The Response of Firms to Maternity Leave and Sickness Absence*

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Abstract

We study how firms respond to predictable, but uncertain, worker absences arising 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 firms' employment, hiring, and separations. Firms respond immediately by increasing hiring, but the increase is substantially less than one-for-one replacement. Hiring responses are more pronounced for absences arising in occupations with more transferable skills and in firms operating in thicker labor markets. Overall, our results imply that using external markets is costly and firms manage absences through other channels.
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1 Introduction

On any given day, an employer can expect to learn that one of their employees is separating from their job, either permanently or to go on leave. Faced with the loss of that worker's labor, the employer must react. Seemingly the most obvious response would be to hire a new worker. If hiring were easy, turnover and leave-taking would barely register as an inconvenience. But employers generally report losing and replacing workers to be quite costly (Nicholson et al. 2006, Pauly et al. 2008).[1] If the external market is not sufficiently fluid, employers may instead invest in internal labor markets that make hiring a replacement unnecessary (Doeringer and Piore 1971). For example, they could maintain excess workers as a precaution (Jäger and Heining 2019) or coax coworkers to cover for an absent teammate (Hensvik and Rosenqvist 2019). Whether employers tend to use external or internal markets has significant implications for the design and evaluation of labor market policies, particularly leave mandates, that directly affect employment disruptions. However, the circumstances under which employers can simply hire replacement labor are not clear.

In this paper, we measure the importance of external markets for firms' ability to react to worker departures. Using Brazilian administrative data on over two million leave spells, we estimate changes in firm employment, hiring, and (permanent) separations in the months surrounding initiation of maternity and non-work-related sickness leave. In contrast to prior research on the effects of worker departure, by focusing on the short run, we can assume the firm's production and management practices are fixed. In Brazil, maternity and sickness leave are quite generous and publicly financed, so there is no direct cost of leave for the employer.[2] Therefore, our estimates should be interpreted as capturing firms' execution of their cost-minimizing plans for managing employment flows when workers take leave.

We find that firms respond immediately to the start of leave by hiring new workers and, to a much lesser extent, by limiting the rate of permanent job separations (quits and fires). The hiring responses, though immediate, are small. 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 0.04–0.07 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. We

[1] Their perceptions are supported by evidence on high costs of workforce adjustment (Kramarz and Michaud 2010), turnover (Kuhn and Yu forthcoming), and search frictions (Engbom and Moser 2018).

[2] Women are guaranteed 120 days of maternity leave with full wage replacement, financed by the Brazilian government. Sickness leave is publicly financed after the first 15 days. We focus on sickness absences that last longer than 15 consecutive days.

provide evidence that hiring prior to maternity leave reflects anticipation effects on the part of the employer, and not that leave is strategically timed to coincide with periods of employment growth.

To help organize and interpret our empirical findings, we introduce a simple model. Firms face random, but predictable, worker separations, and decide each period how much to invest in recruiting. In the short event windows we consider, firm production and recruiting technologies are fixed. As such, firm behavior is constrained by two parameters: an exogenous marginal cost of increased search intensity and an exogenous ability to use internal adjustments to replace the effort of the lost worker. Firm behavior is also affected by uncertainty about whether a leave-taker will return at the conclusion of their leave. The model's implications are straightforward: when the marginal cost of recruiting is sufficiently low, firms immediately hire a replacement when a worker separates. This "one-for-one" short-term hiring response corresponds to a frictionless spot market model. Conversely, if the returns to keeping a job filled are low relative to the returns to having it vacant, the firm will not change its hiring behavior at all in response to a worker's departure. This "null response" corresponds to the assumptions of search and matching models, where random hiring and departures balance to keep the probability a job is filled at a profit-maximizing level (Burdett and Mortensen 1998). Between these extremes, firms will increase recruiting intensity when a worker departs, but not by enough to immediately replace the worker with certainty.

Through the lens of the model, the magnitude of the change in hiring following a worker's departure depends on three factors: the cost of recruiting a replacement, the firm's ability to make internal adjustments, and, in the case of leave, the likelihood the worker returns. The cost of recruiting depends in part on the availability of workers with the requisite skills. 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 among firms in thicker labor markets. However, even when focusing on jobs and markets where hiring costs are expected to be low and workers can be more easily replaced with external hires, the hiring responses are still consistently small.

