 percentage points more likely to have lost their job. Women who kept a job are more often back at work in later years, and within couples, mothers account for almost all of the fall in employment. A similar drop in work at childbirth can thus hide very different positions in the labour market, and a count of women in work misses much of what follows for their careers.

JEL codes: J13, J16, J21, J22, J41.

Keywords: childbirth; employment attachment; non-regular employment; parental leave; Japan.

*University College Dublin. I thank Professor Midori Wakabayashi (Tohoku University) for guidance and for facilitating access to the KHPS/JHPS data, and Naoko Caheny for discussions on women's labour-market conditions in Japan. The KHPS/JHPS data are provided by the Panel Data Research Center at Keio University. All errors are my own.

1 Introduction

Japan presents a puzzle. Over the past three decades, successive governments have expanded work-family policies, parental-leave provision and childcare capacity. Women's employment has increased, but continuity through motherhood remains uneven across the labour market (OECD, 2024; Yamaguchi, 2017). The historical pattern of withdrawal during the childbearing years followed by later re-entry is tied to the organisation of employment and family life (Brinton, 1993). The question is how the expansion of formal support relates to the very different employment positions of women approaching childbirth.

A large literature studies the labour-market consequences of childbirth for women. The child-penalty framework documents the timing and persistence of differences in employment and earnings between mothers and fathers (Kleven et al., 2019). In Japan, the regular/non-regular divide adds an important dimension to this question. An interruption in paid work can occur while a woman retains a job, or it can involve the loss of her employment relationship. The same immediate absence from work may therefore describe very different positions in the labour market. The distinction matters for interpreting the employment decline around childbirth and for understanding the subsequent recovery.

In this paper, I examine employment around childbirth as a trajectory patterned by institutions and influenced in part by women's positions in the labour market before the transition. By labour-market position, I mean regular or non-regular employment and the conditions of job continuity associated with it. I use the Keio Household Panel Survey (KHPS) and Japan Household Panel Survey (JHPS), with observations through 2022, to distinguish current work, temporary absence with attachment, and non-attachment. I then follow employment after attached absence and after attachment loss, before considering the allocation of the immediate adjustment between partners in households with both partners observed. Throughout, I emphasise the magnitude and composition of employment change. I compare employment states among women who all had a job in the year before the birth report, without treating contract status as if it were assigned at random, while examining the sensitivity of the contract differences to the available job characteristics and to the timing of measurement.

My event-time sample contains 4,480 observations for 627 women, aligned to the survey year of the birth report. Retrospective and roster information allows me to exclude some later births, but parity remains unresolved for part of the sample. I therefore describe these as birth-report events, since some may be later biological births. Relative to two years before the report, I find that calendar-year-adjusted current work is 68.6 percentage points lower in the year of the report, while attachment is 37.1 points lower. Both outcomes recover in part over the following five years but remain below their reference positions.

The distinction between the two outcomes becomes clearest when I compare women by their contract before the birth. Among 319 women with a regular or non-regular job at $t = -1$, current work at $t = 0$ is uncommon in both groups, at 11.5% for regular and 13.3% for non-regular workers. Their employment relationships are very different. Among regular workers, 73.8% are absent with attachment and 14.7% have lost attachment. Among non-regular workers, the corresponding shares are 28.1% and 58.6%. After standardising over the same employer-size distribution, I estimate an attachment-loss gap of 42.4 percentage points (95% confidence interval 32.2–52.6). Employer size remains relevant to the institutional discussion, but my sample is too

small to measure its separate role with precision.

Later employment also depends on the position from which a woman starts. Five years after the report, I observe current work for 76.6% of the women absent with attachment in the year of the report and 45.1% of those who had lost attachment in that year, among the women still observed at that horizon. Among women in work, adjusted hours also remain below their level two years before the report. In a paired comparison of 539 households, I find that mothers account for most of the immediate net change in employment.

My contribution is to treat these employment states together, within a common measurement framework. Recent Japanese studies already document large maternal earnings losses (Fukai and Kondo, 2025; Kikuchi, 2026) and much larger employment declines among women in non-regular jobs before the birth (Ozawa, 2020; Kikuchi, 2026). I add to them by separating current work from attachment and by following the two groups of non-working women over time. My survey sample is smaller than administrative records or the largest panels, so my estimates are less precise. In exchange, I observe in the survey whether a woman retains an employment relationship during the interruption.

The institutional discussion is part of that interpretation. Leave eligibility, employer practices, childcare, fiscal incentives and the division of care within the household affect the alternatives available to women. I use these institutional features to place the observed employment differences in their economic and social setting.

I first describe Japan's institutional environment and the related evidence. I then present the data and empirical approach, the work and attachment profiles, the contract comparison, subsequent employment and hours, and household adjustment. In the discussion, I return to the implications for employment continuity, re-entry and policy, together with the limits of the evidence.

2 Institutional context

Several features of the institutional environment affect women's employment around motherhood: a labour market divided into distinct employment categories, earnings thresholds for secondary earners, a household division of labour with care concentrated on mothers, and a work-family policy regime operating through existing employment relationships. These features concern both the immediate interruption and the opportunities available afterwards. Their relevance also changes as children grow older and the household replaces childcare leave with other care, such as a nursery, a kindergarten or help from grandparents.

2.1 The regular/non-regular divide

Japan's labour market is organised around a distinction between regular employees (*seishain*) and non-regular employees (*hi-seiki*). Regular employment is associated with long employment relationships and internal careers, including firm-specific training (Kambayashi and Kato, 2017). Non-regular employment encompasses part-time, fixed-term and dispatched arrangements, with different expectations about tenure and progression (Gordon, 2017). The historical organisation of education, employment and family responsibilities has contributed to women's concentration in jobs with weaker career prospects (Brinton, 1993). In its 2024 survey of Japan, the OECD

reports that non-regular workers account for 55% of female employees (OECD, 2024).

The employment category "part-time" (*pāto*) should be distinguished from a classification based on weekly hours. Regular workers may work reduced hours, while some non-regular workers have long schedules. Throughout the paper, I use regular and non-regular to refer to the survey's employment categories and examine hours as a separate outcome. Fixed-term employment is one part of the non-regular category, so legal rules for fixed-term workers do not apply to every non-regular woman in the sample.

This distinction is important for employment continuity because work-family arrangements operate within jobs. A woman with a retained position can be absent from work while remaining connected to an employer. A woman without such a position faces a job-search problem as well as a childcare problem. In an employment system with substantial rewards to continuity, these positions can have consequences beyond the number of months spent away from work. They may also affect a woman's access to training and her later chances of promotion.

A woman can have a legal right to childcare leave and still find it hard to use. To take leave, she must first meet the eligibility rules, and on a fixed-term contract her right also depends on the employer renewing the contract. Even with leave and a benefit, she returns only if her job is still open to her and she has arranged care for the child. Employers also decide how leave works in practice. In interviews with human resource managers at 25 large Japanese firms, Brinton and Mun (2016) find that managers judge women's use of parental leave against norms of the ideal employee in internal labour markets and of the gendered division of care. I use the regular/non-regular indicator as a summary of a woman's position in this setting. It does not measure her statutory eligibility or the accommodation offered by her employer.

2.2 Access to leave, from the law to the workplace

Japan's work-family policy regime has expanded through successive reforms since the 1990s, with the aim of reducing employment discontinuity around childbirth. The 2004 amendment to the Child Care and Family Care Leave Act, in force from April 2005, extended childcare-leave eligibility to qualifying fixed-term workers (MHLW, n.d.; NIPSSR, 2024). From April 2005 to December 2016, a fixed-term worker was eligible only after at least one year's continuous employment with the same employer, with employment expected to continue beyond the child's first birthday and no clear prospect that the contract would end without renewal before the child's second birthday (MHLW, n.d.). Workers on indefinite contracts faced no statutory tenure condition, although a labour–management agreement could exclude those with less than one year's service. During this period, coverage therefore depended on contract type and tenure.

From January 2017, a fixed-term worker needed at least one year's continuous employment with the same employer, and it could not be clear that the contract, including renewals, would end before the child reached 18 months (MHLW, 2016a). In April 2022, the statutory one-year tenure requirement was removed for fixed-term workers, who are now treated like other employees: a labour–management agreement can still exclude any worker with less than one year's service (MHLW, 2022). These dates are important for interpreting my estimates. Because the panels end with the 2022 survey and each birth report needs potential observation afterwards, my estimates describe the earlier institutional conditions. They cannot evaluate the April 2022 relaxation.

Smaller employers may face tighter staffing margins and fewer replacement possibilities during an extended absence. Larger employers may have more scope to redistribute tasks or provide alternative schedules. For these reasons, I examine employer size alongside employment status, although the availability and use of particular arrangements remain empirical questions. A large employer is not a guarantee of accommodation, and a small employer need not be unable to retain a worker. The distinction between formal arrangements and everyday organisational practices is central to the Japanese workplace evidence discussed by Nemoto (2016, ch. 1, pp. 19–22).

In the harmonised survey measure, I distinguish employers with fewer than 100 employees from larger employers and government. This statistical classification does not reproduce every legal threshold. The reduced-hours mandate, for example, applied from June 2010 to employers with more than 100 employees and only from July 2012 to smaller employers (Nagase, 2014). I use size as a limited composition adjustment and a secondary source of heterogeneity, without exploiting a legal size threshold as an instrument or a reform discontinuity.

