The Response of Firms to Maternity Leave and Sickness Absence*

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Abstract

The costs to a firm of employee absence depend on how easy it is to find a replacement. We study how firms respond to predictable, but uncertain, worker absences that arise from maternity and non-work-related sickness leave. Using administrative data on over two million spells of leave in Brazil, we identify the short-run effects of a leave spell starting on a firm's employment, hiring, and separations. We find that firms respond immediately to the start of leave by hiring new workers, and to a lesser extent, by limiting job separations. However, firms replace leave-takers at far less than the one-for-one rate implied by a frictionless labor market model. Hiring responses are more pronounced for absences arising in occupations with more transferable skills and in firms operating in thicker labor markets. Altogether, our results suggest that replacing workers using external markets is costly and firms manage predictable worker absences through other channels.
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1 Introduction

Firm managers believe employee absence reduces productivity and profits (Nicholson et al. 2006, Pauly et al. 2008). These beliefs are reflected in firms' general opposition to the creation or expansion of policies that mandate sickness leave and parental leave. In the face of such mandates, firms may act to limit the costs imposed by absence. They can do so in two ways. Firms can screen out workers likely to take leave or pressure employees to not take leave. These responses work against the intended effects of leave mandates and exacerbate discrimination. Firms can also respond by modifying the organization of work to mitigate disruptions associated with leave-taking. For example, firms can structure tasks so that it is easy to hire temporary replacements or so that coworkers can always cover for an absent teammate. Convincing firms to adopt such "leave-friendly" policies is important, not only for leave mandates to be successful, but also to help mitigate earnings and career disparities between men and women (Goldin 2014).

To date, we know little about how firms react to employee leave-taking and how their reactions are shaped by the markets in which they operate. We study how firms respond to two common and policy-relevant sources of worker absence—maternity and sickness leave. Specifically, we document the ease with which firms hire new workers to address labor supply disruptions induced by leave-taking.[1] The neoclassical frictionless labor market model provides a useful benchmark: firms should respond to a worker's absence by hiring a new worker from the external market, one-for-one, to replace them. Further, even with perfect foresight, this one-for-one response should happen at the moment leave starts. Deviations from the neoclassical benchmark will be reflected in smaller hiring responses that are also less immediate. Perhaps counterintuitively, greater costs of leave-taking are reflected in smaller observed hiring responses to a new spell of leave.

We analyze firm responses to leave-taking using data from Brazil, where leave mandates are quite generous. Women are guaranteed 120 days of maternity leave with full wage replacement, financed by the Brazilian government. Sickness leave is also publicly financed

[1]Studies on sickness leave have generally focused on how leave mandates impact absenteeism and presenteeism, employee health, and the spread of disease (e.g., Ziebarth and Karlsson 2010, 2014, Pichler and Ziebarth 2017). Pichler and Ziebarth (2020) assess how city- and state-level sickness pay mandates in the United States affect county- and state-level employment and wages. We are unaware of studies that analyze the behavior of firms around the onset of sickness absence. There is also a very large body of work documenting the effects of maternity leave on subsequent labor market outcomes and health of leave-takers (e.g., Lalive and Zweimüller 2009, Rossin 2011, Stearns 2016, Butikofer et al. forthcoming). Less work has documented how firms respond to maternity leave policy with some exceptions that we discuss below.

after the first 15 days.[2] Using administrative data on over two million spells of leave, we estimate the short-run effects of a leave spell starting on firms' employment, hiring, and separations. Our data are monthly, and we observe the exact month in which a leave spell starts, along with employment dynamics in the same plant and occupation as the leave-taker. The detailed timing allows us to identify the effect of leave initiation in an event study framework. We assume the timing of leave spells is random with respect to idiosyncratic employment dynamics specific to the leave-taker's plant and occupation. In particular, we assume workers do not time their leave based on accelerating or decelerating employment growth at their plant. Importantly, the identifying assumptions hold even if firms know that workers are prone to take leave and have plans in place for handling their absence. Our estimates should be interpreted as measuring the firm's execution of its cost-minimizing plans when a worker actually takes leave. As such, they are informative about the ease with which the firm can hire replacement labor on the external market.

We find that firms respond immediately to the start of leave by hiring new workers and, to a lesser extent, by limiting the rate of job separations (quits and fires). In the months following the initiation of maternity leave, employment in the occupation of the leave-taker increases by 0.13–0.2 workers (including the leave-taker). After the onset of sickness leave, net employment in the occupation of the leave-taker increases by 0.03–0.06 workers. For spells of sickness leave, which are more likely to come as a surprise, we find no evidence of employment adjustments in the months before leave starts. By contrast, employment begins increasing two months prior to the start of a maternity leave spell, and we provide evidence that these changes reflect anticipation effects on the part of employers, not strategic leave timing.

The average effects are statistically and economically significant, but still quite small when benchmarked against the prediction of one-for-one replacement in the frictionless labor market model with homogeneous workers. We argue that these small responses reflect that firms find other ways to adjust to labor supply disruptions in the presence of recruiting frictions. In support of this argument, we examine heterogeneity in the employment, hiring, and separation responses across different market settings. We find that hiring responses are strongest in production-related occupations and smallest for managerial jobs where specific capital is likely harder to replace. Similarly, in the case of maternity leave, hiring responses are larger when the leave-taker has relatively less tenure and is likely easier to replace than someone with more firm-specific experience. We also find that the hiring response is stronger

[2] We focus on sickness absences that are not work-related and that last longer than 15 days.

among firms in thicker labor markets. By contrast, firms in thinner markets are more likely to retain incumbent workers in the months prior to maternity leave.[3]

Even when focusing on more fluid markets or cases where workers are more substitutable, we always find hiring responses far below the one-for-one benchmark. It follows that firms generally handle worker absences through channels other than hiring from the external market. Jäger and Heining (2019) suggest that in the presence of market frictions, firms respond to labor supply disruptions by increasing demand for their incumbent workers. That we find some evidence of firms limiting separations prior to maternity leave is consistent with this idea. We surmise that firms and workers may enter implicit contracts in which, as in Stole and Zwiebel (1996), firms hire excess labor and workers fill in to cover temporary workforce disruptions. Such a view is supported by the evidence in Hensvik and Rosenqvist (2019), who find that firms build greater redundancy in occupations where workers are more difficult to replace through external markets.

There are two major concerns about the internal validity of our analysis. First, we assume that leave spells are not timed to coincide with periods of particularly slack, or strong, labor demand. We provide various pieces of evidence in support of this assumption. In particular, we show that our qualitative and quantitative results hold when we consider longer event windows, and are robust to more demanding specifications that include control units, or that control for arbitrary industry-specific employment dynamics. Second, the administrative data we use does not measure informal employment, which accounts for around 40 percent of jobs in Brazil. Prior research on informal employment in Brazil suggests firms are not likely to use informal hires in response to leave-taking (Almeida and Carneiro 2012). Nevertheless, we use data on plant-level labor inspections and show that, if anything, the estimated employment responses are stronger among firms that were previously found to have employed informal workers. The opposite would be the case if firms were using informal hires, which we cannot observe, to replace workers on leave.

Our paper contributes new evidence on employer responses to worker absence and the costs imposed on firms by leave-taking. Firm managers believe workplace absence to be costly (Nicholson et al. 2006, Pauly et al. 2008), but these perceptions are not clearly reflected in firms' responses to parental leave mandates. Gallen (2019) finds no effect of an unexpected

[3]One might be concerned that the leave spells we study are too short for replacement to be necessary. Both maternity and sickness leaves often lead to permanent separation. Separations significantly increase four and five months after maternity leave starts, driven largely by the departure of the leave-taker after taking the full 120 days of leave guaranteed by law. Separations increase after a sickness leave begins, also driven in large part by the permanent departure of the leave-taker. Hence, in response to maternity and sickness leave, firms likely hire with the expectation they will need a permanent replacement.

and retroactively applied 2002 Danish reform that increased parental leave by 22 weeks on coworkers' employment or earnings. She does find that small firms were more likely to shut down after the policy change. Ginja et al. (2020) exploit a similar reform in Sweden that increased paid parental leave from 12 to 15 months. They find private sector firms with greater exposure to the reform reacted by hiring temporary workers and increasing the hours of incumbent workers. Their estimates imply that for the average workplace, having one female worker entitled to the extended leave increased the firm's total wage bill by about the salary cost of one half of a full-time worker. The estimates in Gallen (2019) and Ginja et al. (2020) are identified from variation in leave duration induced by unexpected and retroactively applied reforms that extend parental leave from already generous levels. That is, they examine how firms react when workers unexpectedly become eligible for additional leave. Their estimates, therefore, reflect firms' adjustment costs over and above those already incurred from workers going on leave. In contrast, we are interested in the act of leave-taking itself, not the effects of leave extensions. By showing how firms respond to predictable, but uncertain, absences in a stable policy environment, we add new evidence on the existence and importance of labor market rigidity. Since they do not hire replacements, firms in Brazil likely react to worker departures indirectly through changes to workforce management and production methods.[4]

Two other papers use linked employer-employee data to study how firms use external markets to address workforce disruption with methods similar to ours. Jäger and Heining (2019) study the responses of small German firms to sudden worker deaths, and Brenøe et al. (2020) use Danish data to study the effects of maternity leave on employment dynamics. The sudden deaths in Jäger and Heining (2019) almost certainly occur at random, which helps with identification and internal validity. However, the events they study are rare, probably shocking, and even traumatizing to the firms' employees and managers. We focus, by contrast, on vastly more common causes of workplace absence, and our findings may, therefore, better reflect how firms handle standard employment disruptions.[5,6] Furthermore,

[4]Evidence for the latter mechanism is illustrated by Dillender and Hershbein (2018), who find family leave policies in New Jersey and Rhode Island led firms to increase the skill requirements in job postings, and Hotz et al. (2017), who show that firms are differentiated by their "family-friendly" policies. Dionne and Dostie (2007) also find that absenteeism is associated with firm policies that allow for particular types of scheduling flexibility.

[5]Jäger and Heining (2019) evaluate around 1,500 deaths per year in an economy of around 40 million workers. We study around 400,000 events per year in a labor market only slightly larger.

[6]Using data from Chile, Drexler and Schoar (2014) show the adverse effects on borrowers of loan officer turnover are smaller when turnover is expected, as in the case of maternity leave, and largest in the case of serious unexpected illness.

Jäger and Heining (2019) and Brenøe et al. (2020) can only measure event timing and employment dynamics year-by-year. Our monthly data allow us to pin the expansion in employment directly to the onset of leave and to more clearly scrutinize pre-event outcomes, all of which lend support to our event study model and obviate the need to find complicated control units. More plainly, our results suggest that the lack of substantial employment responses to worker departures documented in other studies does not mask strong short-run fluctuations. Finally, we study a developing country, Brazil, with a large and diverse workforce, and a labor market characterized by significant frictions (Engbom and Moser 2018). Similar to the Northern European countries that have been the focus of prior research, Brazil has generous social insurance and dynamic worker mobility. Brazil also has unique institutional features and administrative data that guide our empirical approach and analysis through the rest of the paper.

2 Institutional Setting

In Brazil, the costs to firms of maternity and sickness leave arise primarily from personnel disruptions and firing restrictions. Firms do not pay to replace the wages of their workers who are on maternity leave. For employees on sickness leave, the firm replaces the worker's full wage during the first 15 days of absence, and the government pays the benefits thereafter. During maternity and sickness leaves, leave-takers have job protection. Workers who return from maternity leave enjoy a period of job protection, but workers returning from non-work-related sickness leave do not. Here we briefly review Brazil's unique policies governing maternity and sickness leave as well as the costs associated with employment and firing.

2.1 Maternity Leave Policies

Maternity leave was established as a constitutional right in 1988.[7] Article 7 (XVIII) of the Brazilian Constitution and Article 392 of the Consolidated Labor Laws outline the details of maternity leave entitlements. All women who are formally employed and contribute to Social Security are eligible for benefits regardless of length of tenure at the employer.

Women employed in the private sector are entitled to 120 days of paid maternity leave, and leave can start as early as the eighth month of pregnancy.[8] Women have job protection starting from when pregnancy is confirmed up to five months after delivery. Those on maternity leave receive 100 percent of their earnings (with no cap). The employer pays the benefit to the woman and is then reimbursed by deductions from owed contributions to the

[7] Prior to 1988, women could take 84 days of paid maternity leave.

[8] Men are entitled to five days of paid paternity leave.

Brazilian Social Security Administration (INSS).

On September 9, 2008, the federal government passed Law 11,770, which established the Empresa Cidadã (EC) Program. The law became effective in the private sector on January 1, 2010.[9] Firms can voluntarily participate in the EC Program, and if they do, they must extend a woman's maternity leave to 180 days total (an additional 60 days). The employer pays the additional two months of maternity leave payments to the woman, but can then deduct those payments from its owed income taxes. Thus, for firms that do or do not participate in the EC Program, there are no direct financial costs of maternity leave since the payments are reimbursed. Extended maternity leave must be guaranteed to all women who work for a firm that participates in the EC program, but women can decline the extension. If a woman would like the extension, she must apply within 30 days of giving birth, and the additional two months of paid leave begin immediately after the initial 120 leave days. According to Machado and Pinho Neto (2018), fewer than 10 percent of eligible firms join the program.[10]

2.2 Sickness Leave Policies

Brazil's Consolidated Labor Laws provide mandatory paid sickness leave (referred to as *Auxílio-Doença*). Individuals who have contributed to Social Security for at least 12 months are eligible to receive sickness benefits.[11] The employer pays the employee's full salary for the first 15 days of sickness absence, regardless of whether the sickness is work-related or not. After the 15th day, INSS pays the sickness leave benefits. The payment is based on a fixed percentage of the worker's "benefit salary," which is the average of the worker's monthly earnings in the 12 months immediately preceding the date which the worker is eligible to receive the benefits (up to a cap).

Sickness absences must be certified by a physician and benefits are granted to those determined to be temporarily unable to work. While receiving sickness benefits, the employee cannot be dismissed. If the individual is determined to be permanently disabled, he/she receives a disability pension rather than sickness benefits. There is no maximum period of payment of sickness benefits, but they are considered temporary. Thus, an individual continues to receive such benefits until he/she is declared fit for work or is declared permanently disabled (or death).

[9] As part of this law, the federal government extended maternity leave to 180 days for its own employees.

[10] They also point out that companies that join the program are relatively large, and as we discuss later, the leave spells we consider in our analysis tend to originate from smaller firms.

[11] There is no minimum contribution period for individuals involved in a work-related accident.

If an employee receives sickness benefits for work-related illness, he/she cannot be dismissed for at least one year after returning to work. However, such job protection upon returning to work does not apply to non-work-related sickness absence, and in practice, workers are often dismissed once they return from a non-work-related sickness leave (Barbosa-Branco et al. 2011). In our analysis, we consider non-work-related sickness leaves only.

2.3 Costs of Employment

Brazil has relatively high costs of employment, and especially of terminating workers. These costs affect the options available to firms to manage workforce disruptions that arise when workers go on leave. Most employment contracts in Brazil are open-term contracts with no fixed expiration. To dismiss a worker employed on an open-term contract, the employer must provide justification, or cause, that aligns with provisions of the labor code. They must also provide advance notification at least 30 days prior to dismissal, with the advance notice period growing proportionately with the worker's tenure. During the notice period, the employer is required to let the worker spend two hours per day searching for a new job. In practice, the employer often just pays the worker a month's salary as a severance payment in lieu of advance notice. Upon dismissal, the employer must also pay out the pro-rated value of any untaken vacation days and the 13th salary (Christmas bonus). In addition, if the worker is fired without cause, the employer must pay a penalty equal to 40 percent of the value in the worker's Social Security account. The Social Security account (*Fundo de Garantia do Tempo e Serviço* or FGTS) is funded by an 8.5 percent employer contribution, deposited monthly. Hence, the cost of firing a worker without cause is high and increasing in proportion with both tenure and the wage rate. In practice, establishing cause for termination is very difficult, so employers generally expect to pay the FGTS penalty.

The two main costs of dismissal, therefore, are the severance payment and the FGTS penalty for termination without cause. Consider an employer who wants to hire a replacement worker during a woman's 4-month maternity leave spell, and they can hire such a replacement at the same wage rate. Between the severance payment and the FGTS penalty, they can expect to pay 1.13 times the monthly wage as a termination cost. The employer could instead offer a temporary or fixed-term contract. However, temporary contracts are tightly regulated in Brazil in a way that can easily make them no less costly than open-term contracts. Firms can also offer a contract with a maximum 90-day probationary period that can be terminated without incurring a firing penalty. After the probationary term, however, the contract automatically transitions to open-term.

3 Data

We use matched employer-employee data from Brazil's *Relação Anual de Informações Sociais* (RAIS). RAIS is an annual census of all formal-sector jobs. Each year, the Brazilian Ministry of Labor and Employment (MTE) collects data on every job for the purpose of administering Social Security and programs to supplement workers' wages. The information in RAIS is provided to the MTE by a manager in each establishment. Compliance with reporting requirements is extremely high, as employers who fail to complete the survey face mandatory fines and also risk litigation from employees. For each job, in each year, the employer reports characteristics of the worker, the job, and the establishment. Worker characteristics include gender, race, age, and educational attainment. Job characteristics relevant to this study include the 6-digit occupation,[12] the exact date of hire, and the month and year of separation. Establishment characteristics include industry, location, and number of employees at the end of the calendar year.

3.1 Leave Spells

Starting in 2007, the RAIS data contain information on up to three leave spells for a worker in a given year, including the start and end dates as well as the reason for each spell. We consider maternity leaves and non-work-related sickness leaves. Only sickness absences longer than 15 uninterrupted days must be reported in RAIS. Thus, most of the sickness leaves we observe exceed the 15 days where the employer provides wage replacement. Important for our analysis is the month in which a maternity or sickness leave starts, as we consider employment dynamics around the onset of a leave spell. If a person takes multiple maternity (sickness) leaves in a calendar year, we treat the start of the maternity (sickness) leave as the start date of the initial spell, and we code the end date as the end date of the last spell.[13] Furthermore, because the data is reported for each calendar year, many leaves have a January 1 start date, but they are spells that continue from the previous calendar year. When a person has a maternity (sickness) leave with a January 1 start date and a maternity (sickness) leave with a December 31 end date in the prior calendar year, we treat this case as one continuous maternity (sickness) leave spell and assign the leave initiation month appropriately. In our extract of the RAIS data, we do not have any leave spell data for 2011. Given we need prior calendar year information to correctly assign leave start dates, we focus on spells of leave

[12] Occupation codes are based on the 2002 vintage of Brazil's occupation classification system, the *Código Brasileiro de Ocupações* (CBO-2002).

[13] If a person has multiple reported maternity (sickness) leaves that overlap, we take the earliest start date for any maternity (sickness) leave in that calendar year.

that start after January 1, 2012.

3.2 Estimation Samples

We study the employment dynamics of occupation groups within plants (hereafter referred to as a plant-occupation) in the months surrounding the initiation of either maternity or sickness leave. To assign jobs to occupation groups, we use the first digit of the CBO occupation code which separates jobs into the categories listed in Table 1. These high-level classifications ensure that occupation groups within plants are sufficiently large while separating workers whose tasks are dissimilar. In some analyses, we further collapse these classifications to distinguish workers in managerial (CBO code 1), technical (CBO codes 2 and 3), and production (all other codes) jobs.[14]

Using the information in RAIS on leave start and end dates, hire dates, and separation dates, we build a monthly plant-occupation panel measuring: the net change in the number of contracted workers as well as the number of workers hired into or separated from the plant-occupation (including zeros).[15,16] In addition to total separations, we consider the number of separations excluding the leave-taker to understand whether observed changes in worker exits are driven by the leave-taker or his/her coworkers. In our primary results, we study an event window that includes the month leave starts and the three months before and after (seven months total).[17] Henceforth, we refer to an event window by the number of months we consider before and after the month of leave onset (e.g., we focus on a 3-month window in our baseline analysis). We mostly focus on dynamics in the plant-occupation of the leave-taker, but we also examine spillover effects on other occupations within the same plant as the leave-taker during the same event window.

To ensure that our results reflect the firm's responses during the event window to a specific leave spell, we focus on what we call "clean" leave spells. A spell of maternity leave is "clean" if the event window centered on the month of initiation does not intersect the event window of another maternity leave spell at that plant. We define clean spells of

[14]The CBO-2002 system categorizes the 1-digit occupations, often referred to as "large groups," into a hierarchy according to the similarity of functions performed and required skill. Our coarse occupation groupings (managerial, technical, production) correspond to this hierarchical categorization.

[15]In a given month, the net change in the number of contracted workers is the number of workers hired in that month minus the number of workers who separate in that month.

[16]We also examined changes in the number of temporary workers, defined as those with temporary contracts or fixed-term contracts, which specify employment for a fixed length of time (up to two years). We generally find no statistically significant effects or very small and not economically meaningful effects of leave-taking on temporary employment dynamics. We, therefore, do not present those results, but they are available upon request.