The most prominent differences in the hiring response are between maternity and sickness leave, which differ both in duration and in the likelihood the worker permanently separates at the conclusion of their leave. Maternity leave almost always lasts the full 120 days guaranteed by law. Interestingly, the average duration of sickness leave is quite long at 200 days, but sickness leaves are more likely to be short—the median duration is about 90 days. Workers often separate permanently after completing leave, and so firms know there is a high probability that they will need a permanent replacement. However, sickness leaves are less likely to end in permanent separation compared to maternity leaves. Given these differences in duration and probability of permanent separation, our model predicts a more muted hiring response to sickness leave initiations, consistent with our empirical findings.

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 support this assumption in several ways. 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-by-state 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 main contribution is to shed light on firms' short-run responses in the months following a worker's departure. In our short time window, firms have no scope to make significant changes, so our estimates capture the execution of cost-minimizing plans given fixed market conditions, technology, and management processes. Our work is closely related to two other papers that use annual data to study year-by-year employment dynamics after similar worker exits. Jäger and Heining (2019) study the responses by small German firms to sudden worker deaths, and Brenøe et al. (2020) use Danish data to study the effects of women taking maternity leave on firm employment. Our results suggest that the limited employment responses in the years following worker departures in Jäger and Heining (2019) and Brenøe et al. (2020) do not mask stronger short-run fluctuations. Furthermore, our analysis complements the study of worker deaths in Jäger and Heining (2019). The sudden death of a coworker is rare, but likely shocking, and even traumatizing to the firm's remaining employees and managers. We focus on vastly more common causes of workplace absence, and our findings may, therefore, better reflect how firms handle more common—and policy-relevant—employment disruptions.[3,4]

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

[4]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.

Gallen (2019), Ginja et al. (2020), and Friedrich and Hackmann (2021) examine how firms react in the years following policies that expand the generosity of family leave. Their estimates reflect how markets and firms adjust to changes in leave policy. In contrast, we are interested in employers' immediate responses to worker departure, holding technologies and market conditions fixed. Gallen (2019) finds limited effects of an unexpected and retroactively applied 2002 Danish reform that increased parental leave by 22 weeks on coworkers' employment or earnings. Ginja et al. (2020) exploit a similar reform in Sweden that increased paid parental leave from 12 to 15 months. There, private sector firms with greater exposure to the reform reacted by hiring temporary workers and increasing the hours of incumbent workers. Friedrich and Hackmann (2021) show that extended parental leave induced a long-run nurse shortage in Denmark with associated reductions in patient outcomes. 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.

More broadly, our work also contributes to the economic literatures on maternity and sickness leave. 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).[5] 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, Bütikofer et al. 2021). Less work has documented how firms respond to maternity leave-taking save for the exceptions discussed above.

2 Institutional Setting

In Brazil, the costs of leave arise primarily from personnel disruptions. Firms do not bear the cost of wage replacement during maternity leave or after the 15th day of a sickness leave spell. Here we briefly review Brazil's policies governing maternity and sickness leave as well as the costs associated with employment and firing. Leave-takers enjoy job protection during leave, and workers who return from maternity leave enjoy an additional month of job protection. The obligation to hold the job open means replacement hires may need to be temporary. While Brazil has high costs of termination in general, most of these costs do not bind for the first 90 days of employment, and are relatively small during the first year of employment. Thus, employers' ability to adjust to leave-induced absences depend on internal and market-based factors rather than institutional constraints.

[5] A recent exception is Pichler and Ziebarth (2020), which assesses how city- and state-level sickness pay mandates in the United States affect county- and state-level employment and wages.

2.1 Maternity Leave Policy

Brazil established maternity leave as a constitutional right in 1988. Article 7 (XVIII) of the Brazilian Constitution and Article 392 of the Consolidated Labor Laws describe the maternity leave entitlements. All women who are formally employed are eligible for benefits regardless of length of tenure at the employer. Women in the private sector are entitled to 120 days of paid maternity leave, which can start as early as the eighth month of pregnancy.[6] 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 and is reimbursed by deductions from owed contributions to the Brazilian Social Security Administration (INSS).

Since January 1, 2010, firms can offer longer leaves by joining the Empresa Cidadã (EC) Program.[7] Firms that choose to join must extend a woman's maternity leave an additional 60 days, to 180 days total. As with standard maternity leave, the wage replacement for the additional 60 days is ultimately covered by the government.[8] According to Machado and Pinho Neto (2018), fewer than 10 percent of eligible firms join the program, and they tend to be large. As we discuss later, the leave spells we consider in our analysis tend to originate from smaller firms.