2.3 Finding childcare when leave ends

Childcare is relevant both to maintaining an existing job and to entering a new one. The end of leave need not coincide with an available nursery place, and a place must be compatible with working hours and travel time. A household may therefore have an employment relationship and a formal entitlement to leave while still facing difficulty arranging the transition back to work. A mother without a job faces the additional problem of coordinating a search for work with access to care.

The reform evidence is mixed because the policies and populations differ. In Asai (2015), the 2001 rise in the leave-benefit replacement rate from 25% to 40%, with leave duration unchanged, produced little evidence of improved job continuity. Using municipality-level data for 2000–2010, Nishitateno and Shikata (2017) attribute part of the increase in maternal employment to improved daycare accessibility, but find a smaller employment response than the increase in nursery enrolment. Their interpretation allows for substitution between care arrangements, with mothers already in work switching from kindergartens to licensed day care. By contrast, Fukai and Kondo (2024) study families seeking accredited childcare for children under two in a municipality with excess demand. Their assignment-based estimates show large employment effects for borderline applicants, most of them mothers returning from childcare leave in households where both parents work full-time. This difference in design is important when I interpret the role of care in a broad household-panel sample.

A practical issue is timing and affordability of care by child age. Nursery-type settings (*hoikuen*) and certified centres are relevant to the earliest years, while kindergarten (*yochien*) and integrated provision form part of the options at older preschool ages. The October 2019 fee reform covered eligible provision for children aged three to five and children aged zero to two in households exempt from municipal residence tax (Children and Families Agency, n.d.). Unlicensed provision is covered only up to monthly ceilings, and meal costs are excluded. This reform changed financial support during the observation period; it did not eliminate differences in availability, opening hours or ancillary costs.

The conditions facing mothers change as the child grows, including the availability and cost of care and the feasibility of sustained employment. For this reason, I examine later employment

alongside the immediate interruption.

2.4 Earnings thresholds for married women

Japan's tax and social-insurance rules also create earnings thresholds for the second earner in a married couple. These rules affect married women's employment whether or not they have children. Before 2018, a married woman with a salary of up to 1.03 million yen paid no income tax on it, and her spouse could claim the full spousal deduction.[1] Above that level, the household could claim a special spousal deduction, which phased out at about 1.41 million yen. Many employers also used the 1.03 million yen limit for their spousal allowances. From 2018, the full deduction applies up to a spouse's salary of 1.5 million yen and phases out at about 2.01 million yen, with smaller deductions for primary earners with total income above 9 million yen (National Tax Agency, 2017). Since then, the limit for a woman's own income tax and the limit for the household's spousal deduction have been two different thresholds (OECD, 2024).

Social insurance adds two further limits. A spouse earning less than 1.3 million yen a year can remain covered as a dependant under the partner's insurance. Since October 2016, a worker at a firm with more than 500 employees comes under employee insurance once her monthly earnings reach 88,000 yen, about 1.06 million yen a year (MHLW, 2016b). Whether this rule applies to a particular woman also depends on her weekly hours and on the rules in force in that year. Because the thresholds serve different purposes, they are not interchangeable cut-offs. Each of them can still reduce the gain from working more hours or from moving to a different type of job (OECD, 2024; Kitao and Mikoshiba, 2022).

In interpreting re-entry, I do not treat labour-market segmentation and fiscal incentives as competing explanations. A mother considering reduced-hours work faces both the jobs available to her and the effect of additional earnings on household resources. If she cannot find a regular-track position, a non-regular job may be her feasible option. Within that job, taxes and contributions may then influence her choice of hours. Kitao and Mikoshiba (2022) examine these connections using evidence from the Japanese Panel Survey of Consumers (JPSC) and a structural life-cycle model.[2] Their policy simulations suggest that changes to fiscal incentives and employment type may need to be considered together.

For my analysis, the relevant distinction is between participation and the terms of participation. A return to employment can coexist with fewer hours and a different career path. To estimate bunching at these thresholds, I would need precise annual earnings for many women close to each limit, and my sample, with a few hundred working mothers at each horizon, is too small for that. I therefore treat the thresholds as part of the economic setting of women's later choices about work, without attributing an individual woman's choice of hours to a threshold.

[1] At the 2017 average exchange rate of 112.1 yen per US dollar (Federal Reserve Board, 2026), 1.03 million yen is about $9,200, 1.06 million yen about $9,500, 1.3 million yen about $11,600, 1.5 million yen about $13,400, 2.01 million yen about $17,900 and 88,000 yen about $790.

[2] The JPSC began in 1993 with 1,500 women aged 24 to 34 and their spouses, and has interviewed them every year since, adding new cohorts in 1997, 2003, 2008 and 2013. The Panel Data Research Center at Keio University, which also runs the KHPS and JHPS, now conducts it.

2.5 The corporate warrior and the gendered household

The figure of the *salaryman* or corporate warrior remains part of the discussion of Japanese corporate life. Expectations of long hours, availability and commitment to the employer can conflict with the daily organisation of care. Nemoto (2016) examines how organisational practices sustain gender inequality despite formal opportunities for advancement. Within households, North (2009) finds continuing inequalities in family work even among dual-income couples with young children. These studies place employment decisions within workplace and family relationships.

When a husband works long hours, the couple has less room to share care after the birth. If one partner works 50 or more hours in a typical week, most of the new care tasks fall to the other partner, to paid care or to relatives. Once this division of labour is in place, a couple may find it hard to change, even if both partners would prefer a different arrangement. I treat this as a possible explanation for the uneven adjustment between mothers and partners in Section 7. The paired comparison shows which partner changes employment, but it does not compare couples by the husband's working hours, so it cannot show that long hours cause the mother's adjustment.

Help from grandparents may ease the pressure on childcare, but living with a grandparent and receiving care from one are not the same. In a shared household, I observe neither the amount nor the timing of a grandparent's help, nor whether the family chose to live together because of the mother's job. Support from relatives outside the household raises the same difficulty. With a small household panel, weak evidence of differences by a coarse family characteristic would therefore not show that family support is unimportant.

A father can also take leave or reduce his hours, which widens the household's options. Since October 2022, fathers can take up to four weeks of childcare leave at birth within eight weeks of the birth (*shusshōji ikuji kyūgyō*, known as *sango papa ikukyū*) (MHLW, 2022). This change came after the births in my sample. In the paired comparison, I instead compare the net change in employment of mothers and their partners within the same couples. I observe only each partner's employment status, so I cannot measure fathers' leave or their share of unpaid care.

2.6 Marriage as an earlier turning point

Marriage remains a turning point in Japanese women's employment, often earlier than the first birth. A woman may plan her career together with marriage and children. Her job may in turn affect whether and when she marries. Using Japanese life-history data, Piotrowski et al. (2015) find that non-regular work is associated with delayed marriage, above all among men. Their evidence concerns entry into marriage, whereas I study employment changes among mothers already in the panel.

For my purposes, the point is interpretive. A woman's employment may already change in the years before the birth, for reasons linked to marriage or to the coming child. With annual observations, I cannot separate these reasons. A woman's region may also affect this sequence, through local job opportunities and the nearness of family. In a robustness check of the contract gap (Appendix C), I add geographic indicators as coarse controls for location, and they make little difference to the estimate.

Taken together, these institutions lead me to distinguish temporary absence, loss of attachment

and later work. Employment continuity depends on the employment relationship together with arrangements outside it. In the empirical analysis, I follow these employment states across labour-market positions and over time.

3 Related evidence and my contribution

3.1 Child penalties and employment continuity

The international child-penalty literature documents persistent differences in labour-market outcomes following parenthood. In Denmark, Kleven et al. (2019) show how earnings differences develop through participation, hours and wage rates. Their administrative framework provides a benchmark for organising outcomes over time. I use event time in a similar way to organise survey evidence on current work and employment attachment, with additional attention to the employment relationship during an interruption.

The distinction between an interruption in work and an interruption in an employment relationship also has a direct precedent. Baker and Milligan (2008) evaluate Canadian job-protected maternity-leave expansions and distinguish time away from work from return to the pre-birth employer. Longer absence can coexist with greater job continuity. It follows that a larger immediate decline in current work need not mean a larger loss of attachment.

The two concepts are related without being identical. Reported attachment at an annual survey date is weaker evidence than verified continuity with the original employer. A woman can change employers between interviews or have more than one interruption. I therefore use attachment to describe the recorded employment relationship at each annual interview and treat later work as a separate question. With the household panel, I can observe this distinction, although I do not have a complete employer-to-employer history for every woman.

3.2 The closest Japanese studies

Fukai and Kondo (2025) use local tax records to document mothers' and fathers' earnings trajectories around childbirth. They report maternal earnings reductions of around 60–80% in the first two years after a first birth, with earnings remaining about 50–60% below the counterfactual four years later. Because childcare-leave benefits are untaxed, their records cannot separate mothers on leave from mothers without a job. For measuring income, their tax records are a stronger source than a small household survey. Both their results and mine show persistent differences after the immediate birth transition. The outcomes differ, however: annual income combines the amount of work with its rate of pay, whereas I separate current work and attachment at particular survey dates. For this reason, I do not set my percentage-point employment changes against their percentage earnings losses.

Kikuchi (2026) is the study closest to the employment-type question. Using the Japanese Panel Study of Employment Dynamics (JPSED),[3] with supplementary evidence from the same JHPS/KHPS panels used here, Kikuchi examines labour-market adjustment to childcare and eldercare and distinguishes regular and non-regular workers. In his childcare analysis, he uses

[3]The Recruit Works Institute has run the JPSED since 2016. Each January it surveys about 50,000 people aged 15 and over across Japan about their work and living conditions in the previous year.

not-yet-treated comparisons and finds much larger employment declines among non-regular women. Contract heterogeneity in Japan is therefore already documented, so my contribution is more specific. I separate non-working women who keep a job from those who have lost one, then follow both groups over the following years.