[17]In Section 5.3, we consider longer event windows.

sickness leave analogously. When using a 3-month event window around leave onset, the leave spell is, therefore, clean if it is the only leave of a given type that starts at that plant in a 13-month span including the six months before and six months after it begins.[18] In addition, a clean spell requires that the leave-taker have at least four months of tenure with their employer at the start of their absence. This requirement is necessary to avoid capturing the hiring of the leave-taker in the employment dynamics surrounding the start of the leave.[19]

Our focus on clean leave spells helps isolate employment responses to a specific cause, but at a cost of inducing sample selection. We only include plant-occupation observations in the estimation sample for periods corresponding to the event window. Thus, the sample is balanced around the event time. However, plants that experience multiple leave starts in a given month or within several months of each other are excluded from the sample. The restriction to clean spells, thus, biases the analysis toward small plants. We discuss selection at length in Section 3.3.

We exclude public sector plants and plants with military-related occupations (CBO code 0) as leave policies governing public sector employees differ from those governing private sector workers (e.g., federal and state governments must provide the maternity leave entitlements of the EC Program). We also exclude plants with less than five contracted workers during the majority of the period they are observed in our RAIS sample (2003–2017), and we only consider plant-occupations with at least one contracted worker throughout the event window (i.e., the three months before, month of, and three months after leave onset).[20] We do this to eliminate very small establishments composed of self-employed individuals as well as establishments where there are temporary periods of no contracted workers in a given 1-digit occupation as responses to leave-taking and employment dynamics more generally may be quite different among these very small firms.

[18]For example, if a plant experienced a maternity leave that started in March 2013, we consider it to be clean if no other maternity leave spells started at that plant between September 2012–September 2013.

[19]Although our unit of analysis is the plant-occupation, we define clean spells at the plant-level (rather than plant-occupation level) because it is possible a leave in one occupation impacts other occupations within the same plant. When we consider spillover effects of leave onset to other occupations, we want to ensure no other leaves of that type had also recently initiated in those other occupations. We discuss results based on a relaxed definition of clean spells in Section 5.2.

[20]We base sample inclusion on plant size during the majority of the time it is observed in RAIS rather than at a given point in time to avoid situations where a plant is in the sample for some periods but not others.

3.3 Descriptive Statistics

Table 2 reports descriptive statistics for the clean maternity and sickness leave spells in our estimation sample in columns 1 and 3, respectively. To document the sample selection introduced by our focus on clean spells, we also report descriptive statistics for the full population of leave spells that meet the other restrictions (e.g., no public sector plants) in columns 2 and 4. The reported statistics are measured in the month of leave onset, with the exception of establishment size, which is measured at the end of the calendar year in which the leave spell starts. Our focus on clean spells is restrictive. Approximately 28 percent of maternity leaves and 6 percent of sickness leaves meet our clean definition. The small share of clean sickness leave spells reflects the fact that many sickness leaves occur at larger firms, where it is more likely that multiple leave spells begin within several months of each other. We, therefore, also report descriptive statistics for all sickness leave spells at establishments with less than 100 workers in column 5. Among these smaller establishments, 16 percent of sickness leaves satisfy the clean definition.[21]

Even though they are only 28 percent of all spells, the characteristics of clean maternity leave spells are similar to those of the full sample. For both sets of spells, the top industries represented are wholesale/retail trade and manufacturing, and almost two-thirds of the workforce at the leave-taker's establishment is female. Maternity leave-takers, on average, have about 30 months of tenure at the start of their leave, and take the full amount of leave provided by law.[22] Women who take maternity leave are most often service workers and vendors or administrative workers. The most notable difference between the clean spell sample and the sample of all spells is the establishment size distribution. Clean spells tend to come from smaller firms with less than 100 employees, whereas about one-third of the leaves in the full sample originate from firms with 100 or more employees. This discrepancy arises largely from our definition of a clean spell. Even with independent timing of leave initiations, it is mechanically more likely that multiple spells begin within several months of each other in larger plants, and hence do not meet our clean criteria. Thus, our analysis largely reflects the responses of smaller firms to maternity leave-taking, but along most other dimensions, the clean leave spells are representative of maternity leaves taken during this period.

[21]Among establishments with less than 50 workers, 21 percent of sickness leaves meet our clean spell definition.

[22]More than 95 percent of clean maternity leaves last at least 120 days, with the vast majority lasting 120–124 days. The careful reader will note that the average leave duration among all spells is approximately 115 days versus 120 days for clean spells. This difference arises largely because leave durations are right-censored for spells that begin late in the year in 2017.

Turning to sickness leaves, the characteristics of clean spells are also quite similar to those of the full sample. For example, sickness leave-takers have on average 44–46 months of tenure at the start of their leave and come from plants and occupations where a little over 40 percent of workers are female. They also tend to be service workers and vendors, administrative workers, or workers in production and manufacturing. The average duration of a clean leave spell is 187 days. The median spell (not reported in the table) is 83 days. Recall that in Brazil, employers pay the first 15 days of sickness leave and do not have to report short spells in the RAIS, so our administrative data largely capture longer leave spells. Indeed, the top decile of the leave length distribution corresponds to more than one year, likely capturing what in many countries would be considered temporary disability.[23]

As is the case with maternity leave, the key difference between our analysis sample of clean spells and the full population of sickness leaves is the firm size distribution. The vast majority of clean sickness leave spells come from plants with fewer than 100 workers, whereas half of all sickness leave spells originate from establishments with 100 or more employees. Like with maternity leaves, we view our sickness leave analysis as being representative of responses to leave-taking among smaller firms. Our implied focus on smaller firms is not without precedent in this literature. For example, Brenøe et al. (2020) limit their sample to firms with 3–30 employees, noting that the impact of a single individual going on leave should be smaller at large firms and that much of the policy attention on leave centers on small firms. Jäger and Heining (2019) consider worker deaths at firms with 3–30 full-time employees, similarly noting that the impact of a worker death on firm or coworker outcomes decreases with firm size, making it difficult to detect an effect among larger firms. Furthermore, similar to our clean spell criteria, Jäger and Heining (2019) exclude firms with multiple worker deaths in a year and point out that worker deaths in larger establishments are more frequent due to the law of large numbers.[24]

In Appendix Figure A1 (a), we plot Kaplan-Meier survival curves for the maternity leave-takers in our estimation sample, where survival means the woman is still contracted with the plant. The x-axis reports months since the month maternity leave began, and the y-axis reports the survival probability. The survival probability declines sharply 5–8 months after maternity leave onset. About a year after leave onset, half of the leave-takers have separated from the plant. Thus, employment dynamics surrounding the initiation of a maternity leave

[23]Brazil has high rates of sickness absence due to muscular-skeletal disorders (Vieira et al. 2011) and mental disorders (Silva-Junior and Fischer 2014), both of which tend to be long-lasting.

[24]In addition, Gallen (2019) only considers firms where one woman gave birth between October 1, 2001 and March 31, 2002, and excludes firms with multiple births during that period.

may reflect not only firms' response to an almost certain 120-day absence, but also the likelihood of the woman separating from her job shortly after her leave and associated job protection end. Similarly, in Figure A1 (b), we show Kaplan-Meier survival curves for our sample of sickness-leave takers, where the x-axis reports months since the month prior to sickness leave ending.[25] The survival probability declines significantly within the six months after sickness leave ends. The reactions of firms to sickness leave, therefore, may capture their response to an absence of an uncertain duration as well as the possibility of the worker departing from the plant soon after the leave ends.

Finally, Table 3 presents summary statistics for the main plant-occupation outcomes we study—net employment change, number of hires, and number of separations. We also present the average number of contracted employees in levels (rather than changes) to provide a sense of plant-occupation size in our sample. The reported statistics are calculated three months preceding the start of a leave spell within the occupation of the leave-taker, which we refer to as the "own" occupation. We also report descriptive statistics in the same month for "spillover" occupations, which are other occupations at the same plant as the leave-taker.[26] On average, about 8–9 workers are employed in the plant-occupation of the leave-taker before leave starts. The average net monthly change in employment is close to zero. High turnover is common in the Brazilian labor market, and the descriptive statistics show there is a non-trivial amount of employee churn. In a given month, on average, 0.3–0.4 workers are hired in the leave-taker's occupation, and about the same number of separations occur, which explains the near zero net change in employment. Spillover occupations tend to be smaller and experience relatively less workforce turnover.

4 Empirical Methods

For our baseline analysis, we estimate the following event study model of employment dynamics:

$$y_{opt} = \phi_{op} + \tau_t + \sum_{k=-2}^{3} \beta_k \times \mathbb{1}(K_{pt} = k) + \varepsilon_{opt} \tag{1}$$

where y_{opt} denotes outcome y for 1-digit occupation group o at plant p at calendar time t. The main outcomes of interest are the number of workers hired during the month and the

[25] Given sickness leaves vary in duration and workers cannot be dismissed while on leave, we find it more informative to measure survival as of the month before the sickness leave ends rather than the month the leave starts.

[26] For each leave spell, the own occupation contributes one monthly observation to these statistics. We average over all 1-digit spillover occupations in the descriptive statistics, as there may be several non-leave-taking occupations within a given plant.

number of workers who separate during the month, as the firm can manage both of these margins. We also model net employment growth, which is simply the difference between new hires and separations. To simplify the discussion, we will often focus on the implied change in the level of employment based on our model of net employment growth.

The variable K_{pt} measures event time relative to the month a leave spell started at plant p.[27] That is, $K_{pt} = 0$ in the month of leave onset, $K_{pt} = 1$ in the month after the leave starts, etc. The variable ϕ_{op} denotes plant-occupation fixed effects and τ_t are calendar time effects (where time is measured in year-months). The coefficients of interest, β_k, represent the effects k months relative to the leave start. We normalize the effect three months prior to the leave start to zero ($\beta_{-3} = 0$), and cluster standard errors at the leave spell level. We primarily use equation (1) to track employment dynamics in the plant-occupation of the leave-taker. Later, we separately estimate equation (1) on other occupations within the same plant as the leave-taker to determine whether there are spillover effects. We estimate equation (1) separately for maternity leave and sickness leave spells.

Identification of equation (1) is based on strong but plausible assumptions about the timing of leave initiation relative to employment growth in the plant-occupation.[28] Ideally, we want to measure the difference in hiring, separations, and employment growth that can be attributed to a leave spell starting during the window. The counterfactual is the evolution of employment changes, hiring, and separations had a leave spell not been active during the event window. The event study identifies this contrast as long as the timing of leave spells is not associated with idiosyncratic employment dynamics. This will be the case when (i) workers do not time leave to begin when, say, the firm is experiencing especially large expansion or contraction, and (ii) firms cannot precisely control when workers take leave. Notably, identification of the event study coefficients does not require that leave onset is a surprise. The firm can be aware that its employees are at risk to take leave and have plans in place to handle leave-taking, but it cannot control precisely when it needs to put those plans in motion. Identification also requires that at least one of the pre-leave onset months be considered untreated. We assume firms do not react to leave-taking until two months before leave onset (i.e., they do not respond three months, $k = -3$, before the leave starts).

[27] Recall that clean maternity or sickness leave spells are defined such that only one leave spell of a given type can start in the plant during the event window.

[28] We also assume the effects of leave-taking on employment dynamics are the same regardless of the calendar date on which a leave spell starts (i.e., there is no cohort-specific treatment effect heterogeneity). Per Sun and Abraham (2020), violations of this assumption can manifest in pre-trends, as they show in settings with variation in treatment timing and cohort-specific treatment effect heterogeneity, event time coefficients can be contaminated by effects from other periods. We discuss this issue further in Section 5.3.1.

For sickness leave, an even stronger assumption that firms do not react until leave onset is reasonable. We justify these assumptions below. If these assumptions hold, the event study measures the firm's reactions around the realization of a predictable, but uncertain, event. If they do not, then the event study measures some combination of the firm's reaction and employment dynamics that lead workers to start a spell of leave.

These assumptions are more straightforward for spells of sickness leave. Although employers know there is a probability of a worker taking leave in any month, presumably they do not know exactly when that probability will be realized. Employers therefore do not know when a worker will go on sickness leave. Our model also implies workers do not time sickness leave with respect to time-varying plant-specific conditions, but rather, if anything, to labor market conditions, which are controlled for via calendar time effects. That sickness leave is essentially a surprise when it starts yields a testable implication that the firm's response should take place entirely after the leave spell begins. In subsequent sections, we provide empirical support for this notion.

By contrast, maternity leave is almost certainly not a surprise to the employer when it starts. Brazilian labor law requires women to formally notify their employer in advance of maternity leave, and job protection begins at that point, so women have a strong incentive to notify early. The timing of a woman's pregnancy may be endogenous to anticipated labor market conditions, but is unlikely to be timed with respect to plant-specific conditions at the time leave starts. More generally, it is difficult and uncommon to precisely time pregnancy. According to the 2006 Brazilian Demographic and Health Survey, about 54 percent of all births that happened five years prior to the survey were unintended, meaning mistimed or unwanted (Ministério da Saúde 2009).[29] In light of the above, we do expect that firms may respond in advance of a maternity leave start. As such, we do not expect the complete absence of a pre-trend or pre-leave onset effects. However, under the assumption that the firm's employment response begins closer to when leave starts, we have a testable implication that there should be no significant change in outcomes the more months we consider prior to maternity leave onset. If this assumption fails, we cannot distinguish anticipation effects from trends in outcomes arising from endogenous timing of maternity leave. In Section 5.3.1, we consider longer event windows and provide empirical support for this assumption.

Another technical issue arises with respect to our ability to separately identify event time effects from calendar time effects. Borusyak and Jaravel (2017) show that in a fully dynamic

[29] In the Birth in Brazil survey, which interviewed and examined medical records of 23,940 mothers from February 2011–October 2012, 55 percent of pregnancies were reported as unintended (Theme-Filha et al. 2016).

event study specification like equation (1), the linear trend in the path of causal effects (i.e., the β_k coefficients) is not identified. That is, one cannot separate the trend in outcomes surrounding the event from the trend in calendar time. The problem is one of normalization. Our main specification is similar to that described in Borusyak and Jaravel (2017), but different in ways that allow us to overcome the normalization problem. Specifically, we often see the same plant-occupation pair in the data multiple times (i.e., a plant-occupation can experience more than one clean leave spell during our sample period), which provides an additional source of variation to identify the plant-occupation effects relative to calendar time effects and the path of event time effects.[30] Separate identification of the trend in event time, thus, relies on the inclusion of plant-occupation fixed effects. If instead we want to include more granular event spell effects, we need an additional restriction or source of information to identify the trend in calendar time effects. The simplest solution is to include control groups, which we explore in Section 5.3.2.[31]

5 Results

We present the estimates of β_k from equation 1 and the corresponding 95 percent confidence intervals in figures, separately for maternity leave and sickness leave initiations. We display results for the following outcomes: net change in the number of contracted employees, number of hires, number of separations, and number of separations excluding the leave-taker.[32] For ease of interpretation, we also present the path of employment in levels using the estimated event time coefficients and standard errors from the specification where the change in employment is the outcome.[33] Note, as long as the leave-taker's contract with the establishment is active in a given month, they are considered employed (i.e., we do not consider

[30] 71 (70) percent of the clean maternity (sickness) leave spells come from plant-occupations that only contribute one spell to the sample; the remainder originate from plant-occupations that contribute multiple clean maternity (sickness) leave spells.

[31] Borusyak and Jaravel (2017) propose other approaches that replace assumptions about the appropriate control group with other modeling assumptions. One alternative involves normalizing two pre-treatment event time effects under the assumption that there are no pre-event trends. In our setting, we have only a handful of pre-leave onset months and we suspect there will be anticipation effects, particularly in the case of maternity leave. As another suggested alternative, if the timing of leave initiation is truly independent of time-invariant plant-occupation heterogeneity, the model can be estimated by mixed effects (i.e., random effects for unit-specific effects rather than fixed effects). Our results are not sensitive to including plant-occupation random effects or to allowing for more granular leave spell random effects.

[32] We also present the estimates for those four outcomes in Appendix Table A1. For all subsequent results, we only present figures in the interest of space.

[33] To determine how many more workers the plant-occupation has in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, we compute $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from the specification where change in employment is the outcome, and create the appropriate confidence intervals.

them separated unless their contract formally ends). In interpreting our results, it is helpful to keep in mind what employment dynamics would look like in a frictionless labor market. If the firm can costlessly replace the labor of the worker going on leave and workers are homogeneous, we would expect to see employment contracts increase by one as the firm hires a new worker to replace the leave-taker. Furthermore, we would expect to see this change exactly when leave begins, even if the firm perfectly anticipated the onset of leave.

5.1 Baseline Estimates

We first report results for maternity leaves. Figure 1 displays the estimated employment dynamics in the plant-occupation of the leave-taker around the start of maternity leave. All effects are estimated relative to three months prior to the start of the leave. Figures 1 (a) and (b) show plant-occupations experience relatively small but statistically significant increases in employment two months before leave onset, and more sharp increases as leave onset approaches. Specifically, the plant-occupation is 0.06 workers larger the month before leave onset and 0.13 workers larger the month of leave onset relative to three months prior. The plant-occupation continues to grow and remains around 0.2 workers larger three months after leave initiation. Figures 1 (c) and (d) show how the employment adjustment is managed through hiring and separations, respectively. Firms increase the number of hires throughout the event window, with the largest increase occurring in the month of leave onset and the next largest increases taking place in the one month before and after the leave starts. The increased hiring prior to leave onset suggests firms respond in anticipation of the woman's absence, an interpretation we explore in more detail in Section 5.3. Separations decay slightly in the months approaching the start of the leave spell, suggesting firms may increase efforts to retain incumbent workers. In the two and three months following leave onset, there is a small and statistically significant increase in separations. In Figure 1 (e), we present results for separations excluding the leave-taker, and find nearly identical increases after leave onset. The increase in worker exits could reflect the separation of the leave-taker's replacement as the return of the leave-taker draws near, recalling that most maternity leaves last 120 days. The typical probationary period for a worker is 90 days and firing costs for the firm increase thereafter. Thus, the increase in separations may also capture firms shedding replacement workers before the probationary period ends.

We contrast these results with the employment dynamics observed around the onset of sickness leave, which are reported in Figure 2. Notably, we do not find significant changes in any of the outcomes we consider two months preceding the start of the leave, with point estimates very close to zero. We also find little economically meaningful change in employment the month before sickness leave onset, with plant-occupations adding 0.006 workers on net (Figure 2 (a)). These results provide support for our identifying assumption that the start of sickness leave is not strategically timed based on idiosyncratic plant dynamics, and they are also consistent with the exact start of a sickness leave coming as a surprise to the firm. Figures 2 (a)–(c) show that in the month of leave onset, the plant-occupation sees an increase in employment, driven by a sharp uptick in the number of hires. Hiring and employment growth continue one month after the start of the leave. Specifically, the plant-occupation is 0.04 workers larger in the month the leave starts and 0.06 workers larger the following month relative to three months before leave onset. The number of separations steadily increases after the leave initiates, peaking at 0.045 three months after the onset of leave (Figure 2 (d)). When we consider separations excluding the leave-taker (Figure 2 (e)), they increase and plateau around 0.018–0.019. Thus, the increase in separations reflects, in part, the leave-taker separating from the establishment and potentially the leave-taker's replacement separating in cases where the sickness leave is of a relatively short duration. By three months after the start of the sickness leave, employment at the plant-occupation has almost returned to its pre-leave level.

In sum, the results for both maternity and sickness leave are not consistent with a frictionless labor market model with homogeneous labor. We do not see one-for-one replacement of the leave-taker in the month of leave onset. Instead, our findings are more consistent with firms handling the costs of disruption associated with predictable, but uncertain, worker absence through channels other than hiring from the external market (e.g., by building redundancy). Furthermore, the relatively larger employment and hiring responses to maternity leave compared to sickness leave likely reflect the different nature of those absences. In the case of maternity leave, firms are typically aware of the absence in advance and know it will almost certainly last 120 days, which may allow them more time and preparation to hire from the external market. The near certainty of the leave length may also make it easier for firms to decide whether it is cost-effective to hire a replacement or mitigate the labor supply disruption in other ways. By contrast, sickness leave is more sudden and uncertain in duration, which may make it difficult for firms in the face of hiring frictions to determine whether and when to replace the leave-taker.

5.2 Spillovers Across Occupations

So far, we have focused on employment dynamics in the 1-digit occupation of the leave-taker. However, the firm might manage leave-taking by hiring workers in a closely-related occupation. It is also possible the absence of workers in supervisory roles (e.g., managers) impacts subordinate workers. Figure 3 displays the effects on occupational employment when a worker from the same plant but a different occupation goes on leave. We display results by the coarse occupation (i.e., managerial, technical, production) of the leave-taker and the coarse occupation category of the other 1-digit occupations at the plant. We present the results broken down this way as there could be complex substitution patterns and complementarities across occupations that get masked by pooling the data. For brevity, we focus on employment in levels in the main text, but estimates for all outcomes can be found in Appendix Figures A2 and A3.