2.2 Sickness Leave Policy

As our analysis focuses on non-work-related sickness leaves, we highlight the institutional details relevant for such leaves. Brazil's Consolidated Labor Laws provide mandatory paid sickness leave (referred to as *Auxílio-Doença*) for individuals who have contributed to Social Security for at least 12 months. The employer pays the employee's full salary for the first 15 days of absence. Thereafter, INSS pays the sickness leave benefits.[9]

Sickness absences must be certified by a physician and benefits are granted to those determined to be temporarily unable to work. If the individual is determined to be permanently disabled, they receive a disability pension instead. There is no maximum time limit on benefit receipt; thus, an individual receives sickness benefits until they are declared fit for work or permanently disabled. While receiving sickness benefits, the employee cannot be dismissed. Job protection upon returning to work does not apply to non-work-related sickness absences, and in practice,

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

[7] The federal government established the EC Program under Law 11,770 in September 2008. As part of this law, the federal government extended maternity leave to 180 days for its own employees. We exclude jobs with the federal government from our analysis.

[8] The employer pays the additional two months of leave payments to the woman, but deducts those payments from its owed income taxes.

[9] 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 start of the sickness leave (up to a cap).

workers are often dismissed once they return (Barbosa-Branco et al. 2011).

2.3 Costs of Employment

Brazil has relatively high costs of terminating workers. However, firms can hire workers on a 90-day probationary period during which the contract can be terminated without penalty. After the probationary period, the two main costs of dismissal are a severance payment and a 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. When the woman returns to work, if the firm needs to dismiss the new worker, they can expect to pay around 1.13 times the monthly wage as a termination cost.[10] Through the judicious use of probationary periods, a firm can mitigate termination costs associated with hiring replacements when a worker takes leave.

3 Data

We use matched employer-employee data from Brazil's *Relação Anual de Informações Sociais* (RAIS) between 2012–2017. Our goal is to estimate whether, and how, the hiring and retention behavior of occupation groups within plants (hereafter referred to as a plant-occupation) change in the months surrounding the initiation of either maternity or non-work-related sickness leave. To ensure that our results reflect firms' responses to a specific leave spell, we focus on what we call "clean" leave spells. A spell of leave is "clean" if the event window centered on the month of initiation does not intersect the event window of another leave spell at the same plant. Focusing on clean spells clarifies the interpretation of our model estimates, but also limits external validity by implicitly restricting the sample to plants that experience leave less frequently—primarily smaller plants.

3.1 Key Features of the RAIS Data

The RAIS is an annual census of all formal employment contracts collected by Brazil's Ministry of Labor and Employment (MTE). They collect information directly from the employing establishment, primarily for the purpose of administering social insurance programs. Compliance and

[10]To dismiss a worker employed on an open-term contract, the most common type of contract in Brazil that has no fixed expiration, the employer must provide cause. They must also provide advance notification 30 days prior to dismissal, with the advance notice period growing proportionately with the worker's tenure. 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. 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 (*Fundo de Garantia do Tempo e Serviço* or FGTS), which is funded by an 8.5 percent employer contribution each month. Employers generally expect to pay the FGTS penalty. Hence, the cost of firing a worker without cause is increasing in proportion to both tenure and the wage rate.

data accuracy are extremely high, as employers who fail to report face mandatory fines and risk litigation from employees. For each contract, the data contain 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,[11] 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.

Information on leave spells is available starting in 2007. The data provide information on up to three leave spells for each worker per year, including the start date, end date, and 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, we only consider sickness leaves longer than 15 consecutive days.

Some cleaning is required to combine information across recorded spells that correspond to the same period of absence. We combine leave spells that have a January 1 start date with spells of the same type that have a December 31 end date in the prior calendar year. For the combined spell, we assign the earliest leave start and latest leave end date across the component spells. Unfortunately, our extract of the RAIS data does not include leave spell details for 2011. Given we need prior calendar year information to correctly assign leave start dates, we restrict our attention to spells of leave that start after January 1, 2012.

3.2 Sample Construction

Our primary unit of analysis is a plant-occupation pair. We assign jobs to occupation groups using the first digit of the CBO occupation code as 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.[12]

Having defined plant-occupation pairs, we build a monthly panel measuring: the net change in the number of contracted workers as well as the number of workers hired into or permanently separated from the plant-occupation (including zeros).[13,14] We also define a measure of separations

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

[12] 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.

[13] 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.

[14] 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

excluding the leave-taker to determine whether separation responses reflect the behavior of other workers.

Our baseline model uses an event window that includes the month leave starts and the three months before and after (seven months total).[15] Henceforth, we refer to an event window by the number of months we consider before and after the month of leave onset (e.g., a 3-month window in our baseline analysis).