The difference in measurement also affects the interpretation of the findings, since similar current-work rates can accompany very different attachment rates. In my main contract comparison, I fix attachment at $t = -1$, measure employment type in that same year and compare mutually exclusive states at $t = 0$. The comparison therefore describes the position of women entering the immediate transition with an employment relationship. Kikuchi instead assigns contract groups by the most frequent employment status in the four years before the birth. With a larger sample and a comparison-group approach, Kikuchi addresses questions beyond my narrower sample. Because of differences in support, event definition and specification, I do not rank the numerical estimates as if they measured the same penalty.

Two further studies use the same household panels. Ozawa (2020) tabulates transitions from one year before to one year after a first birth by pre-birth employment type, counting leave as employment. Among women in non-regular jobs before the birth, 70.4% are not employed after it, compared with 17.1% of those in regular jobs, a gradient of similar direction and size to the attachment-loss difference in my contract comparison. Ozawa (2026) estimates wage functions for married men and women and, with individual effects, finds a motherhood penalty of 8.0% per child in annual labour income, a smaller penalty in hourly wages, and little evidence of a fatherhood premium. To this evidence, I add the distinction between current work and attachment at the birth, measuring contract status in the year before the birth report for every woman in the comparison.

The institutional evaluations address more specific policy questions. Asai (2015) examines the 2001 increase in parental-leave cash benefits and finds little evidence of improved job continuity. Nagase (2014) examines the short-hours mandate and finds a positive response in first childbirth, alongside no statistically significant additional effect on regular-employment continuation in the selected post-birth sample. Fukai and Kondo (2024) links childcare access to employment and earnings through the assignment process for accredited centres. For borderline applicants with children aged zero and one, the preferred estimates of enrolment on mothers' employment, measured as positive salary income, are 40.3 and 18.8 percentage points. Because each of these studies evaluates a different policy and finds a different effect, I treat the policies as separate interventions.

My household-panel evidence complements such evaluations. Within the same survey framework, I observe job characteristics and both adults' employment, so I can relate employment states to their setting. The costs are a smaller sample, imperfect reporting, incomplete parity and changing observation support. For childcare in particular, I have no equivalent source of assignment variation and no complete record of a woman's unsuccessful nursery applications.

3.3 Job quality after the return to work

A woman can return to work without regaining her earlier hours or pay. Goldin (2014) emphasises the organisation and remuneration of work, in particular the rewards to long or particular hours and the cost of temporal flexibility. Goldin's argument links the employment relationship to the

compatibility of work and care. The regular/non-regular distinction adds a different dimension: whether a position remains available during the interruption and the jobs open to her afterwards.

Okuyama et al. (2025) use personnel records from a large Japanese manufacturer to study adjustment among women continuing in their jobs. They show that reductions in time-based pay account for much of the child penalty at first, while job-rank-based pay becomes more important over time. Their promotion evidence concerns career progression within that firm, among regular employees who remain with it. Their evidence shows the value of looking within employment. In the same spirit, I add a measure of work intensity, weekly hours, to the employment profiles.

The concern with labour-market segmentation also has international parallels. In Spain, de Quinto et al. (2021) estimate a child penalty in annual earnings of 28% ten years after the first birth and document adjustments in days worked and moves towards part-time or fixed-term contracts. In Italy, Casarico and Lattanzio (2023) estimate that, relative to women without children, mothers' annual earnings are 52 log points lower in the long run, for the most part because of fewer weeks worked, and document sorting towards firms with lower productivity and wages. Both studies link post-birth employment to the organisation and quality of jobs. Their institutional categories and outcomes differ from mine, so I use them as interpretive comparisons and do not compare effect sizes.

Following these studies, I take a longer view of employment continuity. A woman may resume work while remaining below her earlier hours or outside the employment path open to her before the birth. My observation window ends five years after the birth report, so the later profile describes medium-term employment, with most of the later career unobserved. Within that window, I clarify the employment states at the start of the adjustment and the differences still visible over the available follow-up.

4 Data and empirical approach

4.1 The household panels and the birth sample

I draw on two complementary household panel surveys conducted by the Panel Data Research Center at Keio University. These panels suit my question because they collect employment status, work hours, job characteristics and household information within the same longitudinal framework. Compared with administrative earnings alone, they allow me to consider employment relationships alongside work in a reference period. I access the microdata under the Centre's application and confidentiality procedures. The manuscript source package contains derived tables and figures, with no respondent-level data.

The KHPS began in 2004, with additional cohorts in 2007 and 2012; the JHPS began in 2009. Both surveys are administered by Keio University's Panel Data Research Center and collect employment information for respondents and spouses (Panel Data Research Center, n.d.). I use KHPS waves from 2004 to 2022 and JHPS waves from 2009 to 2022. Each survey retains its own identifier when the panels are combined. To harmonise the panels, I track changes in questionnaire codes as well as variable numbers. For example, the KHPS 2004 employer-size categories differ from the later mapping, so I use the wave-specific classification to identify employers with fewer than 100 employees. I also require employment states to agree across the status and job-response items before treating a wave as determinate.

Although the surveys cover all of Japan, my sample is selected: I keep only births reported while a woman is in the panel, with observations on both sides of the report. I do not use survey weights, because the KHPS and JHPS provide no single longitudinal weight for the pooled panels: their longitudinal weights depend on the combination of cohorts and the final wave. My estimates therefore describe this sample.

I begin with the first birth report observed for each survey respondent and require the respondent's observed panel span to extend on both sides of that report. I also apply a spouse-presence condition within the birth-report history. An observation span crossing the preceding and following calendar years does not guarantee a complete interview in each intervening year. I then remove reports with corroborating evidence of an earlier child from the KHPS retrospective module and the household roster. I cannot run this check for every woman, because the retrospective information is incomplete, in particular outside the relevant KHPS wave. For about two-fifths of the reports I keep, the household roster lists older children, so some of these births may be second or later births. I keep them in the main analysis and exclude them in a conservative check in Appendix D.

The mother whose birth is reported can be a female survey respondent or the spouse of a male respondent. For the latter, I use the spouse section and the household roster to follow the same woman across waves. When spouse information conflicts or I cannot match the woman to the birth report, I treat that wave as unusable. The spouse section is designated for the spouse, but the files do not record the identity of the person completing it. I compare these two routes into the sample in Appendix D.

For the employment profiles, the event window extends from five waves before to five waves after the report. The final models contain 4,480 observations for 627 women with determinate current-work and attachment outcomes. The number of usable observations rises from 109 at $t = -5$ to 623 at $t = 0$, then falls to 411 at $t = +5$. Earlier and later horizons therefore represent different mixtures of women and birth-report cohorts. Because women join the panel in different years and some stop responding before the data end in 2022, I observe each woman for a different number of years. Table 1 lists the populations for the main comparisons.

Table 1: Analysis populations and their observation requirements

Comparison	Women	Requirement
Employment profiles	627	At least one determinate pair of employment outcomes in $t = -5, \ldots, +5$; 4,480 person-waves.
Contract comparison	319	Attached at -1, regular or non-regular wage job observed at -1, determinate state at 0.
Later employment paths	357	Attached at -1 and determinate state at 0, with any type of job.
Paired household changes	539	Both adults' current work and attachment observed at both -1 and 0.
Balanced long window	229	Both maternal outcomes observed at every wave from -2 through $+5$; 1,832 person-waves.

Notes: The samples overlap and are not successive exclusions from a single estimation sample. The pathway figure plots 185 women absent with attachment at $t = 0$ and 118 women without attachment at that wave, among the 357 women in the later-employment row. The remaining 54 women, in current work at $t = 0$, are outside those two plotted groups. Event time zero is the survey wave reporting the birth.

4.2 Measuring work and job attachment

I define current work using the questionnaire's report of work in its reference month. Employment attachment includes current work and temporary absence with a retained job. Among determinate observations, a woman is therefore in one of three mutually exclusive states: currently working, attached but not currently working, or not attached. The last state combines unemployment and labour-force non-participation. It does not tell me whether she left the job herself or her employer ended it.

I check consistency across reported work status, employment form, job position, leave responses and weekly hours. When responses are missing or contradict one another, I treat the wave as indeterminate. A woman counts as working only when she reports work in the reference month, because a woman on leave may still give the usual hours of her job. I therefore exclude attached absentees from the conditional-hours analysis, reported in Appendix F. Attachment at a survey date does not verify uninterrupted retention of the original employer between interviews.

For the main contract comparison, I measure regular or non-regular wage-employment status at $t = -1$, without carrying forward an older job classification when a nearer one is unavailable. I therefore ask a narrow question: among women still attached in the preceding wave, how does attachment loss at the birth-report wave vary with their observed employment status? Women leaving before $t = -1$ are outside this comparison. Since $t = -1$ can overlap pregnancy or earlier planning,[4] I also report a smaller-sample check using contract status at $t = -2$.