The results show little evidence of statistically significant or economically meaningful spillovers of maternity leave-taking. There is some weak evidence of employment growth in production and technical occupations prior to the start of a manager's maternity leave, though the standard errors are large. This result could reflect that workers replacing managers are sometimes hired in at other levels.[34] That the most notable spillovers prior to maternity leave-taking occur when managers take leave is perhaps not surprising, as women with leadership positions within firms may be more deliberate about timing their absence. When we consider spillover effects of sickness leave-taking, we generally find no statistically significant effects on non-leave-taking occupations, including when managers take leave.

The spillover analysis yields some important insights. First, the effects of leave-taking are concentrated within the 1-digit occupation of the leave-taker, with little to no spillover effects on other occupations in the plant.[35] Second, the absence of spillover effects, particularly prior to leave onset, is consistent with our identifying assumptions. If workers time leave-taking to coincide with plant-specific business conditions, then, to the extent that employment dynamics within the same plant are similar across occupations and reflect those same conditions, we would expect to see spurious effects of leave-taking in other occupations. We find little to no evidence of such effects, which also supports our use of these occupations as control units in our robustness exercises in the next section.[36]

[34]Or, incumbent workers are promoted to managers, and other workers are hired to replace them further down the promotion ladder. Given the imprecision of our estimates of the first-order spillover effects from leave-taking managers, we do not examine more complex patterns of internal promotion dynamics in response to leave-taking.

[35]Similar to our results, Brenøe et al. (2020) find the effects of parental leave on coworkers are driven almost entirely by those in the same occupation as the leave-taker.

[36]The lack of spillovers suggests it may be reasonable to define clean spells at the plant-occupation level, rather than the plant level. In Appendix Figures A4 and A5, we present estimates of equation (1) where a clean maternity (sickness) leave is one where the event window centered on the month of initiation does not intersect the event window of another maternity (sickness) leave spell at that plant-occupation. Results are nearly identical to our baseline estimates.

5.3 Robustness

Our main estimating equations are identified under the assumptions that (i) leave is not timed to coincide with periods of accelerating or decelerating plant-occupation employment growth and (ii) employment outcomes three months prior to leave initiation are not correlated with leave-taking (i.e., they are untreated).[37] In this section, we show that our baseline results are robust to relaxation and modification of these assumptions and to alternative specifications.

5.3.1 Evidence on Pre-Leave Trends

Two closely-linked concerns with our analysis are the relatively short pre-event period and the assumption that employment outcomes are untreated three months before leave initiation. These concerns are especially salient for our maternity leave analysis. We know employers are aware of maternity leave spells well before they occur, and expect that they will alter hiring and separation decisions in advance. Figure 1 shows a statistically significant increase in hiring and employment in the two months prior to the leave start. Those pre-leave effects could reflect anticipatory behavior by the employer, which we want to measure. However, they may instead reflect selection effects if women time maternity leave to coincide with an acceleration of employment growth.[38] We cannot formally distinguish these explanations, but if pre-leave responses reflect anticipatory behavior, we expect them to be concentrated right before leave onset, and not several months prior. If they reflect selection effects, we expect to see them even when we consider more months prior to leave onset.

To explore the above idea, we estimate models with longer event windows of four and five months around leave initiation.[39] This exercise also allows us to gauge the reasonableness of our assumption that outcomes three months prior to leave onset are untreated. In the interest of space, we focus on implied employment levels and some other notable results from specifications that extend the event window to five months, which are displayed in Figure 4. The results for all outcomes from the extended event window analyses are presented in Appendix Figures A8–A11. In all cases, we normalize the earliest event time coefficient to zero. For maternity leave spells, the analysis confirms that there is no significant or

[37] Again, in the case of sickness leave, it is reasonable to impose an even stronger assumption that outcomes in all months before leave onset are uncorrelated with leave-taking.

[38] As mentioned earlier, Sun and Abraham (2020) show that pre-trends can arise in settings with variation in treatment timing when there is cohort-specific treatment effect heterogeneity. Therefore, some of the increase in hiring prior to maternity leave onset may reflect such heterogeneity. However, in Appendix Figures A6 and A7, we show that estimated treatment effects (i.e., the event time coefficients) in the first half of our estimation period are indistinguishable from those in the second half.

[39] For the 4-month window, we consider the four months before, the month of, and the four months after leave onset. Likewise for the 5-month window.

economically meaningful increase in the level of employment three or more months before the leave starts (relative to the baseline period). For example, three months before leave onset, the plant-occupation is less than 0.02 workers larger than five months before the leave start. There are then sharper employment upticks two months and one month before leave onset similar to our baseline results. Overall, the analysis supports our assessment that the employment dynamics in the months before maternity leave reflect anticipatory behavior. If selection effects exist, they are negligible.[40] Furthermore, our assumption that outcomes three months prior to leave onset are untreated appears reasonable.

The results for sickness leave fully support the idea that firms are either surprised when workers go on leave, or, if they do anticipate sickness leave, they do not respond in advance. Almost all the event time coefficients prior to leave onset are very close to zero and statistically insignificant. The lone exception is in the 4-month window analysis, there is a less than 0.01 increase in hiring the month before sickness leave begins. In the month of leave onset and the months after, employment dynamics are qualitatively similar to those implied by our baseline estimates.

In addition to bolstering support for our identifying assumptions and establishing the robustness of our estimates, the expanded event window analysis reveals some notable employment dynamics beyond three months after leave onset, especially for maternity leave. In particular, we detect a relatively large and statistically significant increase in separations four and five months after leave initiation, which is predominantly driven by the leave-taker separating from the firm, presumably after the full amount of leave and job protection allowed by law (Figures 4 (c) and (d)). These results are consistent with the survival functions shown earlier which highlighted that women often separate soon after their maternity leave ends. For sickness leave, the separation results from longer event windows (Figures 4 (e) and (f)) underscore that separations driven by the leave-taker remain high months after the leave starts. Thus, the employment dynamics surrounding the initiation of both maternity and sickness leave likely reflect the strong possibility that the leave-taker will permanently separate from the firm after their leave ends.

Note that we prefer not to use 4-month or 5-month windows throughout the analysis

[40]We also re-estimated our baseline maternity leave specifications excluding maternity leaves that began soon after the woman took a sickness leave. About 9 percent of clean maternity leaves are preceded by a sickness leave that ends within 30 days of the start of the maternity leave. Such cases might arise for a variety of reasons, including a difficult pregnancy or the woman using sickness leave as a form of antenatal leave. The results are quantitatively similar to our baseline estimates, suggesting the pre-leave responses indeed reflect anticipatory behavior of the firm, not a response to some women being absent even before their maternity leave begins. Results are available by request.

as they further restrict the sample. Because we require that leave spells be clean, the data for the 4-month and 5-month windows are nested subsets of the main analysis sample. For example, when we use the 5-month window, we require that no other maternity (sickness) leaves began at the plant in the ten months before the leave started and the ten months after it started. We also require that the leave-taker have a minimum amount of tenure such that their hiring is not reflected in the estimated employment dynamics.[41]

5.3.2 Alternative Specifications

We next explore the sensitivity of our estimates to alternative specifications that relax our assumptions about the timing of leave relative to employment dynamics.[42] Our baseline specification includes calendar time fixed effects to account for the possibility that individuals time their leave based on aggregate labor market conditions. In Figures 5 (a) and (b), we compare the baseline estimates to estimates from models that include industry-specific calendar time effects and models without calendar time effects.[43] For both types of leaves, not including calendar time effects leads us to underestimate effects on employment. When we include industry-specific time fixed effects, the estimates are nearly identical to our baseline results, alleviating concerns that leave-takers time the start of their leave to fluctuating industry conditions.

Our baseline model is an event study with no control units. In a model with leave spell-specific fixed effects and no control units, Borusyak and Jaravel (2017) show it is not possible to separately identify the trend in calendar time from a trend in event time. Technically, our model does not suffer from this problem because our baseline specification includes plant-occupation fixed effects and the same plant-occupation can appear for multiple spells. Our baseline model, therefore, relies on the assumption that plant-occupation effects do not change across leave spells.

We consider alternative specifications that relax this somewhat arbitrary assumption. We cannot simply estimate our baseline model with spell-specific effects, as this would raise the indeterminacy issue noted by Borusyak and Jaravel (2017). Instead, we estimate our baseline

[41] About 75 percent of the maternity leave spells in our baseline analysis meet the requirements for the 4-month window analyses; 58 percent meet them for the 5-month window. Likewise, 75 percent of the sickness leave spells in our baseline analysis meet the requirements for the 4-month window; 57 percent meet them for the 5-month window. For the sake of comparison, we present the 3-month window analysis using the spells in the 4-month and 5-month window analysis samples in Figures A12 and A13.

[42] Again, to keep the presentation concise, we report results for employment in levels in the main text. Complete results of these robustness exercises appear in Appendix Figures A14–A17.

[43] Industries are defined using the 21 major sectors in the *Classificação Nacional de Atividades Econômicas* (CNAE) 2.0.

specification and include non-leave-taking occupations at the plant where the leave occurred as control units. We use these non-leave-taking occupations (i.e., what we previously called spillover occupations) as control units because results from Section 5.2 revealed the effects of leave-taking are concentrated within the 1-digit occupation of the leave-taker, with little to no spillover effects on other occupations in the plant, especially prior to leave onset. Figures 5 (c) and (d) show estimates for employment levels first with plant-occupation fixed effects, and then with richer plant-occupation-spell fixed effects. Both specifications are fully identified since we have control units to separately identify the calendar time effects.

For both maternity and sickness leave, the estimated employment dynamics are qualitatively very similar to those implied by our baseline estimates. The employment response is somewhat smaller when we include control occupations, with the gap between the baseline estimates and estimates from the inclusion of control groups growing over time. Notably, when focusing just on the models with control groups, the inclusion of plant-occupation-spell fixed effects yields estimates that are quantitatively very close to those from models with less granular plant-occupation fixed effects. Thus, our assumption that plant-occupation effects are stable across leave spells appears reasonable.[44]

5.3.3 The Influence of Informal Workers

The Brazilian context raises a concern about construct validity. Informal contracts account for roughly 40 percent of total employment (Bosch and Esteban-Pretel 2012). The RAIS data only cover formal employment; thus, our estimates may not fully capture firms' responses to leave-taking if part of their response involves hiring informal workers. But, there are reasons to suspect that use of informal contracts does not drive our results. First, firms primarily use informal contracts to avoid the termination costs described in Section 2.3, and there is evidence that workers on informal contracts trade off the benefits of formal employment against higher wages (Almeida and Carneiro 2012). There is nothing about hiring to replace leave-takers that makes informal contracts more useful from the firm's perspective. For example, the firm could formally hire someone with the usual 90-day probationary period and avoid termination costs as long as that worker is terminated within the 3-month limit. Second, informal contracts are least likely to be used in managerial occupations, and we

[44] Instead of adding control groups, we can impose other assumptions to overcome the normalization problem induced by including plant-occupation-spell effects. If event time effects are truly independent of spell-specific time-invariant unobserved heterogeneity, we can model spell-specific heterogeneity by random effects rather than fixed effects. Appendix Figures A18 and A19 show estimated employment dynamics from models with plant-occupation-spell random effects (without control units). The estimates are a little smaller in magnitude than the baseline estimates, but generally quite similar. These results further support the notion that leave spells are timed randomly with respect to plant-occupation outcomes.

show later in Section 6.1 that the weakest formal sector responses occur when managers go on leave. It seems unlikely that widespread use of informal contracts would explain these results.

To further investigate the importance of informal contracts, we bring in plant-level data on labor inspections from 2003–2011. For each inspection, these data record the month the inspection began, the plant being inspected, each aspect of the labor law that was inspected, and whether the plant was found to be in violation of the relevant law.[45] We first compare employment dynamics in the occupation of the leave-taker for firms that were ever found to have informal workers present between 2003–2011 and those that were not. We only consider firms that were specifically inspected for potential employee registration violations, so they should be similar on characteristics that predict the use of informal contracts.[46]

Figure 6 (a) shows the results for employment levels for maternity leave spells.[47] The employment dynamics are nearly identical across firms regardless of whether they have had informal worker violations. Figure 6 (b) presents the analogous exercise for spells of sickness leave. The patterns are largely the same, but more muted. If informality were a major factor, we would expect firms with a history of informality citations to have a smaller observed employment response to leave-taking, since they presumably use informal workers as a substitute for hiring formal workers. The data show, if anything, the opposite pattern. The level of employment after leave starts is slightly, though not significantly, lower for firms that were never found to have an informality violation.

These analyses are not dispositive of the implications of informal sector employment for our analysis of employer responses to leave. However, they do provide some evidence that our results would not be substantially different were we able to observe both informal and formal contracts. Having thus demonstrated robustness of our baseline results, we conclude that employers do indeed respond to new spells of maternity and sickness leave by hiring new workers; that on average their hiring responses are not nearly sufficient to replace the labor of the leave-taker; and finally, that there are no substantial spillover effects onto hiring in other occupations.

[45] These data are different than the data used by Almeida and Carneiro (2012), which measure aggregate state-level inspection activity.

[46] We present plant characteristics associated with our sample of clean leaves separately by inspection status in Appendix Table A2. Of note, larger establishments are more likely to ever be inspected and to have informal worker violations.

[47] Again, in the interest of space, we focus on employment in levels, but results for all outcomes are presented in Appendix Figures A20 and A21.

6 The Influence of Job Characteristics and Market Conditions

The timing of observed employment responses to leave-taking suggests that employer reactions are immediate. However, the magnitudes suggest that most often firms do not directly respond by hiring workers from the external market to replace the absent labor. It will be more cost-effective to hire from the external market when it is easy to integrate new hires into production and when markets for replacement labor are thick.

6.1 Heterogeneity by Occupation

In the absence of frictions, employment should increase by one in the month of leave onset to replace the departing worker. Our results are clearly inconsistent with such a framework. To explore the nature of possible frictions affecting employment dynamics around leave-taking, we report estimates from our baseline model separately by coarse occupation groups that distinguish managerial, technical, and production workers.[48] The maternity leave results in Figure 7 show qualitatively similar patterns for the change in the number of contracted employees and the number of hires in the occupation of the leave-taker across the three coarse occupational groupings. However, the responses among managerial occupations are far more muted, especially relative to production occupations. This may reflect that managers are more difficult to replace, and therefore, there is limited scope for adjustment when a manager takes maternity leave. On the other hand, hiring a replacement for a production worker may be relatively easier as skills are likely more general, hence the larger employment and hiring response.

When we consider heterogeneity in the responses to sickness leave-taking by coarse occupation groups in Figure 8, we again find that the hiring and employment responses among managerial occupations are muted relative to production and technical occupations. Managerial occupations also experience fewer separations, especially of the leave-taker, after the sickness leave starts. The smaller employer response to managerial sickness leaves, therefore, is consistent with managers being harder to replace, but could also be explained by

[48] As discussed in Section 3, coarse occupation groups are based on 1-digit occupation codes using the CBO-2002 classification system and hierarchy, which groups occupations by similarity of tasks and required skill. Managerial occupations are those with CBO code 1 (e.g., public administration and management). Technical occupations correspond to codes 2 and 3 (e.g., artists, scientists, mid-level technicians). Production occupations are associated with codes 4–9 (e.g., administrative workers, service workers and vendors, agriculture and forestry workers, fisherman, repair and maintenance workers, and those in production and manufacturing work).

managers being relatively more attached to the firm and employers not needing a permanent replacement.

6.2 Heterogeneity by Local Labor Market Thickness

The results above indicate that firms are more able to use external markets to replace workers in occupations where skills are likely more general. A similar logic suggests that firms operating in thick labor markets where there are many replacement workers will be more likely to hire from external markets upon worker departure. We explore this possibility by re-estimating our baseline model separately for occupations in markets with different levels of thickness. We divide markets into terciles on the basis of a thickness measure that captures the availability of workers in the same occupation. Specifically, following Jäger and Heining (2019), we break Brazil into 137 mesoregions, and for each 1-digit occupation, we measure the share of mesoregion employment in that occupation relative to its share in the state where that region is located.[49] A thicker market means there is a relatively higher concentration of workers in the relevant occupation in that local labor market.

We present the results for maternity and sickness leave in Figures 9 and 10, respectively. For maternity leaves, there is some evidence that the hiring and employment responses are smaller in the thinnest markets. We also observe that in the thinnest markets, firms are more likely to reduce separations in the month before leave starts, indicating that these firms do more to retain incumbent workers. For sickness leaves, Figure 10 shows substantial contrasts in employment dynamics by market thickness. In the thickest markets, the employment response peaks at a level almost twice that observed in thinner markets. Overall, these results imply the agglomeration of similar types of labor in local markets increases the firm's ability to hire replacement workers from the external market. Nevertheless, even in the thickest labor markets, we find far less than one-for-one replacement, suggesting firms largely handle absence through channels other than hiring from the external market.

6.3 Heterogeneity by Leave-Taker Tenure

We expect that firms' difficulty in replacing workers will also depend on their firm-specific experience. We examine this possibility by considering heterogeneous employment dynamics for leave-takers with different levels of tenure on the job. The RAIS data include the exact date of hire for each job and the number of months the job has been active. Using this

[49]For official statistics, the lowest level of geography is a municipality which is generally too small to use as a local labor market. A mesoregion is a collection of municipalities that share common characteristics. This geographic coding is taken from Brazil's *Instituto Brasileiro de Geografia e Estatìstica* (IBGE).

information, we group workers into terciles based on their tenure in the month that the leave begins. For maternity leave-takers, jobs in the first tercile have tenure of 14 months or less; jobs in the second tercile have between 14 and 30 months of tenure; jobs in the third tercile have more than 30 months of tenure. For sickness leave-takers, jobs in the first tercile have 16 months of tenure or less; jobs in the second tercile have between 16 and 41 months of tenure; jobs in the third tercile have more than 41 months of tenure.

Figure 11 shows that hiring and employment responses to maternity leave are substantially larger when the worker is in the first tercile of the pre-leave tenure distribution (i.e., the less tenure the leave-taker has). Separations also fall in the months before leave starts, but only for these short tenure jobs. These results are consistent with workers with less tenure being easier to replace via external hiring or the labor of coworkers compared to cases where a worker with more tenure goes on leave.

The patterns are different for sickness leave, as illustrated in Figure 12. The employment and hiring responses are more muted when the leave-taker has less tenure, and there is a sharp increase in separations after the sickness leave starts that is most pronounced when the leave-taker has less tenure. The increase in exits is largely driven by the leave-taker's departure. For absences where the leave-taker's tenure falls in the second and third terciles, the employment dynamics are very similar, with increases in the number of contracted workers, particularly in the month of leave onset and the month that follows. Three months after sickness leave onset, in cases where the leave-taker has very low tenure, the occupation is almost 0.1 workers smaller relative to three months prior to the leave start, while occupations where the leave-taker has relatively high tenure are 0.1 workers larger.

These findings indicate that something else drives employment dynamics when workers take sickness leave very early in their tenure with the firm. Workers with long tenure may be well-matched to their employer, whereas employers may be less certain about workers with short tenure. An early sickness leave may indicate that a worker is about to change jobs or be interpreted as a negative signal by the employer. The data also indicates that workers with less tenure tend to take shorter leaves, which could influence employers' responses. We explore this idea next.

6.4 Heterogeneity by Sickness Leave Duration

Employment responses to sickness leave may be muted if leaves are often too short to bother hiring a replacement worker. We examine this possibility by separately estimating employment dynamics around sickness leaves of different lengths. We compute the completed duration of sickness leaves and group them into terciles. Sickness leaves in our data tend to be long: the first tercile includes leave spells of up to 54 days, and the second tercile includes spells between 55 and 139 days. Given we condition on a post-determined outcome, we interpret the results of this exercise as descriptive.

Figure 13 shows that in the month of leave onset, the change in the number of contracted employees is fairly similar regardless of duration as the plant-occupation of the leave-taker adds 0.02–0.04 workers on net, driven by a 0.04 increase in the number of hires across all leave durations. However, in the months after leave onset, when the leave is of a shorter length, the plant-occupation experiences a net decrease in the number of workers, driven by a sharp increase in separations. The increase in separations reflects the departure of the leave-taker. For spells in the first tercile, where the spell ends within 54 days, we see separations spike two months after leave begins. However, when we exclude the leave-taker, this spike disappears. A similar pattern is present for leave spells with durations in the second tercile. There we see a spike in separations three months after leave begins. Altogether, the evidence suggests that employment dynamics around sickness leave, regardless of duration, often involve a permanent departure, and the need for a permanent replacement.

7 Worker Earnings and Establishment Payrolls

To complement our analysis of employment dynamics, we examine how maternity and sickness leave-taking affect firms' labor costs.[50] Our ability to analyze labor costs is somewhat limited as we only have data on monthly compensation from 2015–2017. For those years, we observe the actual total compensation paid to the worker in each month, which may be distinct from their contracted monthly salary. Using this information, we construct monthly measures of the wage bill in each plant-occupation.[51] We focus on the monthly wage bill including the earnings of all contracted workers except during periods of leave as well as the monthly wage bill excluding the earnings of the focal leave-taker for the entire event window.[52,53] Given the limited years available for this analysis, it is rare to observe the same plant-occupation contribute more than one clean leave, making separate identification of event time, calendar time, and plant-occupation effects challenging. We therefore estimate

[50] Unfortunately, we cannot examine how work hours change in the months surrounding leave onset as the data do not provide a reliable measure of hours (or days) worked.