We exclude public sector plants and plants with military-related occupations (CBO code 0) as leave policies governing the public sector differ from those governing the private sector (e.g., federal and state governments must provide the maternity leave entitlements of the EC Program). We also exclude plants with fewer than five contracted workers during the majority of the period they are observed in our complete RAIS extract (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).[16] We do this to eliminate very small establishments composed of self-employed individuals as well as establishments where there are temporary periods with no contracted workers in a given 1-digit occupation.

3.3 The Construction of Clean Leave Spells

We define a spell of leave as "clean" if the event window centered on the month of initiation does not intersect the event window of another leave spell of the same type at that plant.[17] When using a 3-month event window, the leave spell is clean if it is the only leave of that type that starts at that plant in a 13-month span including the six months before and six months after it begins.[18] Given this requirement, our sample covers leave spells that begin from July 2012–June 2017. To avoid capturing the hiring of the leave-taker in the employment dynamics surrounding leave onset, we also require that the leave-taker have at least four months of tenure at the start of their leave.

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.

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

[16] 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.

[17] Although our unit of analysis is the plant-occupation, we define clean spells at the broader plant level because a leave in one occupation may impact 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.

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

3.4 Characteristics of Leave Spells

To interpret our empirical results, it is helpful to note several features of our sample and the nature of maternity and sickness leave spells. First, our focus on clean leave spells helps isolate employment responses to a specific cause, but at a cost of inducing sample selection. The restriction to clean spells tilts the analysis toward small plants. Second, employers see both maternity leave and sickness leave as relatively long-term—and likely permanent— departures. The vast majority of maternity leaves last 120 days and the sickness leaves we consider are quite long on average, and it is very common for workers to not return to their job after leave.

To document the effects of sample selection, Table 2 reports descriptive statistics for the clean maternity and sickness leave spells in our estimation sample in columns 1 and 3, respectively. We compare them to the full population of leave spells that do not satisfy the clean spell definition, but otherwise meet the sample restrictions (e.g., no public sector plants) in columns 2 and 4.[19]

Our focus on clean spells is restrictive. Approximately 34 percent of maternity leaves and 19 percent of sickness leaves meet our clean definition. The smaller 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, 37 percent of sickness leaves satisfy the clean definition.

Even though they are only 34 percent of all spells, the characteristics of clean maternity leave spells are similar to the full sample. The top industries represented are wholesale/retail trade and manufacturing, and over 60 percent 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. More than 95 percent of clean maternity leaves last at least 120 days, with the vast majority lasting 120–124 days. 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 establishments with less than 100 employees, whereas about one-third of the leaves in the full sample originate from plants with 100 or more employees. This discrepancy arises largely from our definition of a clean spell. Even if leave initiations were timed independently of one another, 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

[19] 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.

dimensions, the clean leave spells are representative of maternity leaves taken during this period.[20]

Turning to sickness leaves, the clean spells are also quite similar to the full sample. For example, sickness leave-takers have on average 40–45 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 200 days. The median spell (not reported in the table) is 92 days. Recall that we only consider sickness leave spells that last longer than 15 consecutive days as shorter spells do not need to be reported in the RAIS, so our administrative data largely capture longer 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.[21]

As is the case with maternity leave, the key difference between our 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 about 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 representative of responses to leave-taking among smaller firms.

Our implied focus on smaller firms is consistent with 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.[22]

Departures of workers going on maternity leave and sickness leave are quite likely to become permanent. In Figure 1 (a), we plot Kaplan-Meier survival curves for the maternity leave-takers in our estimation sample, where survival means 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

[20] There is some evidence that fertility and take-up of parental leave are subject to social influence (Balbo and Barban 2014, Dahl et al. 2014). If that were the case here, then our clean spells may be selected on periods when firms experience relatively few leaves and are relatively unprepared to handle them. However, in Brazil, maternity leave is widely accepted, and expected, leaving little scope for meaningful social contagion effects within plants.