4.3 The event-time profiles

For outcome Y_{iy}, I estimate

$$Y_{iy} = \alpha + \sum_{k \in \{-5,\ldots,5\} \setminus \{-2\}} \beta_k \, \mathbf{1}\{y - B_i = k\} + \lambda_y + \varepsilon_{iy}, \tag{1}$$

where B_i is the birth-report survey year and λ_y denotes calendar-year effects. I estimate the equation for current work and for attachment on the common set of determinate observations. The omitted reference is $t = -2$. I report each binary-outcome coefficient in percentage points, with heteroskedasticity-robust intervals in the main figure. Appendix Table A1 adds standard errors clustered by woman to allow for within-woman dependence.

I include no individual effects or age controls. The event-time coefficients therefore describe adjusted differences in the observed sample, combining within-woman changes with compositional differences across horizons. They do not isolate changes caused by childbirth. Without a never-treated or not-yet-treated comparison group, I do not estimate a cohort-specific treatment effect. The calendar-year effects remove differences common to all women in a given year. Within a woman's own history, however, ageing and the birth move together with event time, so the model cannot tell them apart. With both individual and calendar-year effects, the linear trend in event time is not identified, because event time is the calendar year minus the woman's birth-report year. Individual effects would therefore need further restrictions, and they would still give me

[4] The KHPS and JHPS are fielded each year in January and February, and the work questions refer to January. The files do not record each respondent's interview date. A birth first recorded at $t = 0$ took place, in most cases, in the year before that survey, so a woman whose child was born within nine months of the $t = -1$ survey was already pregnant when she answered it.

no untreated comparison group.

Employment has already declined at $t = -1$, so the preceding wave is not a stable pre-birth position. I therefore use $t = -2$ as the reference. With this choice, I move the point of comparison back without dating the start of an effect. The annual report can also lag delivery by different amounts, so $t = 0$ is not an exact delivery date. Later outcomes may include subsequent births.

In balanced-window checks, I keep only women with both outcomes observed at every wave in a specified interval. I compare each balanced estimate with the unbalanced estimate over the same interval, keeping window changes separate from balancing. These checks show the sensitivity of the estimates to the set of women observed but cannot recover employment for women lost from observation.

4.4 Comparing contracts and following later paths

Let $L_i = 1$ if a woman attached at $t = -1$ is not attached at $t = 0$. In the 319-person wage-worker sample, I fit

$$\Pr(L_i = 1 \mid D_i, S_i, M_i) = \Lambda(\gamma_0 + \gamma_D D_i + \gamma_S S_i + \gamma_M M_i), \tag{2}$$

where D_i denotes non-regular employment, S_i denotes a small employer, M_i denotes missing employer size, and Λ is the logistic function. I measure all predictors at $t = -1$. When size is missing, I set the small-employer indicator to zero and keep the observations through the separate missing-size indicator. This adjusts only for differences in employer size between the two groups. Women in regular and non-regular jobs may still differ in other ways, such as education or earlier careers, and these differences may account for part of the gap.

To compare the two groups on the same footing, I predict each woman's probability of losing attachment twice, once as a regular and once as a non-regular worker, keeping her own employer size, and average each prediction over the 319 women:

$$\widehat{p}(d) = \frac{1}{319} \sum_{i=1}^{319} \Lambda(\widehat{\gamma}_0 + \widehat{\gamma}_D d + \widehat{\gamma}_S S_i + \widehat{\gamma}_M M_i), \qquad \widehat{\Delta} = \widehat{p}(1) - \widehat{p}(0). \tag{3}$$

The difference $\widehat{\Delta}$ is the standardised gap. Because both averages use the same women's employer sizes, the gap does not reflect differences in employer size between the two groups.[5]

For later pathways, I calculate horizon-specific work and attachment shares among women observed at each wave, grouped by their state at $t = 0$. For the paired household comparison, I average each adult's change in employment indicators from $t = -1$ to $t = 0$ among households observed at both waves. These unadjusted paired changes use a different population and reference from equation (1).

[5]The standardised probabilities are non-linear functions of the logit coefficients. I obtain their standard errors, and that of $\widehat{\Delta}$, by the delta method, which replaces these functions with a linear approximation around the estimated coefficients. The coefficient covariance matrix is the HC1 heteroskedasticity-robust estimator, which scales White's estimator by $n/(n-k)$ for the small sample.

5 Work interruption and the contract divide

Work interruption is widespread around the birth-report wave. Among women in non-regular jobs, however, it far more often comes with the loss of attachment, even though current work is uncommon among both regular and non-regular workers.

5.1 Out of work, but still attached

Figure 1 uses the broader event-time sample, so the women contributing observations vary across horizons. The contract comparison in Sections 5.2 and 5.3 uses a narrower group of 319 women. The figure shows calendar-year-adjusted changes in current work and attachment relative to $t = -2$. At the birth-report wave, current work is 68.6 percentage points below the reference, whereas attachment is 37.1 points lower. The much larger fall in current work is consistent with a substantial increase in job-attached absence relative to the reference wave. If I measured employment by current work alone, I would combine this absence with attachment loss and lose the distinction between the two.

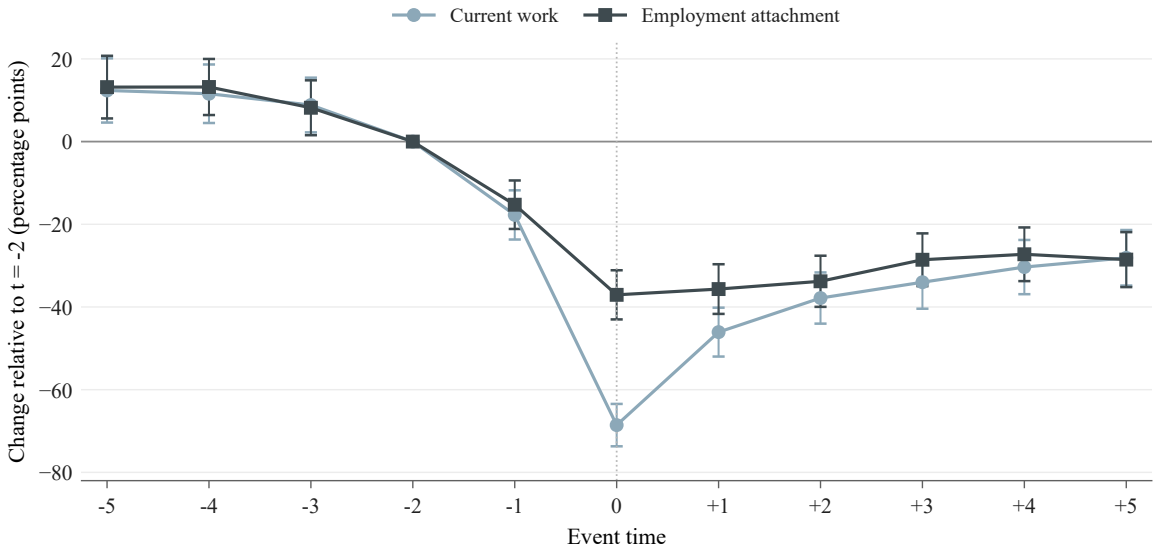


Figure 1: Current work and employment attachment around the birth-report wave

Notes: Points are calendar-year fixed-effect event-study coefficients relative to $t = -2$, with heteroskedasticity-robust 95% confidence intervals. There are no individual fixed effects. Current work is questionnaire-reported paid work; attachment includes verified job-attached temporary absence. The reference coefficient is normalised to zero. Event-time support is unbalanced, so the women contributing observations vary across horizons. The survey wave reporting the birth is $t = 0$.

The two profiles move closer together at later horizons as current work rises more than attachment. At $t = +1$, the respective coefficients are -46.1 and -35.7 percentage points; by $t = +5$, they are -28.1 and -28.5 points. This narrowing is not a return to the reference employment position: both outcomes remain well below their $t = -2$ benchmarks. Nor does the near coincidence of the later coefficients establish identical raw employment levels or the absence of job-attached non-workers. Each series is expressed relative to its own reference, and the coefficients also remove calendar-year differences.

The adjusted employment profile is already below its $t = -2$ reference at $t = -1$. At $t = -1$, current work is 17.7 and attachment 15.3 percentage points below $t = -2$; the earlier coefficients

are above the reference. The profile therefore does not describe a flat pre-transition path followed by an isolated break at childbirth. The preceding survey wave may already overlap pregnancy.

I find the same narrowing when I follow the same women throughout, keeping only those with both outcomes observed at every wave of a window. These balanced samples contain 328 women over $t = -2$ to $+1$, 263 over -2 to $+3$, and 229 over -2 to $+5$. In the longest window, the adjusted current-work and attachment coefficients at $+5$ are -25.4 and -24.7 percentage points. When I estimate their difference as a single coefficient, it is 0.8 points, with a 95% confidence interval of $[-3.2, 4.7]$. This is a difference in changes, not proof that the raw levels coincide: at $+5$, 131 of these 229 women report current work and 144 report attachment. The same-window unbalanced comparison also shows narrowing. Changes in which women are observed therefore do not account for the pattern on their own, although the fixed-sample results do not establish complete recovery or representative long-window support. The longer the window, the fewer women I can keep, and the more they come from earlier birth-report cohorts. I report the details in Appendix B.

5.2 Keeping or losing the job, by contract

Few women in either regular or non-regular jobs before the birth are working at $t = 0$, but far more of the regular workers are still attached. In Figure 2, I focus on the 319 women attached at $t = -1$, with a regular or non-regular wage job at that wave and a determinate employment state at $t = 0$. The comparison therefore covers women entering the transition with an employment relationship, a narrower group than the event-time sample. Regular and non-regular denote employment status, not full-time and part-time hours; non-regular employment is not synonymous with a fixed-term contract.