[51] We first winsorize the individual monthly earnings at the 99.5 percentile.

[52] Recall, the government funds the maternity leave payments in full for the first 120 days (180 days for those in the EC program), and funds sickness leave payments after the first 15 days. By excluding the earnings of leave-takers in the months they are on leave, we may understate the wage bill, particularly right at the start and end of their leave.

[53] Appendix Table A3 reports descriptive statistics for this sample separately by leave-taking occupations and control occupations three months prior to leave onset.

specifications similar to those in Section 5.3.2, using non-leave-taking occupations in the plant where the leave occurred as control groups.[54]

Figure 14 (a) shows that prior to maternity leave onset, there is little economically meaningful change in the wage bill of the occupation of the leave-taker relative to other occupations at the same plant. In the month leave begins, the wage bill drops by approximately 1200 reais relative to three months prior to leave onset, and continues to stay about 1150 reais lower for the following months. This sustained drop in the wage bill reflects that maternity leave typically lasts 120 days, and the woman's salary is paid by the government during that time. In Figure 14 (b), we exclude the leave-taker's earnings for the whole event window, and find that the net wage bill follows dynamics similar to the path of employment. The wage bill rises by less than 100 reais in the months before leave onset, and by two months after the leave starts is 210–250 reais higher than at baseline. These earnings increases likely reflect hiring of external workers, and possibly intensive margin adjustments if firms increase the work hours or remuneration of incumbent workers.

Turning to sickness leave, Figure 14 (c) shows that similar to maternity leave, the wage bill of the leave-taker's occupation is stable prior to leave onset, but then drops sharply by almost 1400 reais at the start of the leave. The wage bill remains lower compared to its pre-leave level, but begins to increase, which could reflect the leave-taker him/herself returning to work among other margins of adjustment. When we exclude the focal leave-taker's earnings for the full event window, Figure 14 (d) shows that the wage bill is about 70–90 reais larger in the month of leave onset and the one and two months following. Overall, our results suggest the plant-occupation's labor costs (exclusive of leave payments funded by the government) decrease after leave onset and remain lower for the next several months.

8 Conclusion

In Brazil, firms do not respond to a leave spell by simply hiring a replacement worker right at the onset of the leave-taker's absence. Firms add, on average, up to one-fifth of a worker to replace an employee departing on maternity leave. At the start of sickness leave, firms add under one-tenth of a worker. These small average responses mask considerable heterogeneity arising from differences in the labor markets in which firms operate. Hiring responses are

[54]Furthermore, we cannot use all clean spells that begin during the 2015–2017 period as we also need to observe earnings in the three months before and after the month of leave onset. In the data used for the earnings analysis, there are 501,607 (522,040) clean maternity (sickness) leave spells, and 88 (86) percent come from plant-occupations that only contribute one clean spell during the period. When we estimate the wage bill models without control groups, our results are qualitatively similar.

more pronounced for absences arising in production and technical occupations, in thicker labor markets, and when the absent worker has less tenure. These results are consistent with the notion that it is difficult to replace labor via external hiring when the absent worker is relatively less substitutable due to specificity of skill or firm-specific human capital or when there is little agglomeration of similar labor in the local market. The lack of one-for-one replacement hiring suggests firms take an active role in managing internal labor markets in anticipation of labor supply disruptions that are predictable, but uncertain in their timing.

Our analysis has centered on employment dynamics before and after the onset of leave. There are other margins on which firms might adjust. Brenøe et al. (2020) find some evidence that firms respond to parental leave-taking by changing hours of incumbent workers. Jäger and Heining (2019) detect no increase in incumbent hours after a coworker death, though they do see an increase in coworker wages. Given the minimal amount of external hiring we observe, it seems possible firms rely on incumbent workers to mitigate temporary labor disruptions. The extent to which incumbent hours can be adjusted is limited in Brazil, however, given the maximum number of working hours per week is 44, the maximum length of a continuous shift of work is six hours, and the minimum overtime remuneration is 1.5 times the normal wage. Collective bargaining agreements may tighten these restrictions further.

We contribute to a small but growing literature studying the effects of family leave policies on firms and coworkers. In the presence of market frictions, firms may respond to leave mandates by adopting more flexible and leave-friendly personnel management practices. However, they might also respond by avoiding hiring workers likely to take leave, or by coercing eligible workers to not take up leave for which they are eligible. Understanding the channels through which firms adjust to labor supply disruptions is an important topic for further research.

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Tables and Figures

Table 1: CBO-2002 Major Occupation Group Classifications

Code	Title
0	Police and Military
1	Public Administration and Management
2	Professionals in Science and Arts
3	Mid-level Technicians
4	Administrative Services
5	Service Workers and Vendors
6	Agriculture, Fishing, and Forestry
7	Production 1
8	Production 2
9	Repair and Maintenance

Note: The table displays English translations of major occupation group classifications from the 2002 vintage of the *Classificação Brasileiro de Ocupações* (Ministerio do Trabalho 2002). The first digit of the 6-digit occupation code indicates the major occupation group.

Table 2: Descriptive Statistics of Maternity and Sickness Leave Spells

	Maternity		Sickness		
	Clean	All	Clean	All	All Small
	(1)	(2)	(3)	(4)	(5)
Plant Characteristics					
Industry					
Agriculture/Forestry/Fishing	0.018	0.015	0.062	0.037	0.049
Mining	0.001	0.002	0.004	0.008	0.005
Manufacturing	0.105	0.160	0.135	0.285	0.163
Utilities: Electric/Gas	0.001	0.001	0.001	0.005	0.003
Utilities: Water/Sewage/Waste	0.002	0.003	0.004	0.012	0.006
Construction	0.017	0.015	0.039	0.038	0.044
Wholesale/Retail Trade	0.429	0.317	0.367	0.209	0.355
Transportation/Storage/Mail	0.026	0.026	0.050	0.066	0.057
Accommodation/Food	0.098	0.074	0.082	0.034	0.063
Information/Communication	0.015	0.022	0.013	0.016	0.016
Financial Services	0.032	0.034	0.022	0.038	0.067
Real Estate	0.007	0.004	0.005	0.002	0.003
Professional/Scientific/Technical	0.042	0.032	0.029	0.012	0.018
Administrative Activities	0.054	0.104	0.075	0.122	0.056
Public Admin/Defense/Social Security	0.000	0.001	0.000	0.002	0.001
Education	0.046	0.058	0.032	0.021	0.028
Health/Social Services	0.057	0.090	0.033	0.070	0.034
Art/Culture/Sports	0.009	0.006	0.008	0.004	0.005
Other Service Activities	0.041	0.036	0.039	0.020	0.028
# of Workers on Dec 31					
0	0.020	0.018	0.009	0.010	0.026
1 to 4	0.267	0.121	0.288	0.043	0.114
5 to 9	0.245	0.119	0.263	0.048	0.129
10 to 19	0.223	0.133	0.234	0.069	0.184
20 to 49	0.170	0.155	0.164	0.109	0.293
50 to 99	0.049	0.095	0.035	0.095	0.255
100 to 249	0.020	0.103	0.007	0.142	0.000
250 to 499	0.004	0.068	0.000	0.117	0.000
500 to 999	0.001	0.059	0.000	0.114	0.000
1000+	0.000	0.129	0.000	0.254	0.000
Female Employee Share	0.645	0.623	0.432	0.421	0.437
	(0.286)	(0.249)	(0.342)	(0.279)	(0.300)
Female Aged 21–35 Employee Share	0.405	0.371	0.211	0.216	0.228
	(0.271)	(0.220)	(0.236)	(0.171)	(0.203)
Plant-Occupation Characteristics					
Female Employee Share	0.776	0.717	0.434	0.428	0.444
	(0.260)	(0.246)	(0.405)	(0.326)	(0.359)
Female Aged 21–35 Employee Share	0.528	0.455	0.201	0.218	0.227
	(0.322)	(0.272)	(0.285)	(0.210)	(0.255)
Leave-Taker Characteristics					
Avg Tenure (Months)	30.417	31.293	44.745	45.750	44.051
	(29.928)	(31.812)	(54.118)	(67.530)	(64.319)
Avg Leave Length (Days)	120.425	115.087	187.124	100.108	131.210
	(21.918)	(32.727)	(279.548)	(285.946)	(311.525)
Female	1.000	1.000	0.460	0.483	0.488
1-Digit Occupation					
Public Administration & Management	0.052	0.042	0.038	0.014	0.025
Professionals in Science and Arts	0.074	0.103	0.038	0.045	0.039
Mid-level Technicians	0.072	0.098	0.057	0.084	0.058
Administrative Services	0.359	0.334	0.177	0.227	0.244
Service Workers & Vendors	0.363	0.291	0.353	0.233	0.304
Agriculture, Fishing, & Forestry	0.011	0.011	0.056	0.046	0.043
Production/Manufacturing I	0.055	0.095	0.217	0.248	0.215
Production/Manufacturing II	0.012	0.025	0.033	0.079	0.048
Repair & Maintenance	0.002	0.003	0.031	0.026	0.025
# of Leaves	1113037	3993954	1174222	19673100	7348795

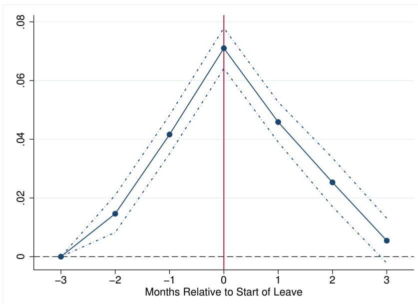
Note: All statistics are measured in the month of leave onset with the exception of establishment size, which is measured at the end of the calendar year of the leave initiation. Columns (1) and (3) include clean maternity and sickness leave spells, respectively. Columns (2) and (4) include maternity and sickness leave spells, respectively, regardless of whether they meet the clean definition as long as the other sample selection criteria are met. Column (5) includes the subset of spells from column (4) at establishments with less than 100 contracted employees at the end of the calendar year. Standard deviations of non-categorical variables are reported in parentheses.

Table 3: Pre-Leave Plant-Occupation Employment Descriptive Statistics

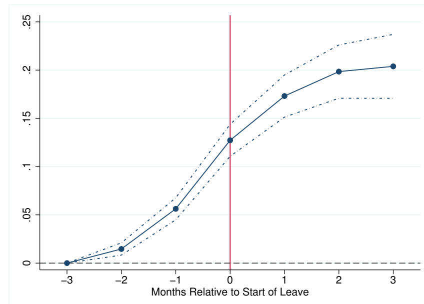
	Maternity		Sickness	
	Own Plant-Occ	Spillover Plant-Occ	Own Plant-Occ	Spillover Plant-Occ
Change in # of Contracted Employees	-0.001	-0.007	-0.011	-0.004
	(2.475)	(3.201)	(1.541)	(1.476)
# of Contracted Employees	8.866	6.659	8.196	4.263
	(20.781)	(22.278)	(12.975)	(8.727)
# of Hires	0.372	0.270	0.323	0.148
	(1.979)	(3.837)	(1.369)	(0.874)
# of Separations	0.373	0.277	0.334	0.152
	(2.295)	(3.978)	(1.414)	(1.434)
# of Plant-Occupations	1113037	1900599	1174222	1795339

Note: Statistics are measured three months prior to leave initiation. Own plant-occupations refer to the plant-occupation of the leave-taker, and spillover plant-occupations refer to other occupations in the same plant as the leave-taker. Standard deviations are in parentheses.

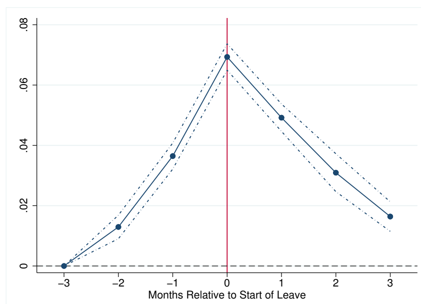
Figure 1: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker



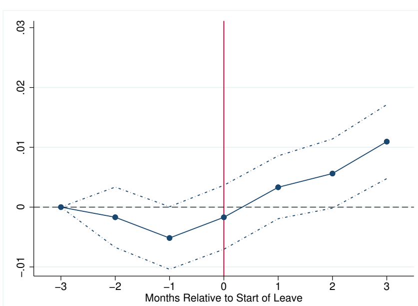
(a) Change in Number of Contracted Employees



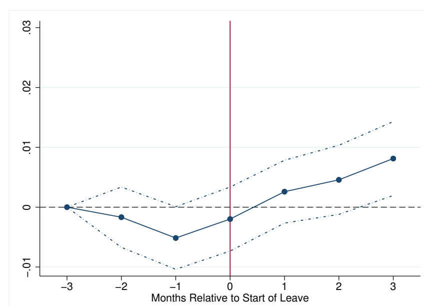
(b) Implied Number of Contracted Employees



(c) Number of Hires



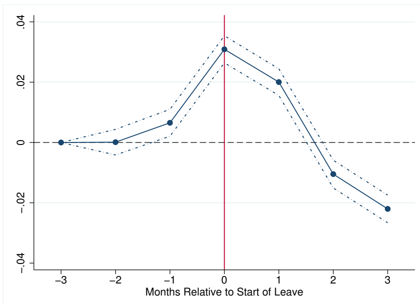
(d) Number of Separations



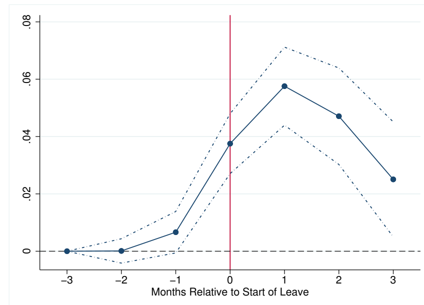
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

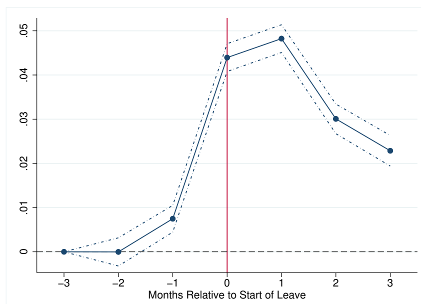
Figure 2: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker



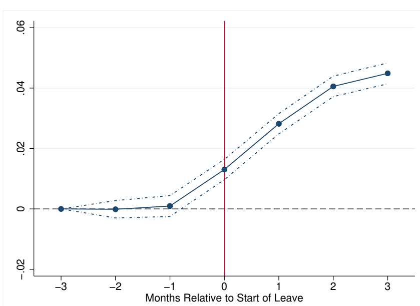
(a) Change in Number of Contracted Employees



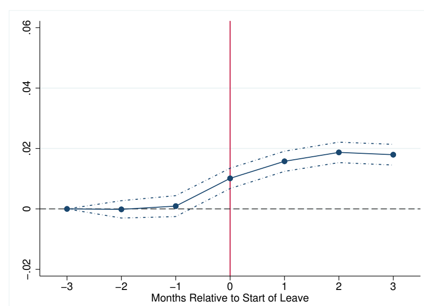
(b) Implied Number of Contracted Employees



(c) Number of Hires



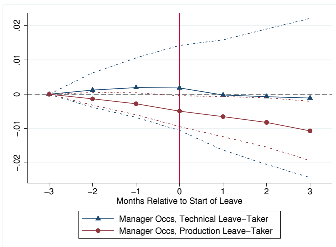
(d) Number of Separations



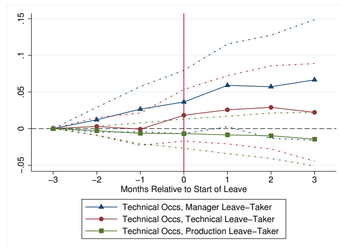
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

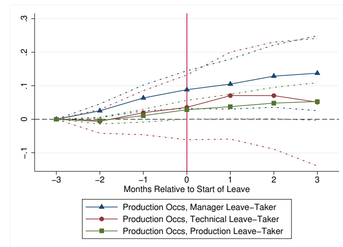
Figure 3: Implied Number of Contracted Employees around Leave Initiation in Non-Leave-Taking Occupations



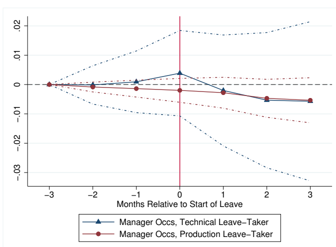
(a) Maternity: Spillovers to Managerial Occupations



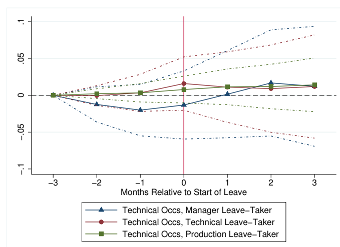
(b) Maternity: Spillovers to Technical Occupations



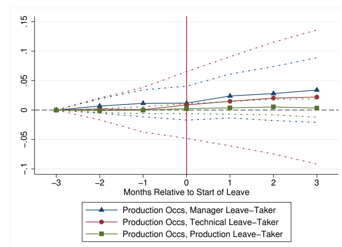
(c) Maternity: Spillovers to Production Occupations



(d) Sickness: Spillovers to Managerial Occupations



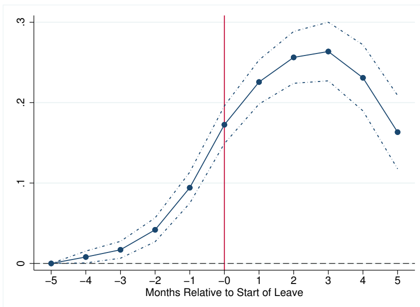
(e) Sickness: Spillovers to Technical Occupations



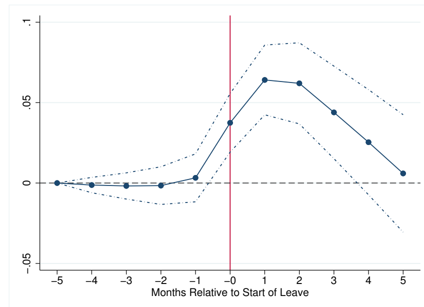
(f) Sickness: Spillovers to Production Occupations

Note: The panels display regression coefficients and associated 95% confidence intervals from an augmented version of equation 1 estimated separately for each coarse occupation grouping (e.g., manager, technical, production) of the spillover occupations. We allow the employment dynamics (i.e., the β_k coefficients) to differ with the coarse occupation of the leave-taker (e.g., manager, technical, production). Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes non-leave-taking plant-occupation groups during the event window.

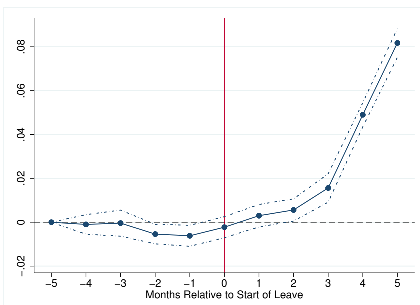
Figure 4: Employment Dynamics around Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window



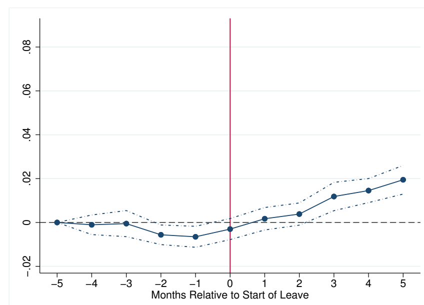
(a) Maternity: Implied Number of Contracted Employees



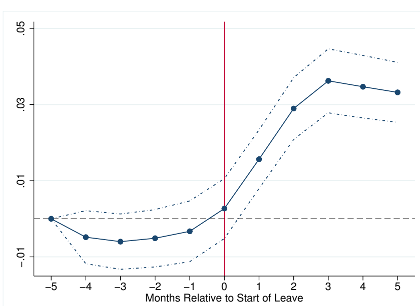
(b) Sickness: Implied Number of Contracted Employees



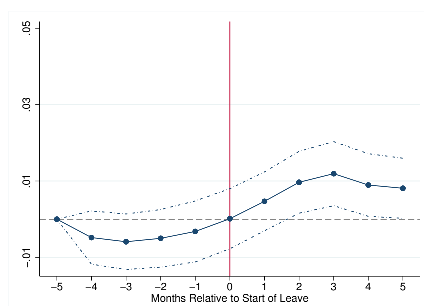
(c) Maternity: Number of Separations



(d) Maternity: Number of Separations Excluding Leave-Taker



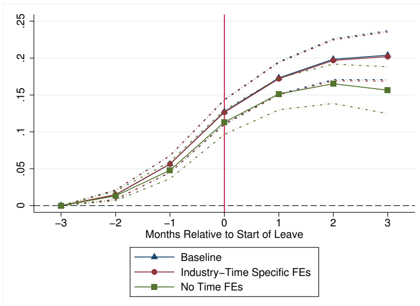
(e) Sickness: Number of Separations



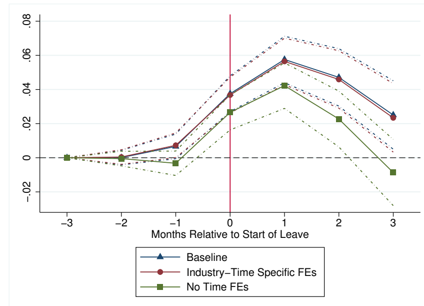
(f) Sickness: Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 where the event window has been extended to five months before and after the month of leave onset. Coefficients in $k = -5$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the leave-taker during the event window.