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

[22] 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.

leave onset, half of the leave-takers have separated from the plant. Thus, employment dynamics surrounding the initiation of a maternity leave 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 1 (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.[23] The survival probability declines significantly within six months after sickness leave ends, and about one-third of the leave-takers separate from the plant within a year after their 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.[24] 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

We base our analysis on the following event study model of employment dynamics:

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

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

[24]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.

where y_{opt} denotes outcome y for 1-digit occupation group o at plant p in month t. Our outcomes of interest include the number of workers hired during the month and the 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. The variable K_{pt} measures event time relative to the month a leave spell started at plant p.[25] That is, $K_{pt} = 0$ in the month of leave onset, $K_{pt} = 1$ in the month after the leave starts, etc. ϕ_{op} are plant-occupation fixed effects and τ_t are calendar time fixed effects (where time is measured in year-months). We cluster standard errors at the leave spell level and estimate equation (1) separately for maternity leave and sickness leave spells. We primarily estimate equation (1) on a sample that only includes the plant-occupation of the leave-taker. To detect spillover effects, we also estimate equation (1) on other occupations within the same plant as the leave-taker. In all analyses, we only include plant-occupation observations for periods corresponding to the event window. Thus, the sample is balanced around event time.

The coefficients of interest, β_k, measure the effects of leave-taking on hiring, separations, and employment growth k months relative to the month a leave spell starts. We normalize the effect three months prior to the leave start to zero ($\beta_{-3} = 0$). Under our maintained modeling assumptions described below, the β_k coefficients are identified relative to the counterfactual evolution of hiring, separations, and employment growth had a leave spell not been active during the event window. Notably, identification of the β_k coefficients does not require that leave onset is unexpected. 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. If these assumptions hold, the event study estimates measure exactly what we want: the firm's behavior triggered by the realization of a predictable, but uncertain, event. If they do not, then the estimates reflect some combination of the firm's reaction and whatever employment dynamics lead workers to start a spell of leave. Note that because our outcomes are measures of employment *change*, the inclusion of plant-occupation fixed effects allows for arbitrary trend growth in plant-occupation employment.

Hence, the key assumption identifying the event study coefficients in equation (1) is that the timing of leave initiation is unrelated to deviations in employment growth from its plant-occupation-specific trend.[26] This condition—exogenous timing—is satisfied as long as workers do not time leave

[25] 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.

[26] 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 (forthcoming), 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.

to begin when, say, the firm is experiencing an abnormally large expansion or contraction in employment. Identification also requires that at least one of the pre-leave onset months be unaffected. This second condition—delayed timing—is satisfied if firms do not react to leave-taking until two months before leave onset (or later). We provide evidence that both of these conditions are satisfied.

The exogenous timing and delayed timing conditions are straightforward to justify for sickness leave spells. Although employers know there is a probability of a worker taking sickness leave in any month, they do not know exactly when that probability will be realized. Our model also assumes workers do not time sickness leave with respect to deviations in employment growth from plant-occupation-specific trends. If anything, workers might time leave-taking to labor market conditions, which we control for with calendar time effects. In some specifications, we allow the calendar time effects to vary by the industry-state pair and results are quite robust. The delayed timing condition is justified by the notion that sickness leave is essentially a surprise when it starts. A testable implication is that the firm's response should take place entirely after the leave spell begins. The data confirm this: we generally observe that hiring responses occur in the month leave starts and after.[27]

Maternity leaves also likely satisfy the exogenous timing condition. It is difficult and uncommon to precisely time pregnancy. During the period of our study, around 55 percent of births in Brazil were unintended, meaning mistimed or unwanted.[28] While the timing of pregnancy could still be endogenous to labor market conditions, which we control for via calendar time effects, it is unlikely to be timed with respect to deviations in employment growth from its plant-occupation-specific trend. The delayed timing assumption is more nuanced in the case of maternity leave. The start of a maternity leave spell almost certainly does not come as a surprise to the employer. Brazilian labor law requires women to notify their employer in advance of maternity leave, and job protection begins at that point, so women have a strong incentive to notify early. Firms may respond by hiring in advance of a maternity leave start. As such, different than sickness leave, we expect to see effects on employment prior to leave onset. The delayed timing condition in this case means the firm's employment response begins closer to when maternity leave begins, which yields the 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 employment dynamics that generate endogenous timing of maternity leave. In Section 5.3.1, we consider longer event windows and provide empirical support for the delayed timing assumption.

[27]When they do not, the early response can presumably be explained by sickness leaves due to procedures and treatments that are usually known in advance, such as major surgery or cancer treatment.

[28]The 2006 Brazilian Demographic and Health Survey finds 54 percent of births were unintended (Ministério da Saúde 2009). The Birth in Brazil survey, which interviewed and examined medical records of 23,940 mothers from February 2011–October 2012, found 55 percent of pregnancies were reported as unintended (Theme-Filha et al. 2016).