The figure separates three mutually exclusive states: currently working; attached but not currently working; and not attached. Among the 191 regular workers, 22 are currently working at $t = 0$ (11.5%), 141 are attached but not working (73.8%), and 28 are not attached (14.7%). Among the 128 non-regular workers, the corresponding counts are 17 (13.3%), 36 (28.1%) and 75 (58.6%). The substantive difference lies in the status accompanying non-work: attached absence is the predominant state among regular workers, whereas being without an employment relationship is the predominant state among non-regular workers. These are unadjusted within-group shares. In the broader specification of Appendix D, the non-regular odds ratio is 1.06 for loss of current work among women working at $t = -1$, compared with 7.87 for loss of attachment among women attached at $t = -1$.

To interpret attachment without current work, I use the reasons women give for their temporary absence. In the broader birth-report cohort, 194 of the 205 women attached but not working at $t = 0$, or 94.6%, report childcare leave. This group is larger than the attached absentees in Figure 2, because it also includes women outside the 319-person contract comparison. For most women who keep a job while absent, the absence is therefore childcare leave. The responses support my distinction between leave-related interruption and attachment loss, although leave provision need not be the cause of the difference between contract groups.

The non-regular women reported different considerations about their jobs. Of the 128 women, 117 supplied a valid closest-reason response at $t = -1$ and 11 had missing or unusable responses. Among valid respondents, 45.3% selected favourable wages, working conditions or treatment,

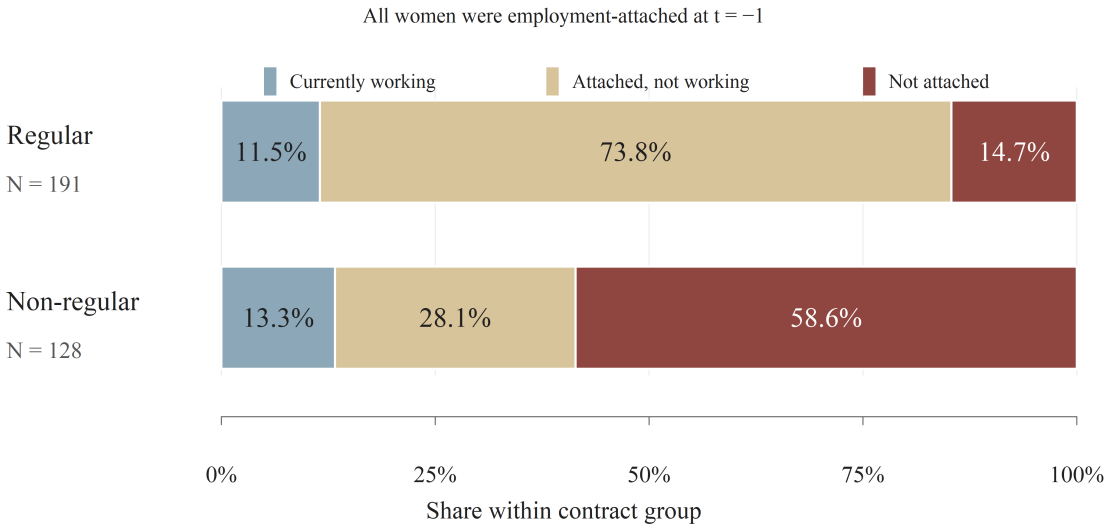


Figure 2: Employment states in the birth-report wave, by pre-transition contract

Notes: The sample comprises 319 women attached at $t = -1$, with a regular or non-regular wage job at that wave and a determinate employment state at $t = 0$. Bars show unadjusted percentages within each pre-transition contract group. Attachment includes current work and verified job-attached temporary absence. The survey wave reporting the birth is $t = 0$.

37.6% selected personal circumstances incompatible with regular-employment conditions, and 8.5% selected wanting regular employment but being unable to find an employer. The remaining 8.5% selected *other*. The full four-category distribution appears in Appendix Table A6. Each woman could give only one closest reason, so the answers offer a partial picture of her preferences and constraints. A woman citing favourable conditions may still have had few alternatives, and personal circumstances may cover more than childcare. These answers describe the women's reasons for holding a non-regular job before the birth.

5.3 The gap after adjusting for employer size

The attachment divide remains large after a limited adjustment for employer size. Table 2 reports standardised attachment-loss probabilities of 15.1% for regular employment and 57.5% for non-regular employment. Their difference is 42.4 percentage points, with a 95% confidence interval of 32.2–52.6 points. I report the probability contrast because it is easier to read than the corresponding odds ratio of 7.80. Non-regular employment is associated with a much higher probability of attachment loss, but the probability itself is not 7.80 times higher.

I fit the logit model of Section 4.4 on the same 319 women, controlling for whether the employer has fewer than 100 employees, with women whose employer size is unknown kept in a separate category.

The gap changes little when I add broad occupation: on the same 319 women, it is 41.1 percentage points (Appendix Table A3). Among women with known employer size, the gap is similar at small employers (42.9 percentage points) and at larger employers or government (43.5 points), but the interval for the difference between them runs from −21.2 to 20.1 points (Appendix C). The differences also remain large when I take contract status from $t = -2$ or include birth-month quarter and reporting lag. These timing checks use smaller samples because the additional information is not available for everyone, so their differences from the baseline

Table 2: Standardised attachment-loss probabilities by pre-transition contract

Quantity	Estimate	95% interval
Regular employment (%)	15.1	[10.0, 20.3]
Non-regular employment (%)	57.5	[48.8, 66.2]
Difference (percentage points)	42.4	[32.2, 52.6]
Non-regular odds ratio	7.80	[4.54, 13.40]
Women	319	

Notes: The outcome is non-attachment at $t = 0$ among women attached at $t = -1$ with a regular or non-regular job at that wave. The logit includes non-regular status, small-employer status (fewer than 100 employees) and missing employer-size information, all measured at $t = -1$. Each probability averages predictions over the same 319 women's employer-size characteristics, setting contract status to regular or non-regular in turn. Probability intervals and the interval for their difference use the delta method with HC1 heteroskedasticity-robust covariance; the odds-ratio interval is formed on the log-odds scale.

combine changes in information or specification with changes in the women included (Appendix Table A3).

The low rate of current work at the birth-report wave accompanies different employment positions. Regular workers are much more often absent with attachment, while non-regular workers are much more often without an employment relationship.

6 Employment in the years after the birth

Women absent with attachment at the birth report are more often observed working in later waves than women who had lost attachment at that wave. Among women in work, reported hours remain below their earlier level.

6.1 Later work after keeping or losing the job

In Figure 3, I follow women with employment attachment at $t = -1$, separating those attached but not currently working at $t = 0$ from those not attached at that wave. The first group contains 185 women and the second 118. They belong to a broader group of 357 women attached at $t = -1$, with any type of job. This population differs from both the full birth-report cohort and the 319-person contract comparison of Sections 5.2 and 5.3. Women currently working at $t = 0$ are outside the two plotted groups. I define the groups by observed employment state, without reference to contract status or a childcare-leave response.

Both groups report no current work at $t = 0$ by construction, yet in later waves current work is much more common among the attached absentees. At $t = +1$, 55.8% of the 181 observed women in that group report current work, compared with 18.1% of the 116 observed attachment losers. At $t = +2$, the respective shares are 75.6% and 25.7%, among 156 and 113 observed women. Two groups with the same initial absence from work thus differ a great deal in their employment over the next two survey waves.

In later waves, I observe both employment among women without attachment at $t = 0$ and a continuing difference between the groups. At $t = +5$, current work is reported by 76.6% of the 107 observed women absent with attachment at $t = 0$ and 45.1% of the 91 observed attachment losers. Among the women without attachment at $t = 0$, a growing share is in work

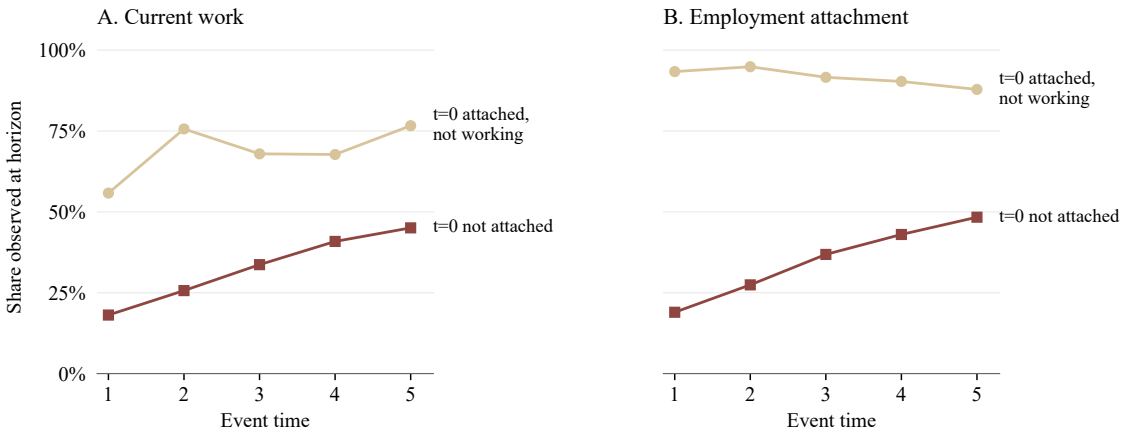


Figure 3: Employment pathways after attached absence and attachment loss

Notes: Women were employment-attached at $t = -1$ and had a determinate state at $t = 0$. At $t = 0$, the two groups contain 185 women attached but not currently working and 118 women not attached, among the 357 women attached at $t = -1$. Women currently working at $t = 0$ are not plotted. Panels show raw shares among women with both employment outcomes observed at each horizon; denominators vary over time. At $+1$ the denominators are 181 and 116, and at $+5$ they are 107 and 91, in the same group order. The survey wave reporting the birth is $t = 0$. These are descriptive employment-state profiles, not cumulative return probabilities or evidence of uninterrupted attachment to the same employer.

at each successive horizon. At the same time, employment in this group remains less common than among the initial attached absentees. The attachment panel shows the corresponding difference in employment relationships, counting temporary absence as well as current work. High later attachment in the first group does not mean that all women continued with their original employer, and attachment at a later wave in the second group need not be their first return.