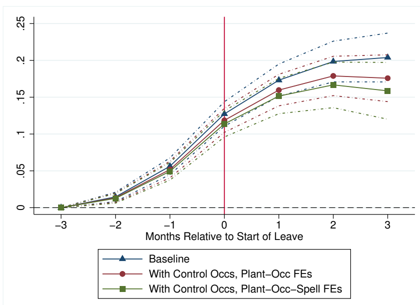
Figure 5: Robustness of Implied Number of Contracted Employees in Occupation of Leave-Taker to Different Modeling Assumptions



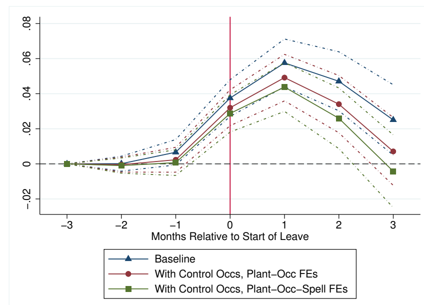
(a) Maternity: Alternate Time Controls



(b) Sickness: Alternate Time Controls



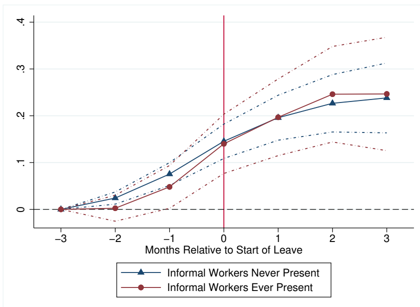
(c) Maternity: With Control Occupations



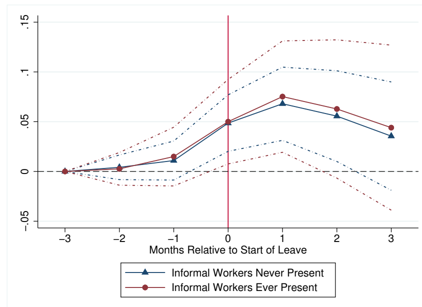
(d) Sickness: With Control Occupations

Note: The panels display regression coefficients and associated 95% confidence intervals from modified versions of equation 1. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. In Panels (a) and (b), the sample includes plant-occupation groups of the leave-taker during the event window. In Panels (c) and (d), the sample additionally includes control occupations (i.e., other occupations in the same plant as the leave-taker during the event window).

Figure 6: Heterogeneity in Implied Number of Contracted Employees around Leave Initiation in Occupation of Leave-Taker by Inspection Violation Status



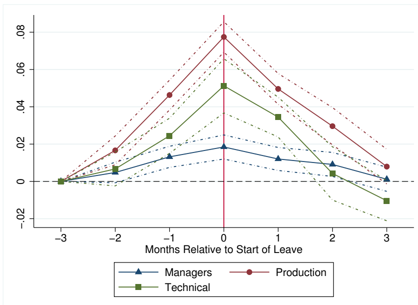
(a) Maternity: By Informal Workers Ever Present Between 2003–2011



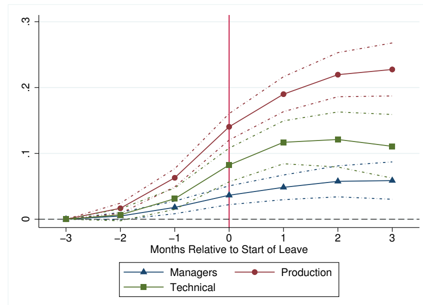
(b) Sickness: By Informal Workers Ever Present Between 2003–2011

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1, estimated separately by plant inspection status. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The samples include plant-occupation groups of the leave-taker during the event window and only plants that ever had their employee registration inspected between 2003–2011.

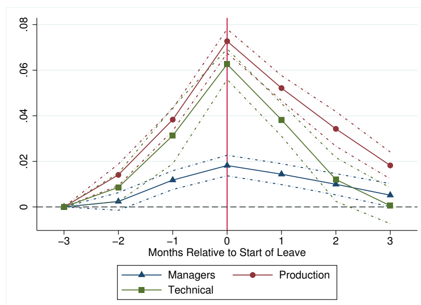
Figure 7: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Coarse Occupation Categories



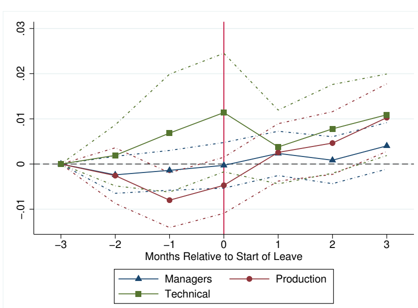
(a) Change in Number of Contracted Employees



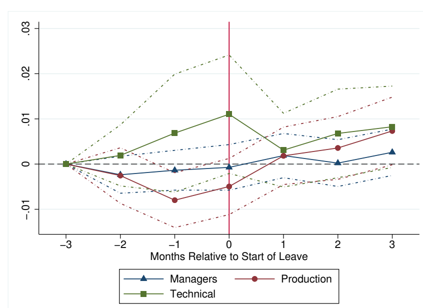
(b) Implied Number of Contracted Employees



(c) Number of Hires



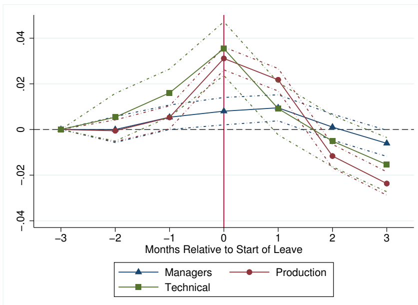
(d) Number of Separations



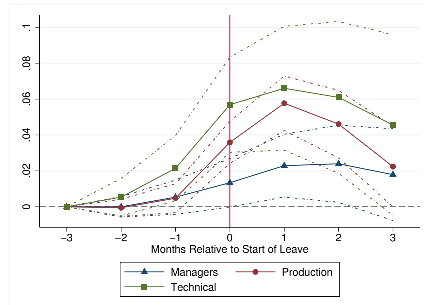
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately for each coarse occupation grouping (e.g., manager, technical, production). Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

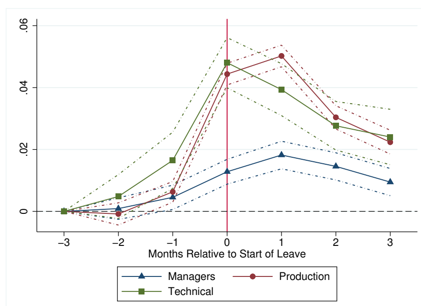
Figure 8: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Coarse Occupation Categories



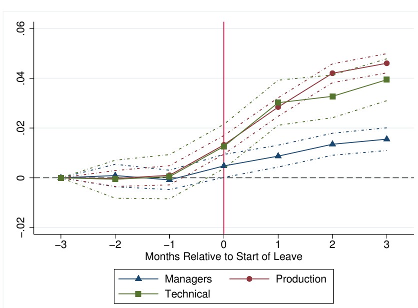
(a) Change in Number of Contracted Employees



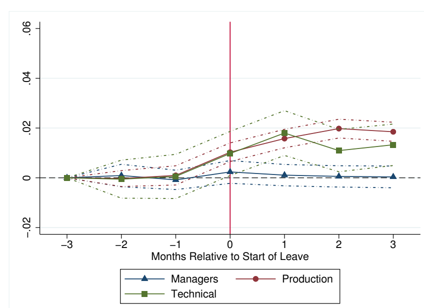
(b) Implied Number of Contracted Employees



(c) Number of Hires



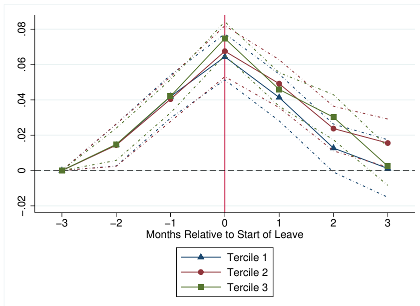
(d) Number of Separations



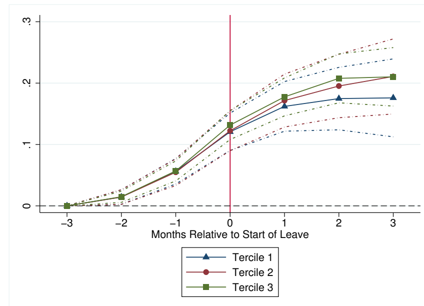
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately for each coarse occupation grouping (e.g., manager, technical, production). Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

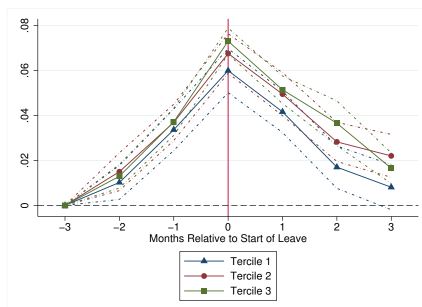
Figure 9: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Market Thickness



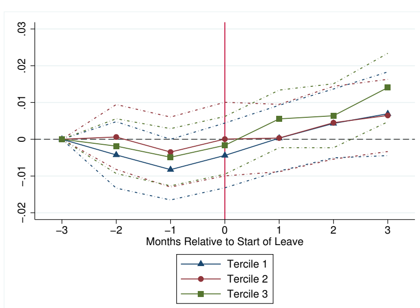
(a) Change in Number of Contracted Employees



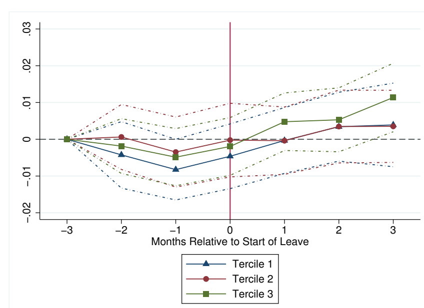
(b) Implied Number of Contracted Employees



(c) Number of Hires



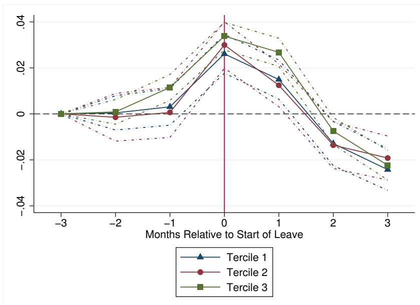
(d) Number of Separations



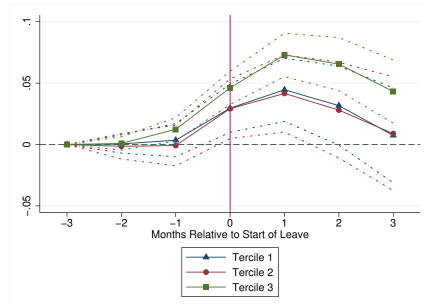
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately for each labor market thickness tercile. Labor market thickness is defined as the relative market share of the occupation (of the leave-taker) in the local labor market. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

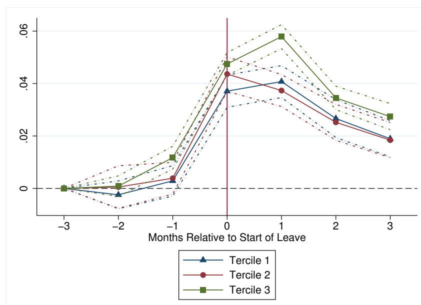
Figure 10: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Market Thickness



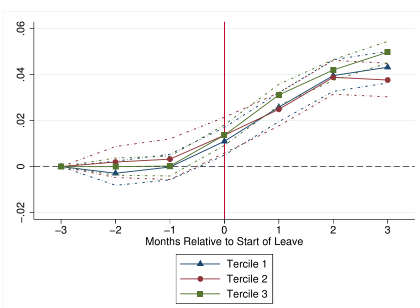
(a) Change in Number of Contracted Employees



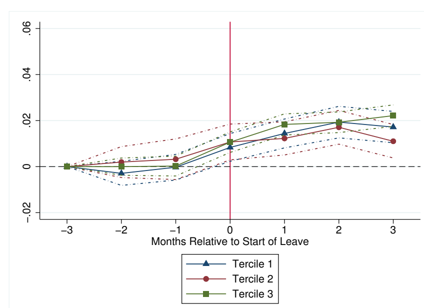
(b) Implied Number of Contracted Employees



(c) Number of Hires



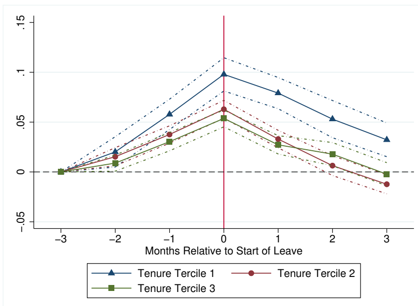
(d) Number of Separations



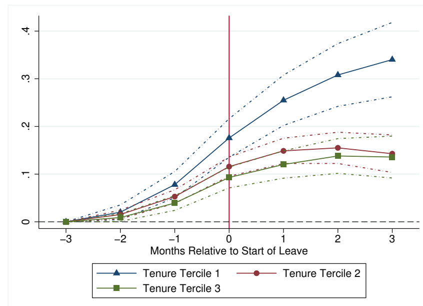
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately for each labor market thickness tercile. Labor market thickness is defined as the relative market share of the occupation (of the leave-taker) in the local labor market. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

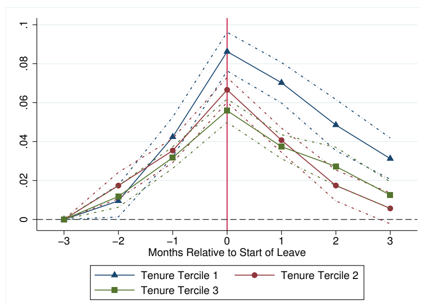
Figure 11: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Leave-Taker Tenure



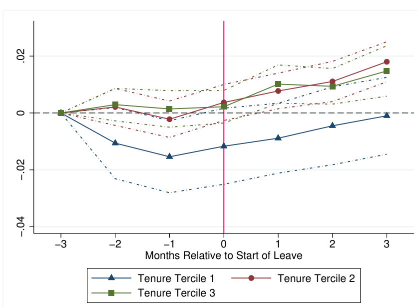
(a) Change in Number of Contracted Employees



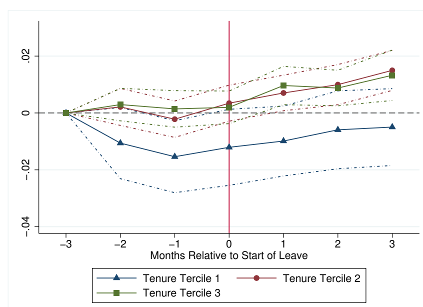
(b) Implied Number of Contracted Employees



(c) Number of Hires



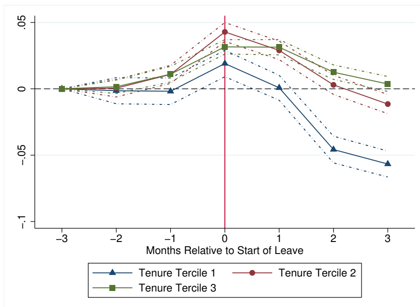
(d) Number of Separations



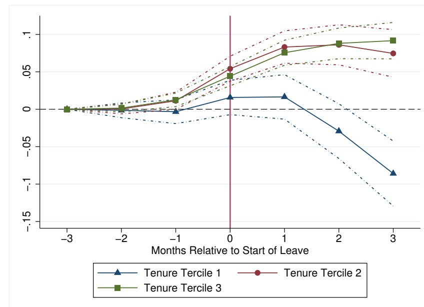
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from an augmented version of equation 1. We allow the employment dynamics (i.e., the β_k coefficients) to differ with the tenure of the leave-taker (measured at the start of their leave), grouping tenure into terciles. Maternity leave-takers in the first tercile have tenure of 14 months or less; workers in the second tercile have between 14 and 30 months of tenure; and, workers in the third tercile have more than 30 months of tenure. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

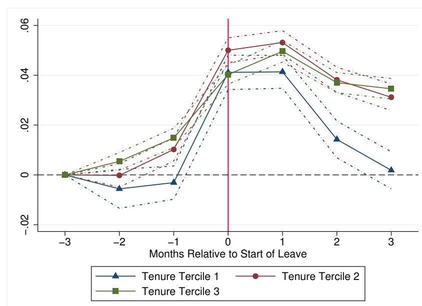
Figure 12: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Leave-Taker Tenure



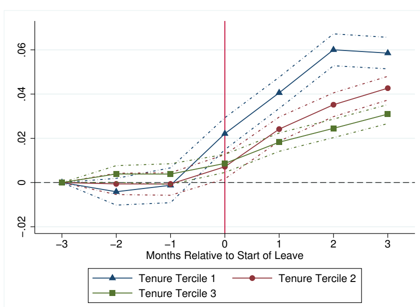
(a) Change in Number of Contracted Employees



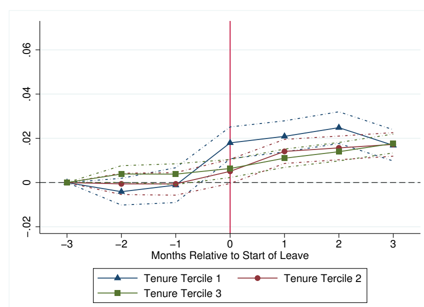
(b) Implied Number of Contracted Employees



(c) Number of Hires



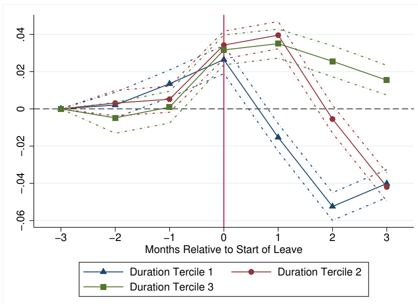
(d) Number of Separations



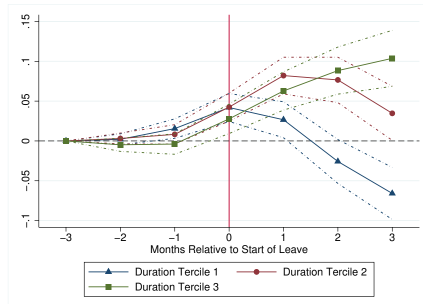
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from an augmented version of equation 1. We allow the employment dynamics (i.e., the β_k coefficients) to differ with the tenure of the leave-taker (measured at the start of their leave), grouping tenure into terciles. Sickness leave-takers in the first tercile have tenure of 16 months or less; workers in the second tercile have between 16 and 41 months of tenure; and, workers in the third tercile have more than 41 months of tenure. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

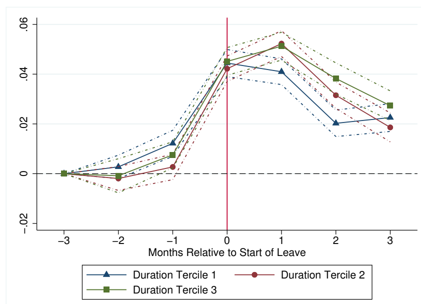
Figure 13: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Leave Duration



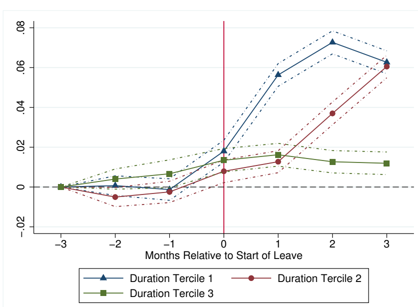
(a) Change in Number of Contracted Employees



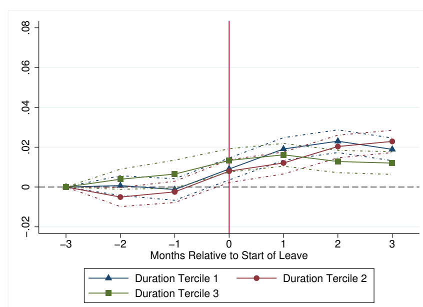
(b) Implied Number of Contracted Employees



(c) Number of Hires



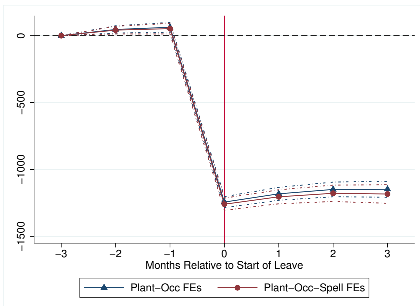
(d) Number of Separations



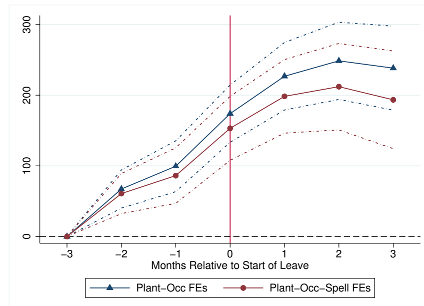
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from an augmented version of equation 1. We allow the employment dynamics (i.e., the β_k coefficients) to differ with leave duration, grouping duration into terciles. Tercile 1 corresponds to less than 54 days; tercile 2 corresponds to between 54 and 139 days; and, tercile 3 corresponds to 140 days or more. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

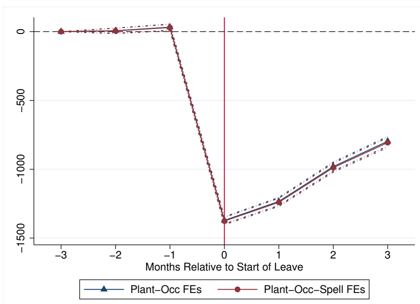
Figure 14: Wage Bill Dynamics around Leave Initiation in Occupation of Leave-Taker Including Control Groups



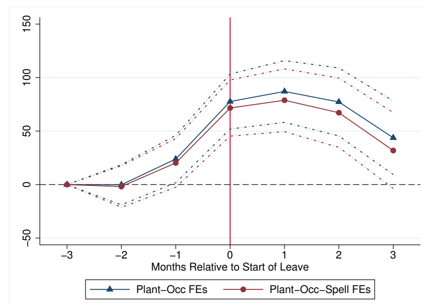
(a) Maternity: Wage Bill Excluding Earnings During Periods of Leave



(b) Maternity: Wage Bill Excluding Leave-Taker Earnings for Entire Window



(c) Sickness: Wage Bill Excluding Earnings During Periods of Leave



(d) Sickness: Wage Bill Excluding Leave-Taker Earnings for Entire Window

Note: The panels display regression coefficients and associated 95% confidence intervals from a modified version of equation 1, where non-leave-taking occupations in the same plant as the leave-taker are included as control groups. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes occupation groups in the same plant as the leave-taker during the event window.