A 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 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.[29] 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 plant-occupation-leave spell effects, we need an additional normalization or source of information to identify the trend in calendar time. The simplest solution is to include control groups, which we explore in Section 5.3.2.[30]

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.[31] 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.[32] 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 them separated unless their contract

[29] 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.

[30] 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 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 more granular leave spell random effects.

[31] 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.

[32] 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.

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 2 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 2 (a) and (b) show plant-occupations experience relatively small but statistically significant increases in employment two months before leave onset, and sharper 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 2 (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 2 (e), we present results for separations excluding the leave-taker, and find nearly identical increases. 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 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 3. 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 0.007 workers on net (Figure 3 (a)). These results support our identifying assumption that sickness leave is not strategically timed based on idiosyncratic plant dynamics, and are consistent with the exact start of a sickness leave coming as a surprise to the firm. Figures 3 (a)–(c) show that in the month of leave onset, the plant-occupation sees an increase in employment, driven by an 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 0.07 workers larger the following month relative to three months before leave onset. Separations steadily increase after the leave initiates, peaking at 0.045 three months after the onset of leave (Figure 3 (d)). When we consider separations excluding the leave-taker (Figure 3 (e)), they increase and plateau around 0.02. 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 predictable, but uncertain, worker absences 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. Firms are typically aware of maternity leave 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 4 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 of the other 1-digit occupations at the plant 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 A1 and A2.

The results show little evidence of statistically significant or economically meaningful spillovers of maternity leave-taking. There is 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.[33] That the most notable spillovers prior to maternity leave 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.[34] Second, the absence of spillover effects, particularly prior to leave onset, is consistent with our identifying assumptions. If workers time leave to coincide with business conditions that generate deviations in employment growth from its plant-occupation-specific trend, 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.[35]

5.3 Robustness

Our main estimating equations are identified under the assumptions that (i) leave is not timed to coincide with deviations in employment growth from its plant-occupation-specific trend (ii) employment outcomes three months prior to leave initiation are not correlated with leave-taking (i.e., they are untreated).[36] 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

[33]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.

[34]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.

[35]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 A3 and A4, 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.

[36]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.

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 2 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 a large acceleration of employment growth.[37] 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.[38] 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 5. The results for all outcomes from the extended event window analyses are presented in Appendix Figures A7–A10. 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 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 sharp 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.[39] 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

[37]As mentioned earlier, Sun and Abraham (forthcoming) show that pre-trends can arise in settings with variation in treatment timing when there is cohort-specific treatment effect heterogeneity. The increase in hiring prior to maternity leave onset may reflect such heterogeneity. However, in Appendix Figures A5 and A6, we show that estimated event time coefficients from the first half of our estimation period are indistinguishable from those in the second half.

[38]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.

[39]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.

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 maternity 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 5 (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 5 (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 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.[40]

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.[41] 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 6 (a) and (b), we compare the baseline estimates to estimates from models that include (i) industry-state-specific calendar time effects

[40] For both maternity and sickness leaves, 75 percent of the spells in our baseline analysis meet the requirements for the 4-month window analyses; 58 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 samples in Figures A11 and A12.

[41] 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 A13–A16.

and (ii) no calendar time effects.[42] For both types of leaves, not including calendar time effects leads us to underestimate effects on employment. When we include industry-state-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 regional 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 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 6 (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 similar to our baseline results. 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.[43]

[42] Industries are defined using the 21 major sectors in the *Classificação Nacional de Atividades Econômicas* (CNAE) 2.0. There are 26 states in Brazil.

[43] 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 A17 and A18 show estimated employment dynamics from models with plant-occupation-spell random effects (without control units). The estimates are smaller in magnitude than the baseline estimates, but generally quite similar. These results support the notion that leave spells are timed randomly with respect to plant-occupation outcomes.

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 show later in Section 6.2 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, the data contain the month the inspection began, the plant being inspected, each aspect of the labor law that was inspected, and whether the plant was found in violation of the relevant law.[44] We 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 specifically inspected for potential employee registration violations, so they should be similar on characteristics that predict the use of informal contracts.[45]

Figure 7 (a) shows the results for employment levels for maternity leave spells.[46] The employment dynamics are nearly identical across firms regardless of whether they have had informal worker violations. Figure 7 (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

[44] These data are different than the aggregate state-level inspection data used by Almeida and Carneiro (2012).

[45] In Appendix Table A2, we show plant characteristics associated with clean leaves separately by inspection status. Of note, larger establishments are more likely to ever be inspected and to have informal worker violations.

[46] Again, in the interest of space, we focus on employment in levels, but results for all outcomes are presented in Appendix Figures A19 and A20.