I observe each woman once a year, so any moves in and out of work or between employers in the months between interviews go unrecorded. The profiles are also unbalanced: the women contributing observations change, and the decline in available observations differs between the two starting groups. Changes in the plotted shares can therefore reflect both employment transitions and changes in composition.

The initial states also reflect the employment positions documented in Section 5.2, where regular workers were more often absent with attachment and non-regular workers more often lost attachment. Because the two groups also differ in their earlier jobs and circumstances, the later difference describes their paths, and part of it may come from those earlier differences.

6.2 Employment recovery and its limits

A woman who returns to work may do so in a lesser job, with fewer hours or weaker prospects of promotion. The initial employment relationship also matters: returning after an attached absence and finding work after a separation need not involve the same adjustment. I interpret the pathways in Section 6.1 with this distinction in mind.

Current work recovers in part over the observed horizons, and more among women who kept a job than among those who lost one. From these shares alone, however, I learn little about the jobs women return to, since a later job may be with a different employer and on different terms

from the one held before the birth. I therefore add weekly hours, which I can measure only for women in work.

The working sample is selected. At a later horizon, women entering work can replace women leaving it, so the mean hours of current workers can change without the same change occurring for every woman. A higher work share and a lower mean among workers are compatible, but their combination is not an individual transition estimate. I examine reported weekly hours with that distinction in mind.

6.3 Shorter hours among women in work

I measure weekly hours among the women in current work who give a valid hours response, leaving out women on leave from a job, whose reported hours belong to their usual job. This gives 2,073 person-waves, which I analyse with the same calendar-year-adjusted event-time specification as the employment outcomes, again with $t = -2$ as the reference.

Figure 4 shows a sustained reduction in reported hours among the observed current workers. At $t = -1$, mean hours are 34.0 among 319 current workers with valid responses, a group distinct from the 319-woman contract comparison. At $t = 0$, the corresponding mean is 27.2 among 61 workers; at $t = +5$, it is 26.3 among 211 workers. These are raw means. The adjusted change at $+5$ is -9.35 hours relative to -2, with a 95% interval of $[-12.4, -6.3]$ clustered by woman. The adjusted profile also shows that hours among current workers were already falling before the reference wave: they are 8.7 hours higher at $t = -5$ than at $t = -2$. As with the employment profiles, the later differences are measured from a position that already reflects some earlier adjustment. The share of current workers reporting at least 35 hours a week falls from 63% at $t = -1$ to 39% at $t = 0$ and 38% at $t = +5$. Appendix Table A5 reports means, support and adjusted coefficients at each horizon.

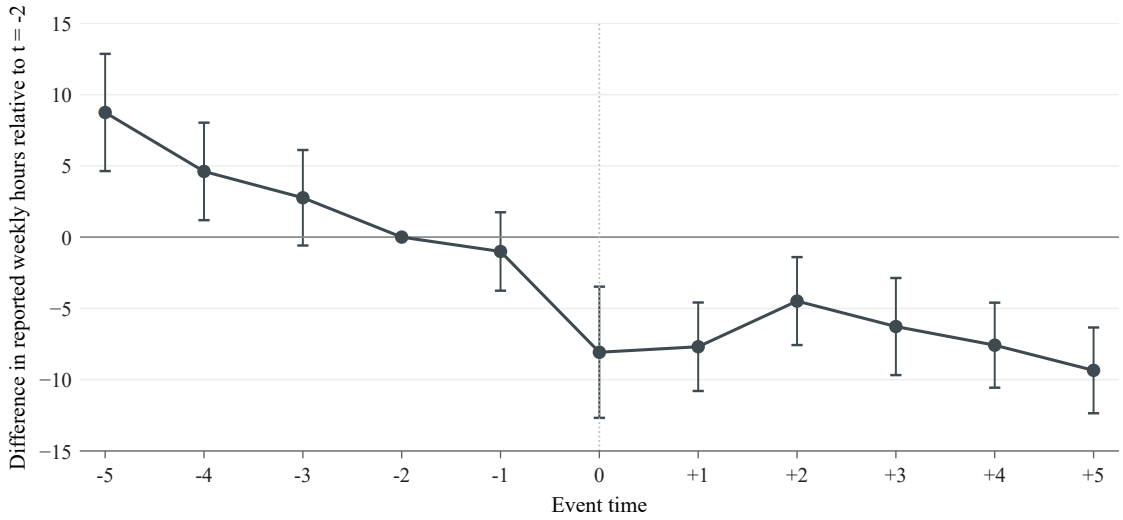


Figure 4: Reported weekly hours conditional on current work

Notes: Points are calendar-year-adjusted differences relative to $t = -2$, with heteroskedasticity-robust 95% intervals. The sample contains 2,073 person-waves with current work and valid reported hours. Attached absentees are excluded. Appendix Table A5 gives raw means and clustered intervals at each horizon. The workers contributing to each horizon differ.

The profile suggests that a count of women in work gives an incomplete account of the

medium-term adjustment. Shorter hours may be desired, constrained, or both. Either way, the opportunities available alongside that choice affect women's ability to sustain earnings and progress within a career.

6.4 Re-entry and training

Workplace arrangements help a woman most when she still has a job after the birth. If she keeps her job, she can use reduced hours or flexibility within it; if she has lost it, she must first find a new one, often without the firm-specific experience and internal opportunities she had built up. For this reason, the distinction between attached absence and non-attachment remains relevant beyond the immediate wave.

An auxiliary question is whether interruptions also reduce investment in job-related skills. Career interruptions can weaken the connection to employer-provided training, while limited time and care responsibilities may restrict self-directed learning. In Japan, the OECD documents much lower training participation among non-regular workers and employees of smaller firms. The main barrier is lack of time for work and family, while non-regular workers are somewhat more likely to cite cost and a lack of information (OECD, 2021). These conditions affect the opportunities for learning during re-entry.

The KHPS and JHPS also ask about voluntary efforts to improve work-related skills and about use of the Training and Education Benefits System. I would need a separate sample and eligibility rules to analyse these questions, so I leave training for future work, together with the question of whether a woman returning after a separation has access to learning opportunities compatible with her care responsibilities and with the jobs open to her.

A lasting disadvantage at re-entry is therefore a possible interpretation, to be tested with evidence on later jobs and pay. My results show that non-regular workers lose attachment far more often and that women who lose attachment are less often in work in later years. With individual histories over a longer period, a study could show whether later work leads back to an earlier career or onto a different, lasting path.

7 Mothers' and partners' employment

In households with both partners observed, I find that mothers account for most of the immediate net change in employment.

Parenthood creates a household shock with two potential margins of adjustment. Either partner can absorb childcare time through lower market work, and the realised allocation may reflect wages, job security, workplace flexibility and social norms within the household. I compare the net change in market employment across the transition for the two adults. Figure 5 uses 539 households with valid current-work and attachment status for both the mother and her partner at both $t = -1$ and $t = 0$. I do not require the mother to have been attached at $t = -1$, so this population differs from both the contract comparison and the pathway groups. By pairing the same households across the two waves, I avoid comparing one set of observed couples before the transition with a different set afterwards.

Among households observed for both adults across the transition, the fall in market work is

almost all among mothers. Maternal current work falls by 45.8 percentage points and employment attachment by 18.2 points, while partners' corresponding net changes are −0.4 and 0.0 points.

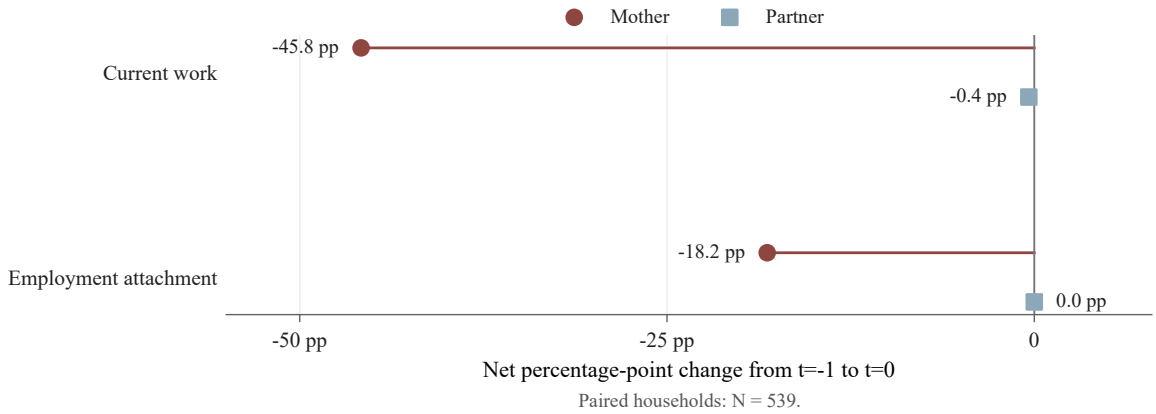


Figure 5: Mother–partner net labour-market changes

Notes: The sample contains 539 households with valid maternal and partner employment status in both $t = -1$ and $t = 0$. The latter is the survey wave reporting the birth. Values are paired within-household percentage-point net changes, calculated for each adult and each employment outcome. Zero net partner attachment change does not mean that no individual partners changed status.