Appendix Tables and Figures

Table A1: Employment Dynamics around Leave Initiation in Occupation of Leave-Taker

	Change in # Employees (1)	# of Hires (2)	# of Separations (3)	# Seps Excl Leave-Taker (4)
Panel A: Maternity Leave				
β_{-2}	0.0146***	0.0129***	-0.00169	-0.00168
	(0.00324)	(0.00199)	(0.00257)	(0.00257)
β_{-1}	0.0416***	0.0365***	-0.00516*	-0.00516*
	(0.00335)	(0.00220)	(0.00267)	(0.00267)
β_0	0.0710***	0.0693***	-0.00170	-0.00199
	(0.00352)	(0.00223)	(0.00273)	(0.00273)
β_1	0.0459***	0.0492***	0.00332	0.00259
	(0.00348)	(0.00232)	(0.00268)	(0.00268)
β_2	0.0253***	0.0309***	0.00562*	0.00457
	(0.00423)	(0.00321)	(0.00295)	(0.00295)
β_3	0.00545	0.0164***	0.0110***	0.00813***
	(0.00390)	(0.00250)	(0.00315)	(0.00315)
R^2	0.114	0.408	0.396	0.396
N	7791259	7791259	7791259	7791259
Panel B: Sickness Leave				
β_{-2}	0.000103	-0.0000261	-0.000129	-0.000152
	(0.00217)	(0.00163)	(0.00147)	(0.00147)
β_{-1}	0.00653***	0.00748***	0.000947	0.000925
	(0.00228)	(0.00154)	(0.00177)	(0.00177)
β_0	0.0309***	0.0439***	0.0130***	0.0101***
	(0.00229)	(0.00160)	(0.00172)	(0.00171)
β_1	0.0200***	0.0482***	0.0282***	0.0158***
	(0.00225)	(0.00160)	(0.00171)	(0.00170)
β_2	-0.0105***	0.0301***	0.0406***	0.0187***
	(0.00234)	(0.00171)	(0.00173)	(0.00173)
β_3	-0.0220***	0.0229***	0.0449***	0.0179***
	(0.00234)	(0.00177)	(0.00176)	(0.00175)
R^2	0.108	0.385	0.401	0.402
N	8219554	8219554	8219554	8219554

Note: Each column displays estimated coefficients from separate regressions of equation 1. Coefficients in $k = -3$ are normalized to zero. Standard errors are clustered at the leave spell level and shown in parentheses. The sample includes plant-occupation groups of the leave-taker during the event window. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Plant Characteristics Associated with Leave Spells by Inspection Status

	Maternity Leave Spells				Sickness Leave Spells			
	Never Inspected	Inspected, No Reg Inspection	Reg Inspected, No Inf Workers	Reg Inspected, Inf Workers Present	Never Inspected	Inspected, No Reg Inspection	Reg Inspected, No Inf Workers	Reg Inspected, Inf Workers Present
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Industry								
Agriculture/Forestry/Fishing	0.022	0.006	0.014	0.014	0.083	0.013	0.035	0.032
Mining	0.001	0.001	0.002	0.002	0.003	0.003	0.005	0.005
Manufacturing	0.082	0.123	0.128	0.150	0.112	0.171	0.165	0.168
Utilities: Electric/Gas	0.000	0.001	0.001	0.002	0.001	0.001	0.001	0.001
Utilities: Water/Sewage/Waste	0.002	0.002	0.002	0.003	0.004	0.004	0.003	0.003
Construction	0.011	0.014	0.022	0.033	0.033	0.035	0.049	0.049
Wholesale/Retail Trade	0.415	0.400	0.432	0.489	0.348	0.358	0.373	0.446
Transportation/Storage/Mail	0.023	0.027	0.030	0.031	0.053	0.054	0.047	0.036
Accommodation/Food	0.098	0.087	0.099	0.097	0.079	0.072	0.085	0.097
Information/Communication	0.016	0.019	0.015	0.012	0.013	0.015	0.014	0.012
Financial Services	0.033	0.031	0.037	0.022	0.022	0.019	0.026	0.018
Real Estate	0.009	0.007	0.005	0.003	0.005	0.005	0.004	0.003
Professional/Scientific/Technical	0.054	0.048	0.027	0.015	0.035	0.033	0.021	0.014
Administrative Activities	0.062	0.061	0.049	0.031	0.088	0.093	0.062	0.032
Public Admin/Defense/Social Security	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Education	0.047	0.056	0.049	0.036	0.030	0.036	0.038	0.033
Health/Social Services	0.067	0.051	0.048	0.031	0.035	0.029	0.032	0.024
Art/Culture/Sports	0.010	0.008	0.007	0.006	0.009	0.008	0.007	0.006
Other Service Activities	0.049	0.057	0.033	0.021	0.045	0.048	0.031	0.020
# of Workers on Dec 31								
0	0.027	0.014	0.011	0.008	0.012	0.006	0.006	0.005
1 to 4	0.367	0.233	0.146	0.088	0.384	0.257	0.154	0.101
5 to 9	0.275	0.276	0.221	0.159	0.287	0.294	0.239	0.180
10 to 19	0.200	0.247	0.262	0.246	0.201	0.249	0.287	0.283
20 to 49	0.109	0.176	0.245	0.282	0.102	0.162	0.246	0.306
50 to 99	0.018	0.042	0.079	0.125	0.013	0.028	0.057	0.097
100 to 249	0.003	0.011	0.030	0.071	0.001	0.004	0.011	0.025
250 to 499	0.000	0.001	0.005	0.017	0.000	0.000	0.001	0.002
500 to 999	0.000	0.000	0.001	0.005	0.000	0.000	0.000	0.000
1000+	0.000	0.000	0.000	0.001	0.000	0.000	0.000	0.000
# of Leaves	633913	51540	262930	164654	691131	59916	275075	148100

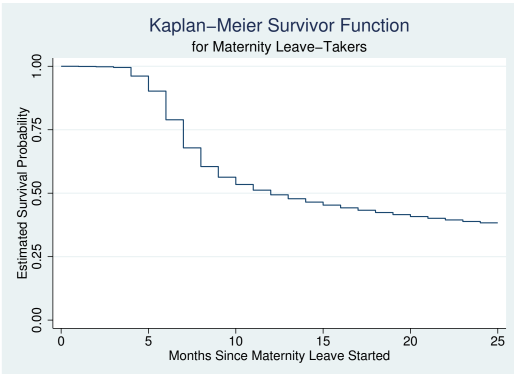
Note: We categorize maternity and sickness leave spells by the inspection status of the associated plant and provide plant-level characteristics associated with each spell. In columns (1) and (5), spells are associated with plants that were never inspected from 2003–2011. In columns (2) and (6), spells are associated with plants that were inspected at some point from 2003–2011 but were never inspected for possible registration violations. In columns (3) and (7), spells are associated with plants that were ever inspected for registration violations from 2003–2011, but informal workers were never found present. In columns (4) and (8), spells are associated with plants that were ever inspected for registration violations from 2003–2011, and informal workers were present.

Table A3: Pre-Leave Plant-Occupation Wage Bill Descriptive Statistics

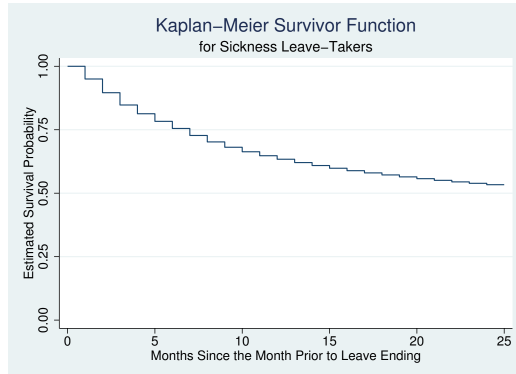
	Maternity		Sickness	
	Own Plant-Occ	Control Plant-Occ	Own Plant-Occ	Control Plant-Occ
Wage Bill	15210.738	13865.463	13566.762	8933.454
	(54155.618)	(55848.095)	(35261.904)	(27313.927)
Wage Bill Excluding Leave-Taker Earnings	13797.404		12118.617	
	(53784.036)		(34910.684)	
# of Plant-Occupations	501607	857263	522040	814492

Note: Statistics are measured three months prior to leave initiation. Own plant-occupations refer to the plant-occupation of the leave-taker, and spillover plant-occupations refer to other occupations in the same plant as the leave-taker. Standard deviations are in parentheses.

Figure A1: Kaplan-Meier Survival Functions for Maternity and Sickness Leave-Takers



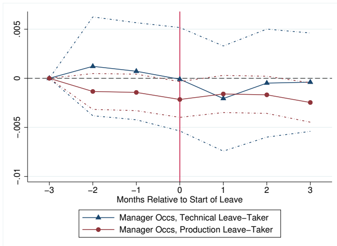
(a) Maternity Leave-Takers



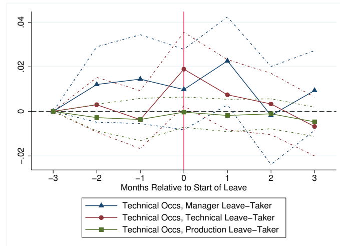
(b) Sickness Leave-Takers

Note: The figures show Kaplan-Meier survivor functions for the maternity leave-takers and sickness leave-takers in our main estimation sample. The x-axis in Panel (a) is months since the month of maternity leave onset, and in Panel (b) is the months since the month prior to sickness leave ending. Survival is defined as still being contracted with the plant where the leave began.

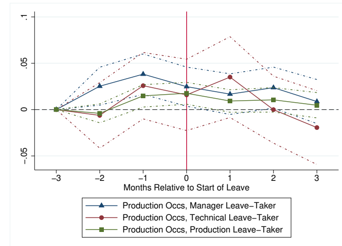
Figure A2: Employment Dynamics around Maternity Leave Initiation in Non-Leave-Taking Occupations



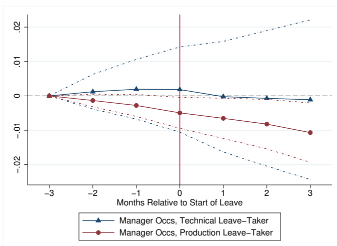
(a) Change in Number of Contracted Employees



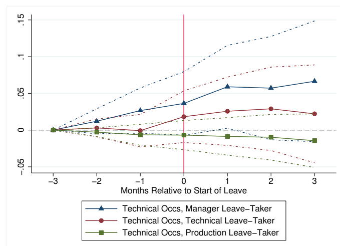
(b) Change in Number of Contracted Employees



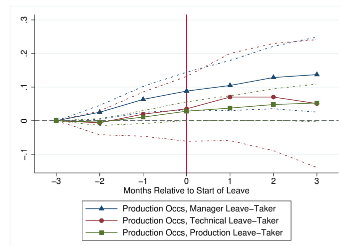
(c) Change in Number of Contracted Employees



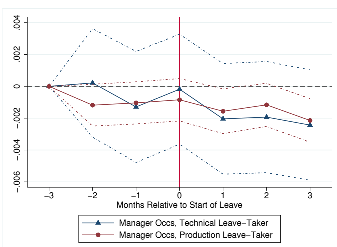
(d) Implied Number of Contracted Employees



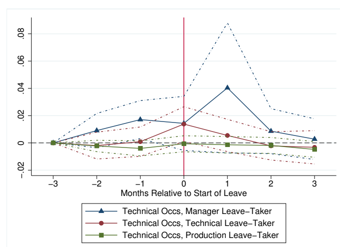
(e) Implied Number of Contracted Employees



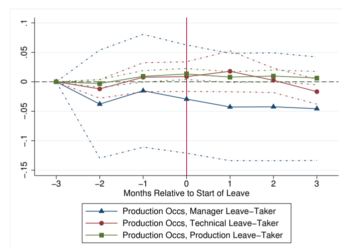
(f) Implied Number of Contracted Employees



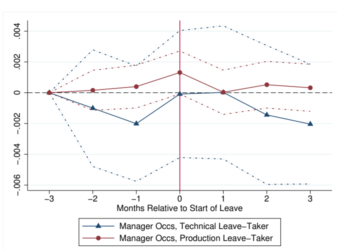
(g) Number of Hires



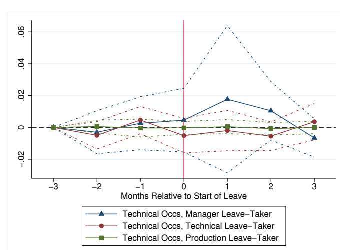
(h) Number of Hires



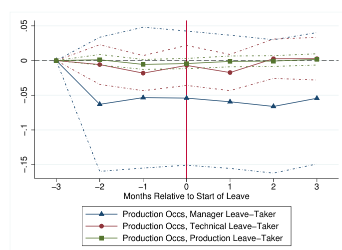
(i) Number of Hires



(j) Number of Separations



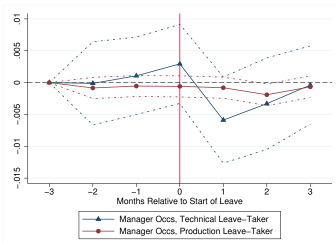
(k) Number of Separations



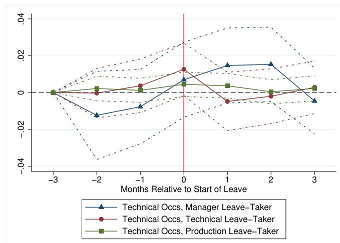
(l) Number of Separations

Note: The panels display regression coefficients and associated 95% confidence intervals from an augmented version of equation 1 estimated separately for each coarse occupation grouping (e.g., manager, technical, production) of the spillover occupations. We allow the employment dynamics (i.e., the β_k coefficients) to differ with the coarse occupation of the leave-taker (e.g., manager, technical, production). Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes non-maternity-leave-taking plant-occupation groups during the event window.

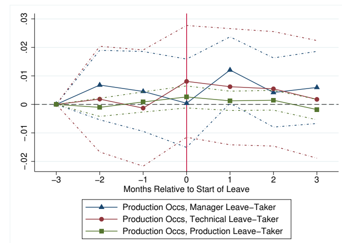
Figure A3: Employment Dynamics around Sickness Leave Initiation in Non-Leave-Taking Occupations



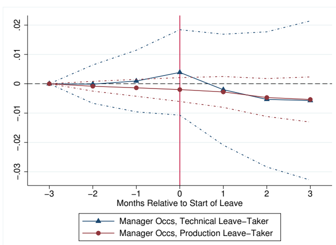
(a) Change in Number of Contracted Employees



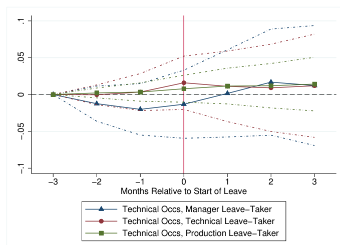
(b) Change in Number of Contracted Employees



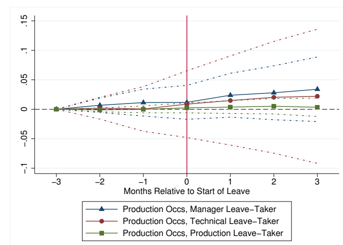
(c) Change in Number of Contracted Employees



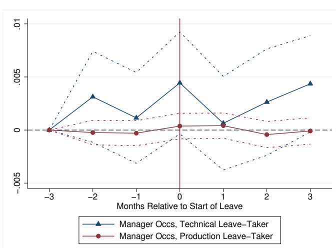
(d) Implied Number of Contracted Employees



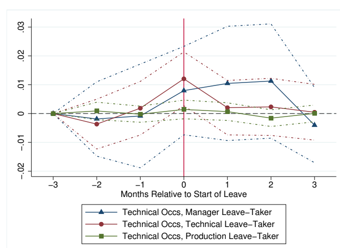
(e) Implied Number of Contracted Employees



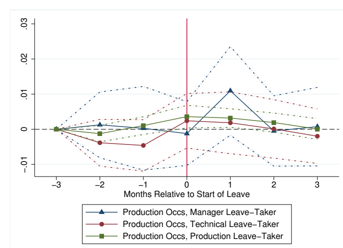
(f) Implied Number of Contracted Employees



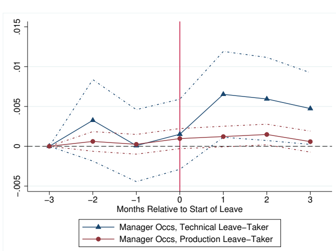
(g) Number of Hires



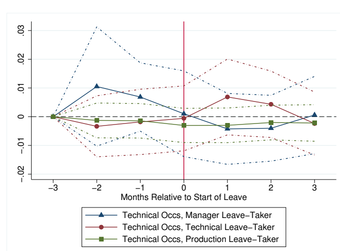
(h) Number of Hires



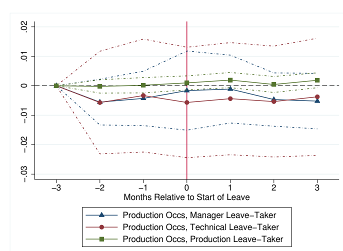
(i) Number of Hires



(j) Number of Separations



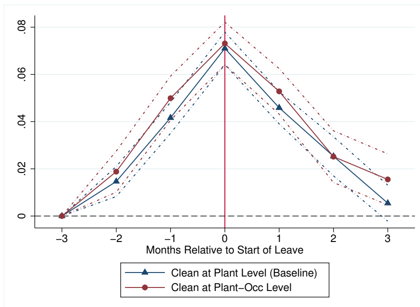
(k) Number of Separations



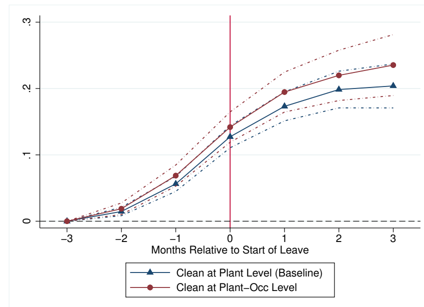
(l) Number of Separations

Note: The panels display regression coefficients and associated 95% confidence intervals from an augmented version of equation 1 estimated separately for each coarse occupation grouping (e.g., manager, technical, production) of the spillover occupations. We allow the employment dynamics (i.e., the β_k coefficients) to differ with the coarse occupation of the leave-taker (e.g., manager, technical, production). Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes non-sickness-leave-taking plant-occupation groups during the event window.