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.

6 The Influence of Job Characteristics and Market Conditions

We have shown that employers respond to a new maternity or sickness leave spell by increasing hiring. In both cases, they increase hiring by substantially less than would be expected in a frictionless spot market. We argue that the small hiring response reflects employers' use of internal labor markets, and these responses should be less muted in jobs where workers who separate permanently or temporarily are easier to replace with external hires.

To build intuition, we introduce a simple model that highlights the aspects of managerial behavior we are interested in, and the factors that constrain that behavior. The model implies that the hiring response to leave should be stronger when there is (1) low capacity for using internal substitutes; and (2) low marginal costs of recruiting on the external market. The change in recruiting effort induced by a separation will also be larger when the probability the worker returns from leave is low. Indeed we find that hiring responses are stronger in non-managerial occupations, in thicker labor markets, and for maternity leave-takers who have completed less tenure, cases where the marginal costs of recruiting a replacement from the external market are arguably relatively low. However, while the patterns are qualitatively consistent with the intuition captured in the model, they are quantitatively small, suggesting that any differences across jobs and labor markets are not substantial enough to overcome the forces driving firms toward internal adjustments.

6.1 Model

We consider the behavior of a manager whose objective is to maximize firm profits. They are responsible for recruiting workers to cover a fixed set of jobs. Event time is discrete, and at the start of each period, a job is either in a filled ($j = 1$) or unfilled ($j = 0$) state. The marginal revenue net of labor costs for the job is R_j. The difference $R_1 - R_0$ captures the extent to which the firm can find ways to pick up the slack temporarily when a worker separates. For example, if the firm can keep production at the same level by increasing demand for internal workers, as discussed in Jäger and Heining (2019), we might have $R_1 = R_0$.

In each period, the firm manager chooses how much effort to put into recruiting a worker to cover the job in the next period, $s_j \in [0, \bar{s}]$, at a cost $c(s_j)$. With probability $p(s_j)$ the manager finds a viable candidate and hires them if the job has fallen vacant. The probability of successfully hiring increases in recruiting intensity, $p'(s_j) > 0$. By choosing recruiting effort $\bar{s}$, the manager can hire with certainty, $p(\bar{s}) = 1$. If the manager does not engage in any recruiting effort, $s_j = 0$, they hire with probability $p(0) \geq 0$. That is, we allow for an exogenous arrival rate of workers to the firm even in the absence of recruiting effort. When the job is filled, $(j = 1)$, the incumbent worker separates with probability δ. When the job is unfilled, $(j = 0)$, the original worker will fail to return next period with probability τ, reflecting the possibility they were on leave. For regular separations, $\tau = 1$.[47]

The manager's value function for the filled job is:

$$V_1(s_1) = R_1 - c(s_1) + \beta \left\{ \left[1 - \delta(1 - p(s_1))\right] V_1(s_1) + \delta(1 - p(s_1))V_0(s_0) \right\}, \tag{2}$$

where β is the discount rate. For the unfilled job, the value function is:

$$V_0(s_0) = R_0 - c(s_0) + \beta \left\{ \left[1 - \tau(1 - p(s_0))\right] V_1(s_1) + \tau(1 - p(s_0))V_0(s_0) \right\}. \tag{3}$$

The spot market outcome corresponds to the case where $s_1 = 0$ and $s_0 = \bar{s}$; the manager does not do any precautionary hiring, and hires with certainty in the event a worker leaves. The extreme frictional search outcome has $s_1 = s_0$; the manager does not alter recruiting behavior at all in response to worker separation.

At an interior solution, the optimal s_1 and s_0 must satisfy

$$\frac{c'(s_1)}{p'(s_1)} = \beta\delta \left[V_1(s_1) - V_0(s_0)\right] \tag{4}$$

and

$$\frac{c'(s_0)}{p'(s_0)} = \beta\tau \left[V_1(s_1) - V_0(s_0)\right]. \tag{5}$$

Both expressions imply that at an optimal interior recruiting intensity, the marginal cost of increased recruiting intensity is equal to the discounted value of filling an empty job.