Each plotted value averages the change in an employment indicator between the two waves for the same adults. It is a net change: entries and exits can offset one another, so the maternal decline is not a gross count of work exits. The figure describes the balance of changes in market-employment status within the paired sample.

This comparison differs from Figure 1 in its reference and estimand. Here I report unadjusted paired changes from $t = -1$ to $t = 0$, whereas the event-study profile gives calendar-year-adjusted differences relative to $t = -2$ on changing support, so the numerical magnitudes need not coincide. The requirement to observe both adults at both waves also selects the households in the comparison. The results may not apply to households without an observable partner or with missing employment information.

Within the household, mothers account for almost all of the net decline in employment, while partners' aggregate employment changes little.

8 Discussion and conclusion

8.1 Keeping the job through motherhood

My findings describe employment around childbirth as an outcome patterned by institutions. Much depends on whether an employment relationship remains in place during the interruption. Current work is uncommon in the year of the birth report for both regular and non-regular workers, but most regular workers keep their job while absent, whereas most non-regular workers no longer have one. Once I measure attachment on its own, I can describe the immediate transition in more detail and follow the different employment paths afterwards.

I read this pattern as raising a question about unequal effective access to continuity across employment categories. Prior career histories, care arrangements, household resources and anticipated employment choices can all matter for whether a job survives the interruption.

The institutional setting helps to make this question more specific. Leave rights are relevant only under the applicable eligibility rules, and returning to a job requires an employment relationship as well as a feasible care arrangement. The household's capacity to redistribute care depends on both adults' schedules and available support. The same observed non-working state can therefore arise from different constraints and decisions. I separate attached absence from non-attachment because the two positions imply different immediate relationships to employment.

8.2 Policy implications

The policy questions begin with the distinction between maintaining an existing employment relationship and supporting work after it has ended.

For non-regular workers, effective access to leave and continuity deserves particular attention: in the contract comparison, 58.6% of them lose attachment, against 14.7% of regular workers. A right to leave helps little if a woman does not qualify for it or her contract ends first, and the panel records the separation without its cause. To evaluate these protections, a study would need direct information on eligibility and contract renewal, together with variation in the relevant policy.

Employers may also differ in their capacity to cover an absence. A small firm with few staff may find an extended absence hard to accommodate, yet my estimates of the contract gap are similar at small and larger employers, with a wide interval for their difference. Subsidies for replacement staff or help with scheduling could ease these constraints, and both merit evaluation.

Childcare remains an important complementary condition. A retained job is of limited use if returning to work cannot be coordinated with care. A childcare place, in turn, does not by itself provide a suitable job after a separation. The childcare evaluations discussed in Section 2.3 show that the population and the intervention both matter: access to a scarce place among applicants can have a large effect, while a broad expansion may also change the form of care used by already employed mothers. An evaluation of the role of care should therefore consider both employment continuity and entry into a new job.

The paired household results also draw attention to fathers' opportunities to adjust. Mothers account for most of the net employment decline among couples with both partners observed. If fathers found it easier to reduce their hours or take leave, households might share care on more equal terms. Because I observe only employment status, a net change of zero in partners' attachment may still hide changes in fathers' working hours or in their share of care, which would need direct measurement.

For re-entry, the concern is the set of jobs and career paths available after an interruption. Reduced-hours employment may be compatible with family needs while offering limited subsequent progression. Training and information about employment opportunities may be relevant, but their value depends on access, time and prospective returns. Here too, I do not treat labour-market segmentation and fiscal thresholds as competing explanations. The jobs available, their hours and pay, and the household's tax and contribution position can together influence the feasible return to work.

8.3 Beyond Japan

The connection between employment continuity and job structure extends beyond Japan. In Spain and Italy, the studies discussed in Section 3 find that mothers' earnings fall in part because they work fewer days or weeks and in part because they move to part-time or fixed-term contracts or to lower-paying firms. The institutions differ, and Japanese non-regular employment covers more than fixed-term contracts. In each country, however, the effect of an interruption may depend on the worker's position in the labour market and on the jobs open to her afterwards.

My findings also relate to Goldin (2014)'s argument that persistent gender gaps depend on the structure of jobs. Goldin emphasises temporal flexibility and its remuneration. My results concern the employment relationship during an interruption and the later hours of observed workers. These dimensions can interact: flexibility may help maintain a position, while losing a position can change access to flexibility and progression in later work.

A five-year follow-up remains a medium-term view. Employment after that point may change as children enter school, households adjust care arrangements, or further births occur. Earnings and promotion may evolve even among women with uninterrupted employment, as the Japanese personnel evidence illustrates (Okuyama et al., 2025). I therefore read the current-work and attachment profiles as part of a longer career process. They do not measure the lifetime cost of motherhood.

8.4 Limitations

The sample consists of selected birth reports with employment information in the surrounding years. Some reports retain unresolved parity, so I cannot identify a first-child penalty for all women. The marriage-related selection and the requirement for observable panel histories also limit external validity. My findings apply to these observed birth-report trajectories and are not representative of all mothers in Japan.

The surveys are annual, so I can date employment changes only to the year, and much of the change takes place before the birth report. I estimate the profiles with calendar-year effects but without age controls, individual effects or a never-treated or not-yet-treated comparison group. They combine changes within women with differences in the women observed at each horizon. These features limit causal interpretation regardless of the precision of an individual coefficient.

In the balanced-window checks, I find the same broad pattern among women with complete follow-up, although these women are a selected group and the outcomes of women lost from observation remain unknown. I also lose women from the two pathway groups at different rates, and I see employment only at each interview. The displayed shares therefore describe the women still observed at each horizon.

The contract comparison covers only women still attached at $t = -1$, so it leaves out women who had already left work, for example around marriage. Women in non-regular jobs may also differ from regular workers in their family circumstances and earlier careers, and the baseline model adjusts only for employer size, so I interpret the contract gap as a difference in attachment loss between the two groups, part of which may reflect these other differences. Measuring contract type at $t = -2$ lets me check the timing, but on a smaller sample, so its differences from the baseline mix timing with the change in sample. The association is also stronger for

women reported in the spouse section of their husband's questionnaire (odds ratio 18.6) than for female respondents (4.1), and that comparison mixes differences between the two groups of women with differences in how the questionnaire was completed.

My employment measures describe only part of a mother's situation, leaving out her care arrangements and the quality of her job. With data on a woman's access to leave and childcare and on her later jobs, I could say more about the reasons behind these patterns. The reasons women give for a non-regular job also refer to the time before the birth, so they leave the cause of a later separation open. Preferences and constraints may therefore both play a part, often together.

8.5 Concluding remarks

In this paper, I document unequal employment attachment around childbirth in Japan. Few regular or non-regular workers are in current work in the year of the birth report, but non-regular workers lose attachment far more often. Later employment becomes more common in both initial non-working groups, but the observed paths remain different for five years after the birth report. Among current workers, reported hours also remain below the earlier reference.

A woman's later employment depends on whether her job survives the interruption and, if it does not, on the opportunities open to her afterwards. Childcare, leave, workplace practices and household care arrangements are all relevant to both. My findings show the value of distinguishing work from employment attachment in discussions of continuity through motherhood. A recovery in employment counts is only part of the subsequent adjustment.

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A Employment construction and event-time support

The current-work indicator equals one for the harmonised questionnaire categories reporting work and zero for temporary absence or non-work. Attachment also includes temporary absence with a job. The questionnaire's employment-form and job-position responses distinguish wage employment from other forms of work. Contradictions include positive hours with a non-working status, zero hours with a working status, incompatible employment-form and position responses, or a childcare/nursing-leave response inconsistent with temporary absence. Such observations remain indeterminate. I do not treat hours associated with a usual job during temporary absence as evidence of actual work in the reference month.

I require the woman in a spouse record to match the mother identified at the birth report, so a change of spouse or inconsistent identifying information does not create an employment transition for the mother. I keep valid observations on either side of gaps without imputing the missing employment states. I use one birth report for each respondent history, so the birth event is the first report observed in the panel, subject to the retrospective parity exclusion described in Section 4.1.

Table A1 reports the complete binary-outcome coefficient series and its observation support. Calendar-year effects are the only fixed effects. For the clustered standard errors, I treat each woman as a cluster, allowing dependence within her observed history; the coefficients are unchanged. Neither variance estimator reflects uncertainty about selection, reporting or the event definition.