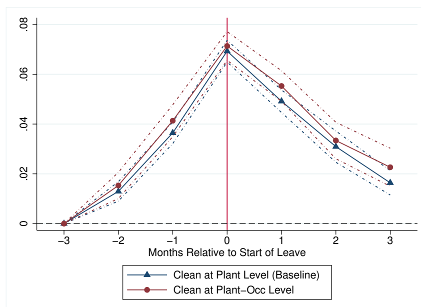
Figure A4: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker Using a Relaxed Clean-Spell Definition



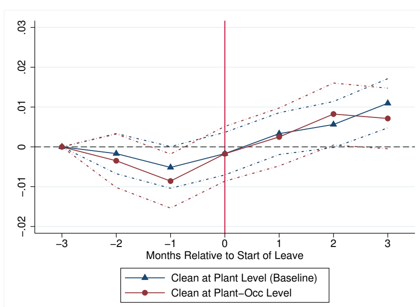
(a) Change in Number of Contracted Employees



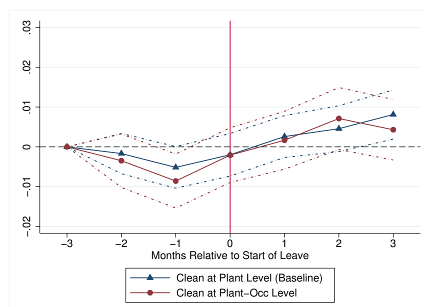
(b) Implied Number of Contracted Employees



(c) Number of Hires



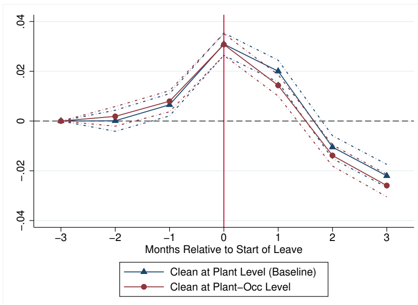
(d) Number of Separations



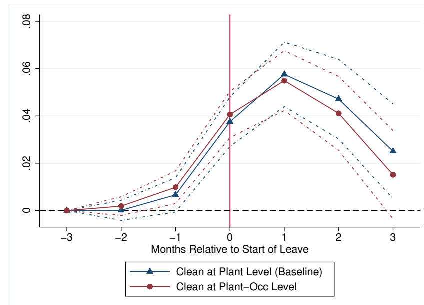
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 using leave spells where the event window centered on the month of initiation does not intersect the event window of another maternity leave spell at that plant-occupation. This is a relaxation of the clean-spell definition used in our baseline analysis, which is restricted to spells where the event window centered on the month of initiation does not intersect the event window of another maternity leave spell at that plant. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

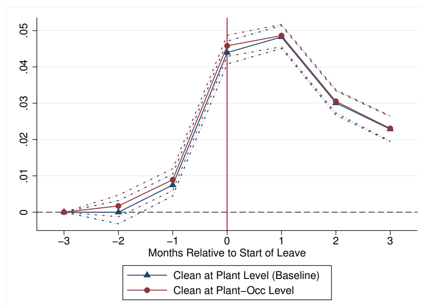
Figure A5: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Using a Relaxed Clean-Spell Definition



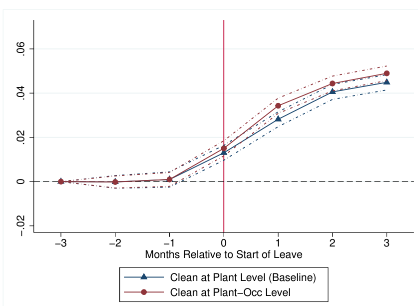
(a) Change in Number of Contracted Employees



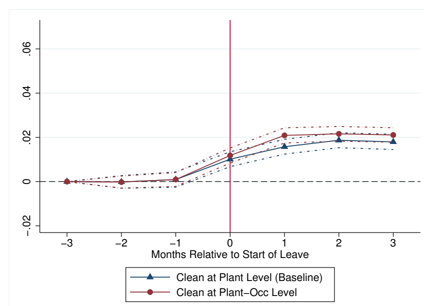
(b) Implied Number of Contracted Employees



(c) Number of Hires



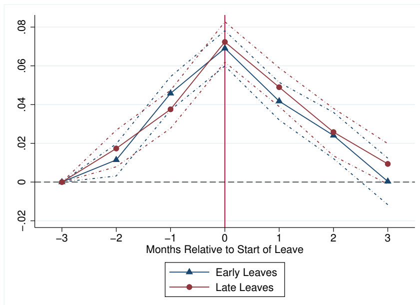
(d) Number of Separations



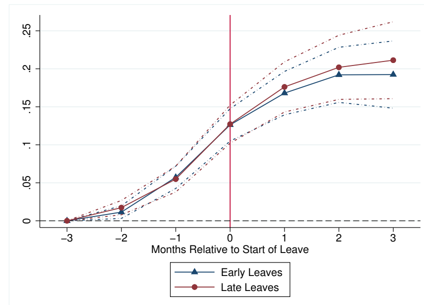
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 using leave spells where the event window centered on the month of initiation does not intersect the event window of another sickness leave spell at that plant-occupation. This is a relaxation of the clean-spell definition used in our baseline analysis, which is restricted to spells where the event window centered on the month of initiation does not intersect the event window of another sickness leave spell at that plant. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

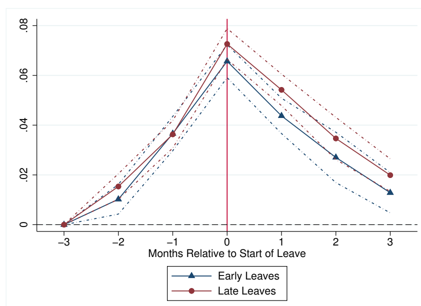
Figure A6: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker in Early and Later Years



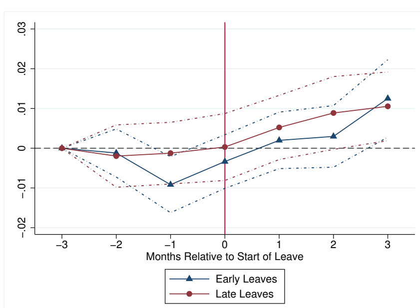
(a) Change in Number of Contracted Employees



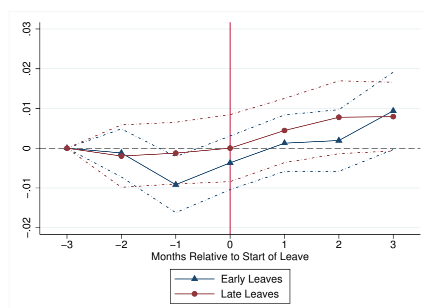
(b) Implied Number of Contracted Employees



(c) Number of Hires



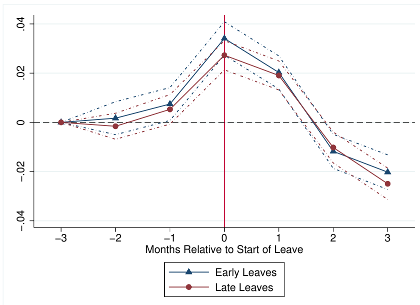
(d) Number of Separations



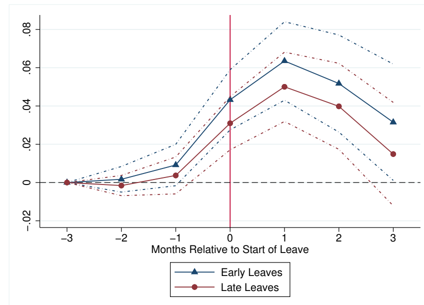
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 for leave spells that begin in 2012–2014 (Early) and those that begin in 2015–2017 (Late). The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

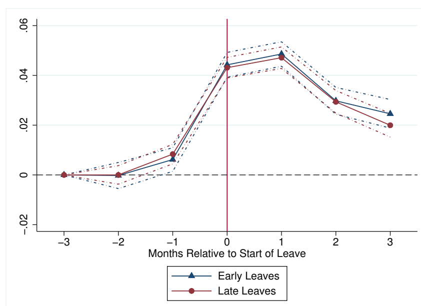
Figure A7: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker in Early and Later Years



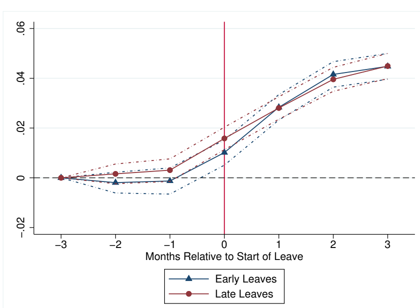
(a) Change in Number of Contracted Employees



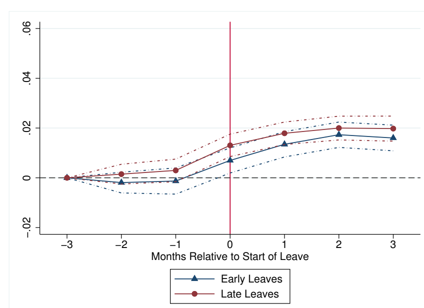
(b) Implied Number of Contracted Employees



(c) Number of Hires



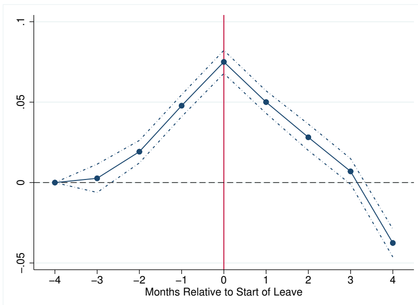
(d) Number of Separations



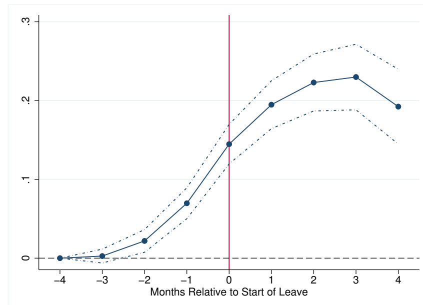
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 for leave spells that begin in 2012–2014 (Early) and those that begin in 2015–2017 (Late). The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

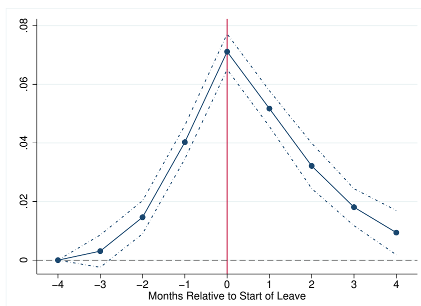
Figure A8: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 4-Month Event Window



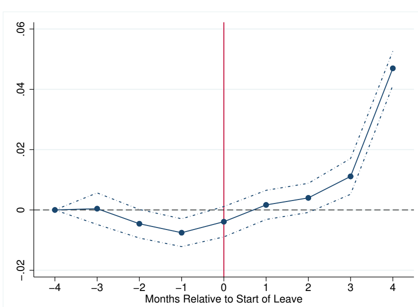
(a) Change in Number of Contracted Employees



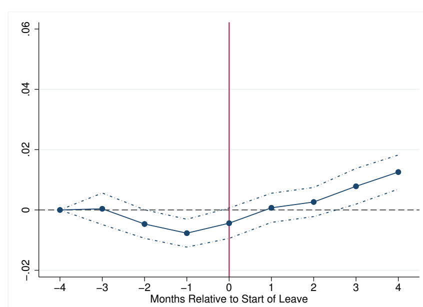
(b) Implied Number of Contracted Employees



(c) Number of Hires



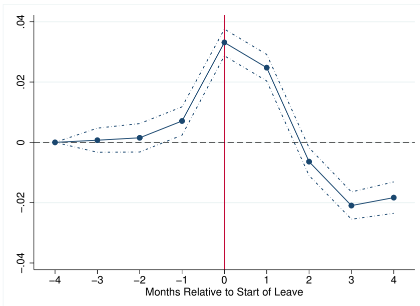
(d) Number of Separations



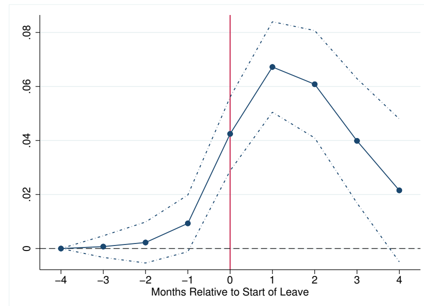
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 where the event window has been extended to four months before and after the month of maternity leave onset. Coefficients in $k = -4$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

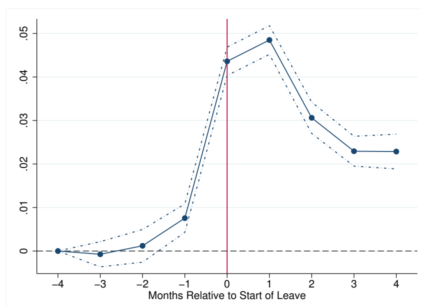
Figure A9: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 4-Month Event Window



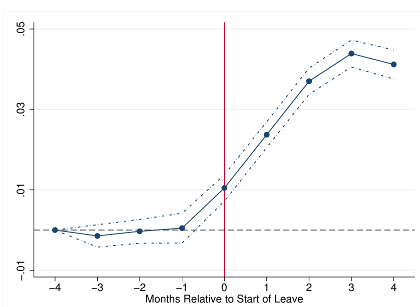
(a) Change in Number of Contracted Employees



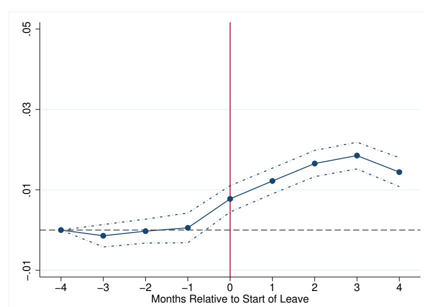
(b) Implied Number of Contracted Employees



(c) Number of Hires



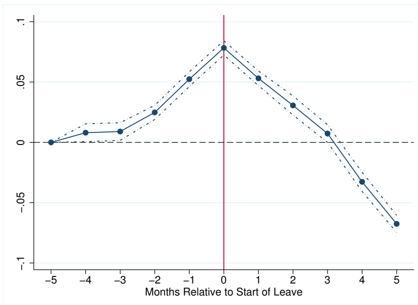
(d) Number of Separations



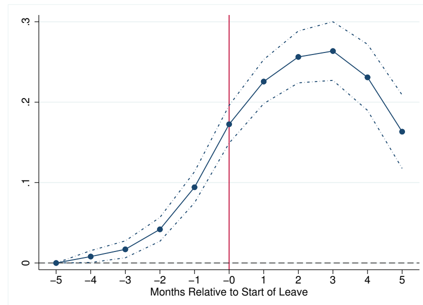
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 where the event window has been extended to four months before and after the month of sickness leave onset. Coefficients in $k = -4$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

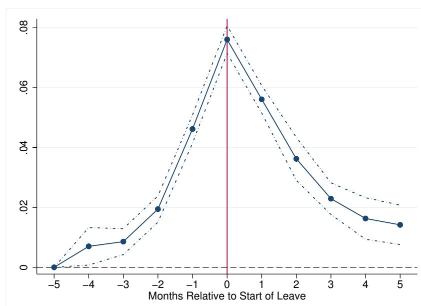
Figure A10: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window



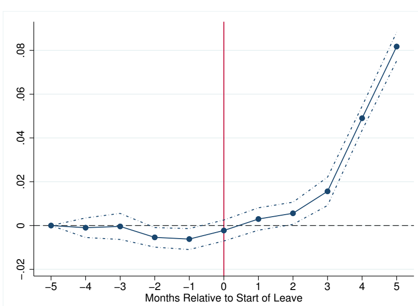
(a) Change in Number of Contracted Employees



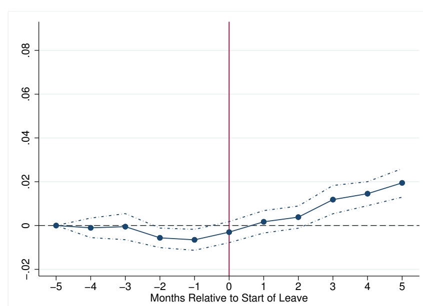
(b) Implied Number of Contracted Employees



(c) Number of Hires



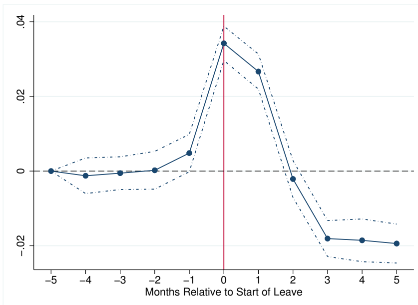
(d) Number of Separations



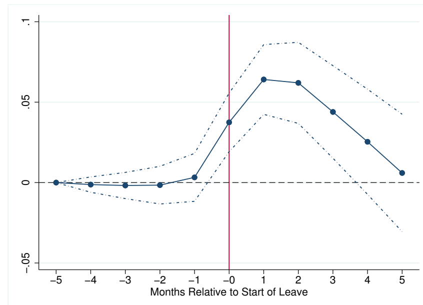
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 where the event window has been extended to five months before and after the month of maternity leave onset. Coefficients in $k = -5$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

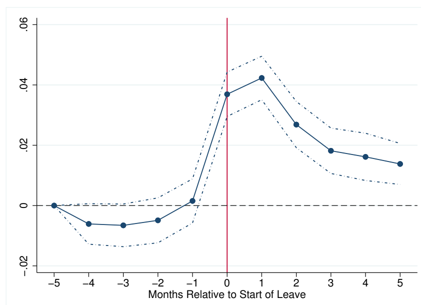
Figure A11: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window



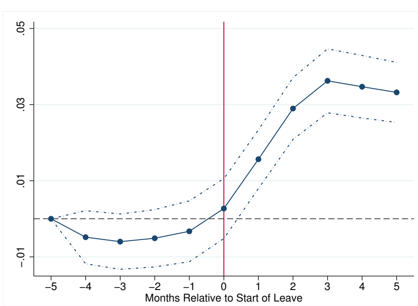
(a) Change in Number of Contracted Employees



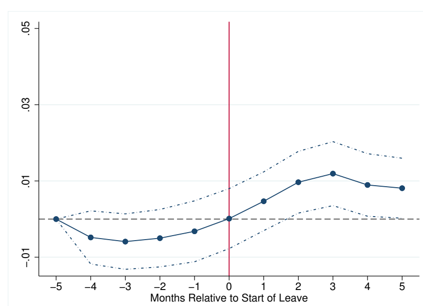
(b) Implied Number of Contracted Employees



(c) Number of Hires



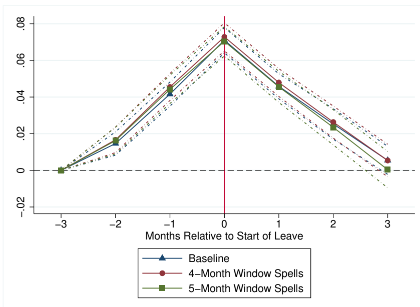
(d) Number of Separations



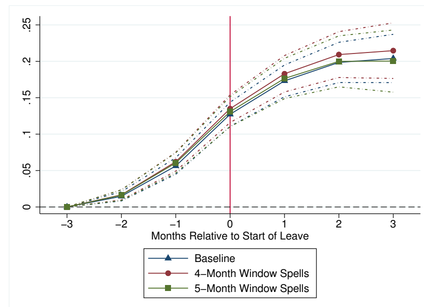
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 where the event window has been extended to five months before and after the month of sickness leave onset. Coefficients in $k = -5$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

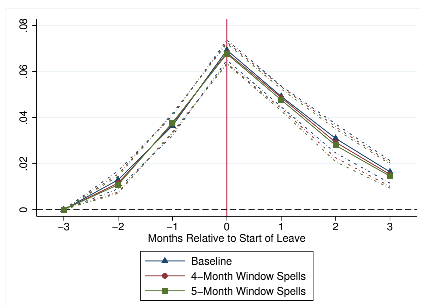
Figure A12: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 3-Month Event Window Using 4-Month and 5-Month Window Spells



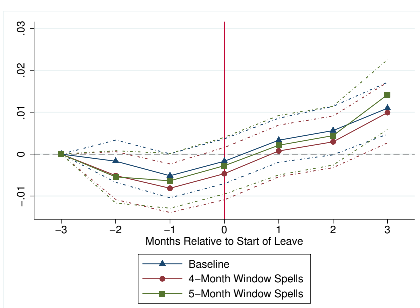
(a) Change in Number of Contracted Employees



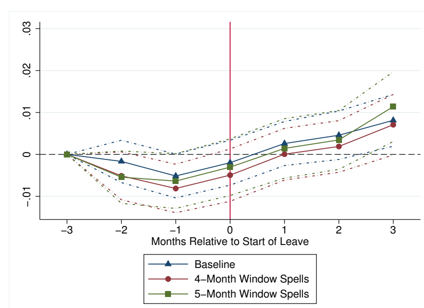
(b) Implied Number of Contracted Employees



(c) Number of Hires



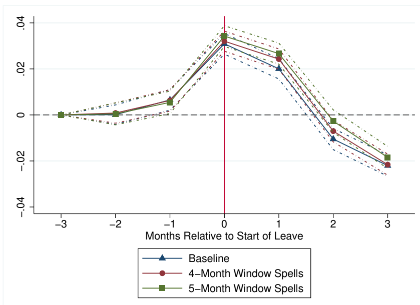
(d) Number of Separations



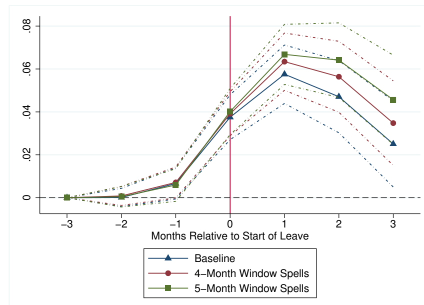
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

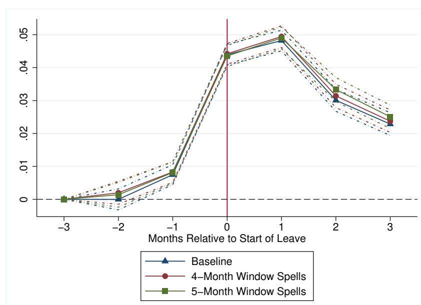
Figure A13: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 3-Month Event Window Using 4-Month and 5-Month Window Spells



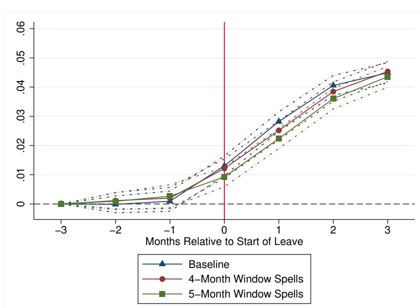
(a) Change in Number of Contracted Employees



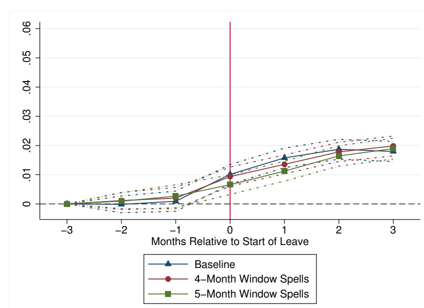
(b) Implied Number of Contracted Employees



(c) Number of Hires



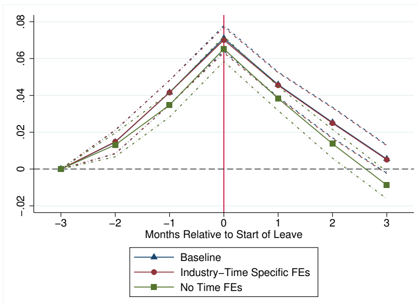
(d) Number of Separations



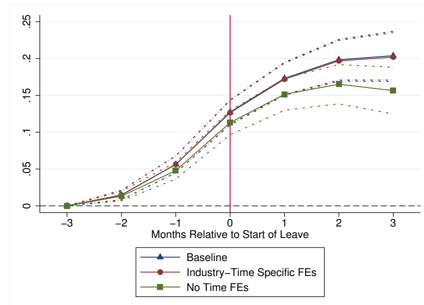
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

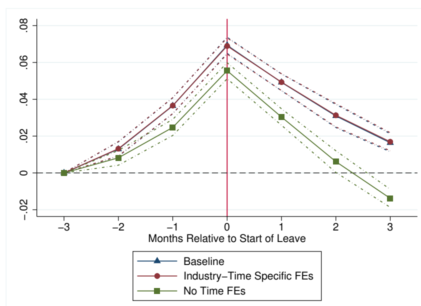
Figure A14: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with Different Calendar Time Controls



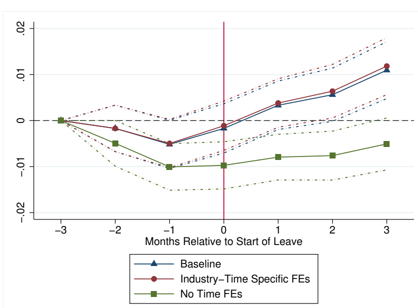
(a) Change in Number of Contracted Employees



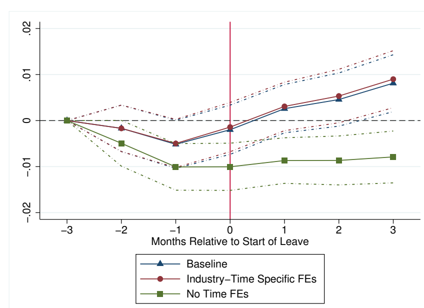
(b) Implied Number of Contracted Employees



(c) Number of Hires



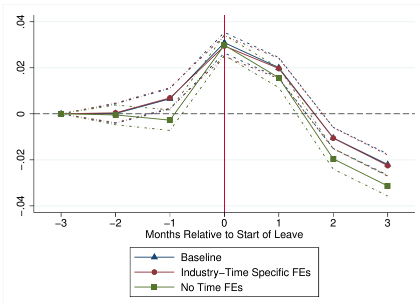
(d) Number of Separations



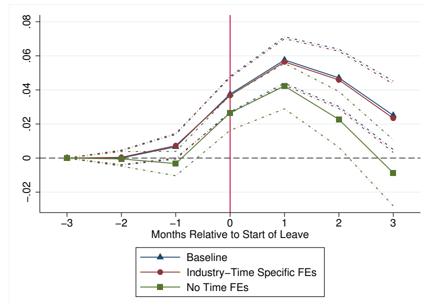
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from various versions of equation 1 with either calendar time fixed effects (baseline), no calendar time fixed effects, or industry-specific time fixed effects. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

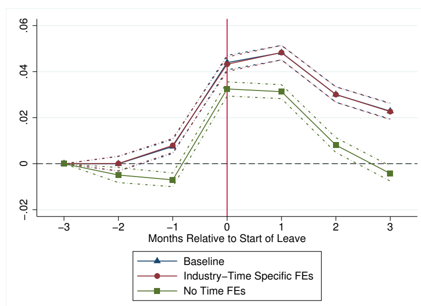
Figure A15: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with Different Calendar Time Controls



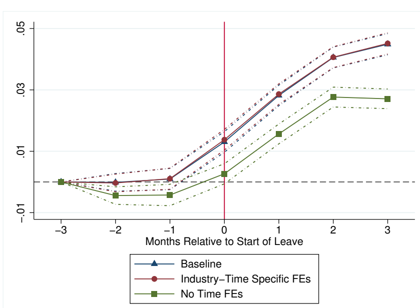
(a) Change in Number of Contracted Employees



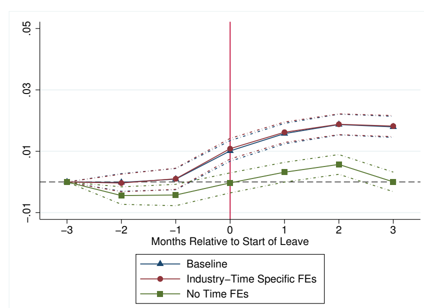
(b) Implied Number of Contracted Employees



(c) Number of Hires



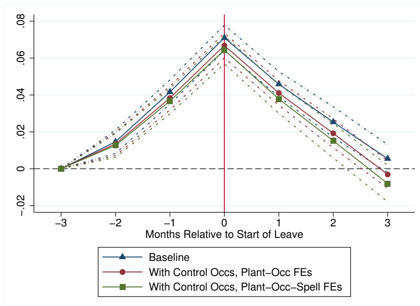
(d) Number of Separations



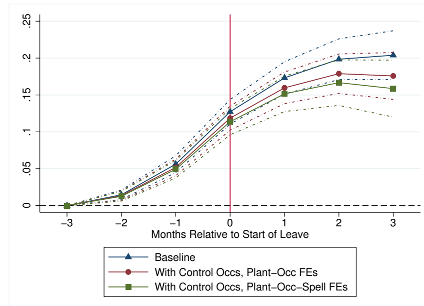
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from various versions of equation 1 with either calendar time fixed effects (baseline), no calendar time fixed effects, or industry-specific time fixed effects. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

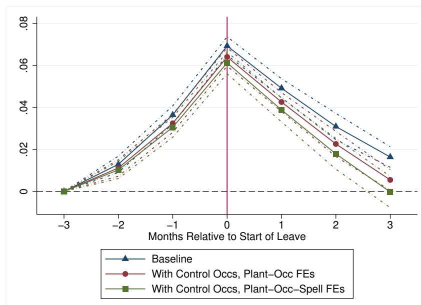
Figure A16: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker Including Control Groups



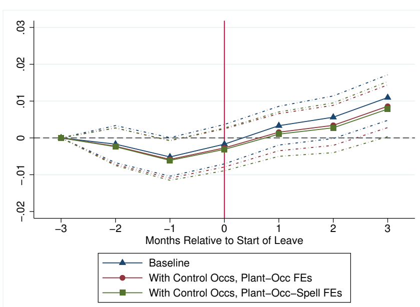
(a) Change in Number of Contracted Employees



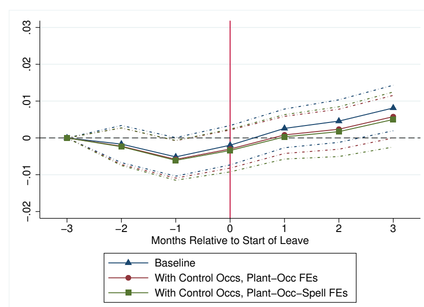
(b) Implied Number of Contracted Employees



(c) Number of Hires



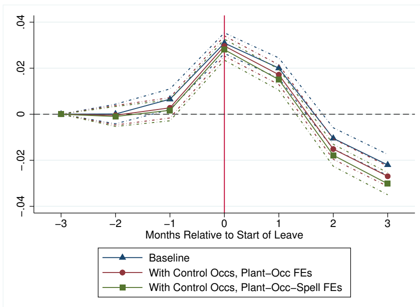
(d) Number of Separations



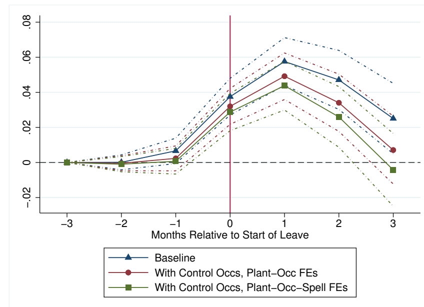
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from modified versions of equation 1, where non-leave-taking occupations in the same plant as the leave-taker are included as control groups. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes occupation groups in the same plant as the maternity leave-taker during the event window.

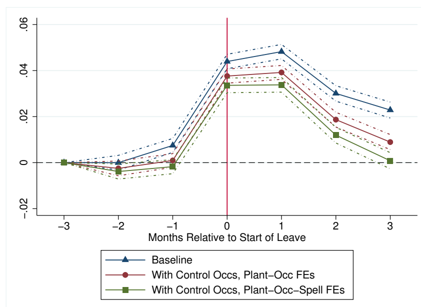
Figure A17: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Including Control Groups



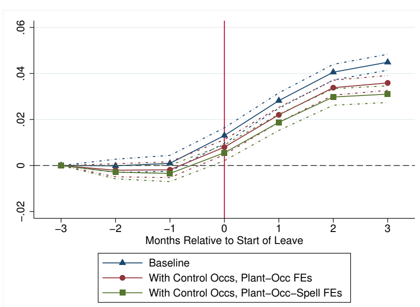
(a) Change in Number of Contracted Employees



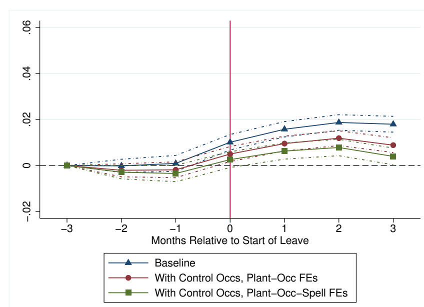
(b) Implied Number of Contracted Employees



(c) Number of Hires



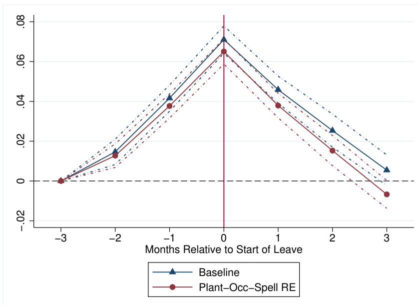
(d) Number of Separations



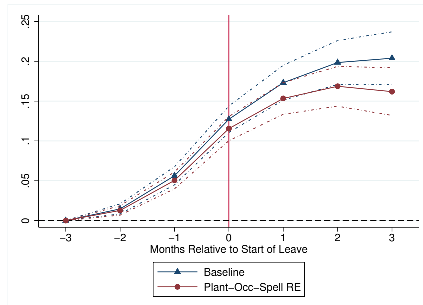
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from modified versions of equation 1, where non-leave-taking occupations in the same plant as the leave-taker are included as control groups. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes occupation groups in the same plant as the sickness leave-taker during the event window.

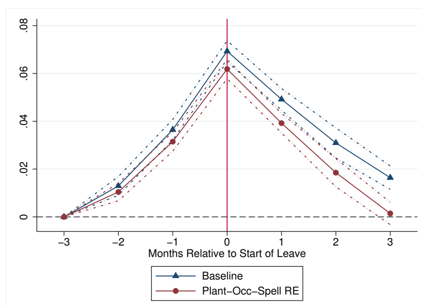
Figure A18: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker Including Plant-Occupation-Spell Random Effects



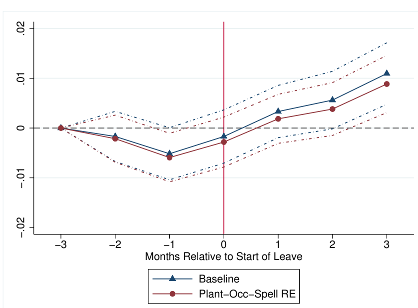
(a) Change in Number of Contracted Employees



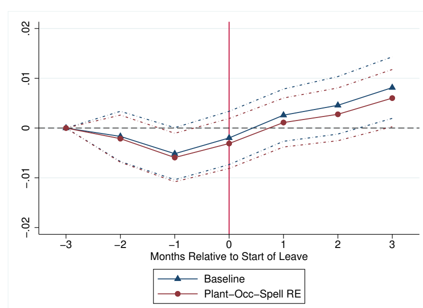
(b) Implied Number of Contracted Employees



(c) Number of Hires



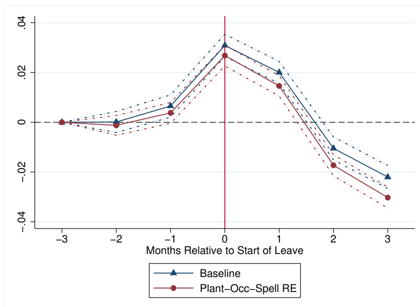
(d) Number of Separations



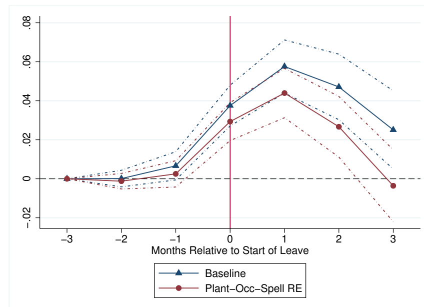
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from a modified version of equation 1 where plant-occupation fixed effects are replaced with plant-occupation-spell random effects. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window.

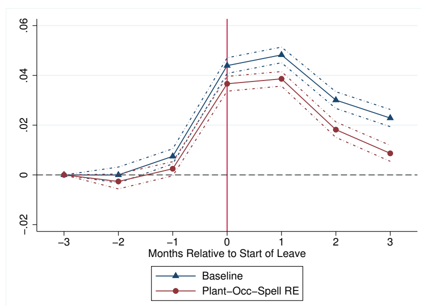
Figure A19: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Including Plant-Occupation-Spell Random Effects



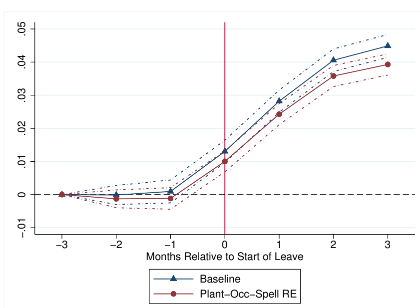
(a) Change in Number of Contracted Employees



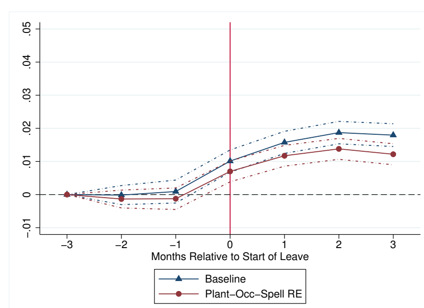
(b) Implied Number of Contracted Employees



(c) Number of Hires



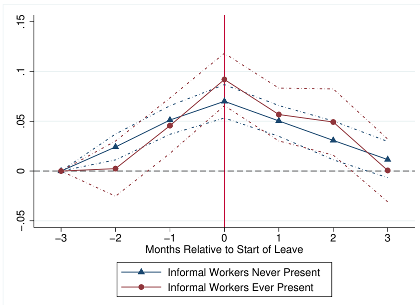
(d) Number of Separations



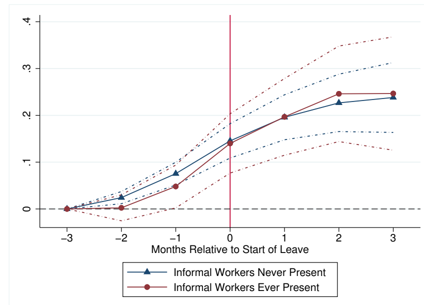
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from a modified version of equation 1 where plant-occupation fixed effects are replaced with plant-occupation-spell random effects. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window.

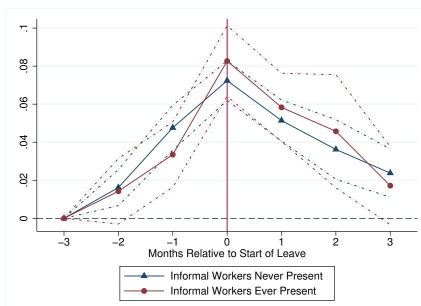
Figure A20: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Inspection Status from 2003–2011



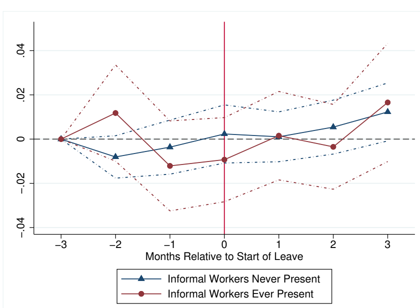
(a) Change in Number of Contracted Employees



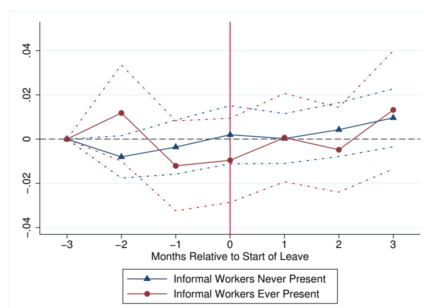
(b) Implied Number of Contracted Employees



(c) Number of Hires



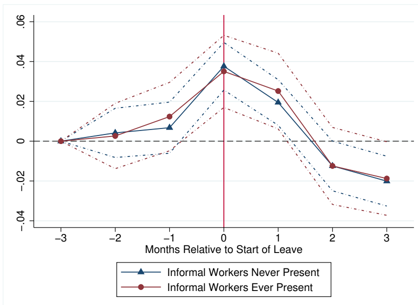
(d) Number of Separations



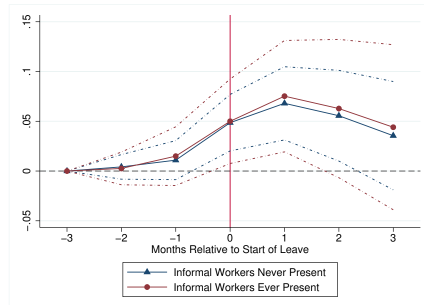
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately by plant inspection status. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the maternity leave-taker during the event window and only plants that ever had their employee registration inspected between 2003–2011.

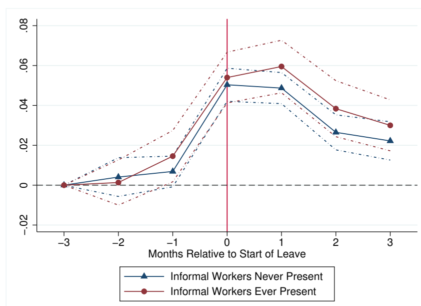
Figure A21: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Inspection Status from 2003–2011



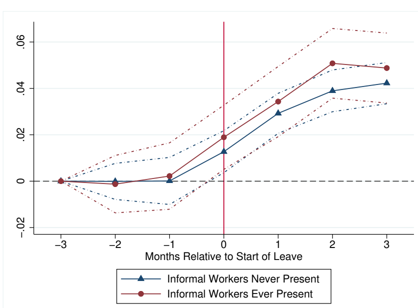
(a) Change in Number of Contracted Employees



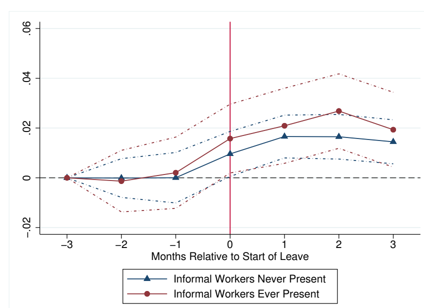
(b) Implied Number of Contracted Employees



(c) Number of Hires



(d) Number of Separations



(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately by plant inspection status. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. The sample includes plant-occupation groups of the sickness leave-taker during the event window and only plants that ever had their employee registration inspected between 2003–2011.