Table A1: Employment profiles and within-woman clustered standard errors

t	N_t	Work	HC SE	Cl. SE	Attach.	HC SE	Cl. SE
−5	109	12.4	4.0	3.7	13.2	3.9	3.5
−4	148	11.6	3.6	3.2	13.2	3.5	3.0
−3	206	8.8	3.4	2.6	8.2	3.4	2.6
−2	342	0.0	—	—	0.0	—	—
−1	578	-17.7	3.0	2.3	-15.3	3.0	2.2
0	623	-68.6	2.6	2.6	-37.1	3.0	2.6
+1	607	-46.1	3.0	2.9	-35.7	3.1	2.8
+2	549	-37.8	3.2	3.0	-33.8	3.2	3.0
+3	465	-34.0	3.3	3.2	-28.6	3.3	3.1
+4	442	-30.3	3.3	3.4	-27.3	3.3	3.4
+5	411	-28.1	3.4	3.6	-28.5	3.4	3.6

Notes: Coefficients and standard errors are in percentage points. HC denotes heteroskedasticity-robust standard errors; Cl. denotes clustering by woman. Work and attachment use the same 4,480 observations for 627 women. N_t is the number of determinate observations at each event time. The $t = -2$ coefficients are normalised to zero.

B Balanced-window comparison

The balanced window from −2 to +1 retains 328 women, the window to +3 retains 263 and the window to +5 retains 229. Each window requires both employment outcomes at every wave. The longest window contains 1,832 person-waves and birth-report cohorts from 2006 to 2017. Its same-window unbalanced comparison contains 4,017 person-waves for 627 women. The two

regressions in Table A2 use the same event-time range and differ only in observation support.

Both samples show current work and attachment below their respective references at +5. The coefficient for attached absence, estimated as its own outcome, is close to zero, with an interval spanning zero. This result concerns the change in the difference between the two outcomes. The balanced raw levels remain distinct: 131 of 229 women report current work and 144 report attachment at +5. A small difference between coefficients cannot establish equality of those raw levels. The balanced sample also selects earlier birth-report cohorts with complete follow-up and cannot represent women without complete observations.

Table A2: Employment at +5 relative to −2, using a common estimation window

Sample	Outcome	N	$\hat{\beta}_5$ (pp)	HC interval	Cluster interval
Balanced	Current work	229	-25.4	[-35.3, -15.6]	[-37.4, -13.4]
Balanced	Attachment	229	-24.7	[-34.5, -14.8]	[-37.7, -11.6]
Balanced	Attached absence	229	0.8	[-3.2, 4.7]	[-3.9, 5.5]
Unbalanced	Current work	627	-28.5	[-35.3, -21.8]	[-35.7, -21.4]
Unbalanced	Attachment	627	-29.3	[-36.0, -22.6]	[-36.5, -22.1]
Unbalanced	Attached absence	627	-0.7	[-3.3, 1.9]	[-3.3, 1.9]

Notes: All regressions use $t = -2, \ldots, +5$ and calendar-year effects. Intervals are 95% intervals, either heteroskedasticity-robust or clustered by woman. The attached-absence outcome equals attachment minus current work and has its own regression. Coefficients differ from the full-window figure because that figure also includes $-5, -4, -3$.

C Contract comparisons and timing

Table A3 distinguishes adjustments on the same 319 women from checks on smaller available samples. Broad occupation, commute burden and the geographic indicators make little difference to the standardised gap on the common sample. Geography is a coarse control for location. These specifications do not identify an occupation, commuting or regional mechanism.

The $t = -2$ contract check retains 200 women with the earlier classification available. Employer-size covariates remain measured at $t = -1$. The birth-month and reporting-lag adjustment retains 311 women. Their gaps remain large, but differences from the baseline combine specification changes with changes in sample. I therefore do not interpret these checks as the isolated effect of changing the timing measure on an otherwise identical population.

In the known-employer-size comparison, the regular/non-regular gap is 42.9 percentage points within small employers (95% interval 27.2–58.6) and 43.5 points within larger employers or government (30.1–56.9). The estimated difference between those gaps is −0.6 points, with an interval from −21.2 to 20.1. The wide interval leaves room for differences of economic importance. This comparison supports a large contract association in both employer-size categories without establishing equal gaps or a causal firm-size effect. In this calculation, I exclude observations with missing employer size, which the main model retains.

Table A3: Standardised attachment-loss gaps under alternative specifications

Specification	N	Gap (pp)	95% interval
Baseline	319	42.4	[32.2, 52.6]
+ broad occupation	319	41.1	[30.3, 51.9]
+ commute burden	319	41.9	[31.5, 52.4]
+ designated-major-city indicator	319	41.7	[31.5, 51.8]
+ Kanto/Kinki indicator	319	42.3	[32.1, 52.6]
Contract measured at $t = -2$	200	39.8	[26.6, 53.1]
+ birth-month/report-lag adjustment	311	42.2	[31.7, 52.6]

Notes: Each row reports the standardised probability difference for non-regular minus regular employment, in percentage points, with its 95% delta-method interval using HC1 covariance. Each model averages over its own estimation sample. The first five specifications use the same 319 women and 103 attachment losses. The final two rows use different women.

D Parity uncertainty and route into the sample

The retrospective exclusion does not establish biological parity for the remaining reports. In a conservative check, I remove all unresolved older-child flags, which leaves 373 birth-report events before outcome-specific restrictions. Among women with a regular or non-regular job at $t = -1$, 24 of 140 regular workers lose attachment (17.1%), compared with 57 of 86 non-regular workers (66.3%). The raw contract difference therefore remains substantial under this exclusion. The excluded reports are unresolved cases; the check does not establish that every excluded event was a later birth or every retained event a first birth.

I also fit a broader model of attachment loss, which keeps missing contract and employer-size information through separate indicators. It includes non-regular status, small-employer status and the two missing-information indicators. Its non-regular odds ratio is 7.87 (HC1 95% interval 4.58–13.54) among the 357 women attached at $t = -1$ and 9.32 (4.92–17.65) among the 245 women left after the conservative parity exclusion. For loss of current work among the 334 women currently working at $t = -1$, the same specification gives a non-regular odds ratio of 1.06 (0.52–2.16): 87.4% of regular and 86.9% of non-regular workers are not currently working at $t = 0$. These are distinct from the main 319-person model and its standardised probability gap. They also use different women.

The same broader modelling approach gives an odds ratio of 4.10 (2.02–8.33) for female survey respondents and 18.61 (7.39–46.88) for women observed through the spouse section of a male respondent. The association is positive in both routes, with substantial heterogeneity in magnitude. The questionnaires designate the spouse section for the spouse, while the files do not record the identity of the person completing it. I cannot separate population composition from unobserved completion behaviour in these results. I read this heterogeneity as a qualification to pooling, without treating it as proof of proxy-reporting bias.

E Observation support for later employment

Table A4 gives the denominators behind the current-work panel in Figure 3. The starting population is the 357 women attached at $t = -1$, with any type of job. The two initial groups lose observations at different rates. A higher work share at a later horizon is therefore a property

of the observed group and need not equal the transition probability of the initial cohort. I do not observe every return and later exit between interviews.

Table A4: Current work and available observations by initial employment state

t	Attached absence: N	Work (%)	Attachment loss: N	Work (%)
0	185	0.0	118	0.0
1	181	55.8	116	18.1
2	156	75.6	113	25.7
3	131	67.9	95	33.7
4	124	67.7	93	40.9
5	107	76.6	91	45.1

Notes: The initial groups are attached but not currently working and not attached at $t = 0$, conditional on attachment at -1. Each percentage uses the corresponding horizon-specific N. Zero current work at $t = 0$ follows from the group definitions.

F Hours among current workers

The hours analysis uses 2,073 person-waves with current work and valid weekly hours. I exclude women on job-attached leave even when they report positive usual hours. The adjusted change at $+5$ is -9.35 hours relative to -2, with a 95% interval of $[-12.4, -6.3]$ clustered by woman. The workers observed at each horizon differ, so this is a conditional-hours profile. It does not establish the change in hours for every returning mother or an unconditional hours loss across the original cohort.

Figure 4 in the main text shows the complete conditional-hours profile.

Table A5: Conditional hours: raw levels and adjusted differences

t	N_t	Mean hours	Adjusted change	Cluster interval
-5	93	43.4	8.74	[5.0, 12.5]
-4	121	39.7	4.61	[1.7, 7.5]
-3	166	37.8	2.76	[0.0, 5.5]
-2	251	35.0	0.0	—
-1	319	34.0	-1.01	[-3.3, 1.3]
0	61	27.2	-8.08	[-12.4, -3.7]
1	201	28.2	-7.69	[-10.6, -4.7]
2	229	30.6	-4.49	[-7.4, -1.5]
3	207	28.9	-6.27	[-9.7, -2.9]
4	214	28.1	-7.58	[-10.6, -4.6]
5	211	26.3	-9.35	[-12.4, -6.3]

Notes: N_t counts current workers with valid hours at each horizon. Mean hours are unadjusted. Adjusted changes use the full -5 to $+5$ window and calendar-year effects; intervals cluster by woman. The reference coefficient is normalised to zero.

G Reasons for non-regular employment before the transition

Of the 128 non-regular workers in the main contract comparison, 117 provide a valid closest-reason response at $t = -1$; 11 responses are missing or unusable. Table A6 reports the distribution among valid respondents. A single closest reason does not give an exhaustive account of preferences or constraints. These responses concern the employment form before the transition and do not identify the causes of later attachment loss.

Table A6: Reported reason for non-regular employment at $t = -1$

Closest reason at $t = -1$	N	Share (%)
Wanted regular employment but could not find an employer	10	8.5
Favourable wages, working conditions or treatment	53	45.3
Personal circumstances incompatible with regular-employment conditions	44	37.6
Other	10	8.5

Notes: The denominator is 117 valid responses among 128 non-regular women in the main 319-person contract sample. Percentages may not sum to 100 because of rounding. Labels summarise the questionnaire categories.