We are also interested in corner solutions. The case $s_j = \bar{s}$ means the firm makes a direct hire immediately upon a worker's departure, while $s_j = 0$ means the firm does not expend any effort

[47] In a more elaborate setup, firms are exposed to both quits and leave-taking at different rates. When leave spells are a small share of all separations, the firm's hiring response to leave can also be muted.

in recruiting. For a filled job, the corner solution where $s_1 = 0$ implies:

$$\frac{c'(0)}{p'(0)} > \beta\delta\left[V_1(0) - V_0(s_0)\right]. \tag{6}$$

Conversely, if $s_1 = \bar{s}$ for a filled job:

$$\frac{c'(\bar{s})}{p'(\bar{s})} < \beta\delta\left[V_1(\bar{s}) - V_0(s_0)\right]. \tag{7}$$

The corner solutions for the unfilled job are similar:

$$\frac{c'(0)}{p'(0)} > \beta\tau\left[V_1(s_1) - V_0(0)\right]; \tag{8}$$

and

$$\frac{c'(\bar{s})}{p'(\bar{s})} < \beta\tau\left[V_1(s_1) - V_0(\bar{s})\right]. \tag{9}$$

Result 1: The case $s_1 = 0$ and $s_0 = \bar{s}$, the spot market outcome, is more likely when

1. the probability of separation, δ, is much smaller than the probability a worker does not return from leave, τ;
2. the gap $R_1 - R_0$ is large;
3. recruiting costs are high; but
4. $c'(s_j)$ increases slowly relative to $p'(s_j)$.

Result 2: The case $s_1 = s_0$, the extreme frictional search outcome, requires either the knife-edge case $\delta = \tau$ or $R_1 = R_0$. In the latter case, $s_1 = s_0 = 0$ and the firm is indifferent between having the job filled or unfilled.

In words, hiring responses are muted when internal substitutes are easy to find and the marginal cost of increased recruiting effort is high. They are also muted when workers are very likely to return from leave, particularly in combination with these other factors.

6.2 Heterogeneity by Occupation

Motivated by the model predictions, we examine heterogeneity with respect to characteristics that distinguish jobs and employers by different types of labor market flexibility. Here, we estimate our baseline model separately for managerial, technical, and production workers.[48] The idea is that these coarse occupational groupings capture differences in the generality of worker skill, and hence differences in the marginal cost of increased recruiting effort. The maternity leave results in Figure 8 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 9, 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. Through the lens of our model, the smaller response to sickness leaves of managers is consistent with a higher marginal cost of recruiting managerial skill as well as managers being relatively more attached to the firm (higher τ), decreasing the employer's need for a permanent replacement.

6.3 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 and, therefore, the marginal cost of increased recruiting effort is relatively low. 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. That is, in thick markets, we also expect the marginal cost of increased recruiting activity to be relatively low. 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

[48] As discussed in Section 3, these 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).

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 10 and 11, 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 11 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. Our results are consistent with Jäger and Heining (2019) and Ginja et al. (2020), who also find external hiring is more likely in thick markets. 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.

6.4 Heterogeneity by Leave-Taker Tenure

We expect that firms' marginal cost of increased recruiting effort will also vary with the leave-taker's tenure at the firm. This should be the case whether higher tenure reflects that a worker has more job- or firm-specific human capital as in Topel (1991), or that a worker is better matched to their job, as in Jovanovic (1979). In either case, the employer would find it harder to replace a worker with high tenure than one with lower tenure. The RAIS data include the exact date of hire for each job, which we use to group workers into terciles based on their tenure in the month that leave begins. For maternity leave-takers, jobs in the first tercile have tenure of 13 months or less; jobs in the second tercile have between 14 and 29 months of tenure; jobs in the third tercile have more than 29 months of tenure. For sickness leave-takers, jobs in the first tercile have 15 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 12 shows that hiring and employment responses to maternity leave are substantially larger when the worker is in the first tercile of the 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.

[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).

The patterns are different for sickness leave, as illustrated in Figure 13. 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. The data show that workers with less tenure tend to take shorter leaves, which could influence employers' behavior.[50] We explore this idea in Section 6.6.

6.5 Heterogeneity by Plant Size

The model above implies there may be factors internal to the firm associated with its responsiveness to a worker's departure, as represented by $R_1 - R_0$. In particular, larger plants may (a) find it easier to spread work to current employees (Jäger and Heining 2019, Hensvik and Rosenqvist 2019); and (b) have human resource management systems that facilitate internal employment adjustments (Holzer 1987). Thus, we might expect larger plants to be less responsive to worker departures than smaller plants. Figure 14 shows results where we allow employment dynamics around maternity leave to vary across plants with 1–4, 5–9, 10–19, and 20–49 workers.[51] These size classes account for almost all (90 percent) of our clean spells. Figure 15 shows the analogous results for sickness leave.

For both forms of leave, there is no indication that larger plants are less responsive. Instead, employment dynamics are basically identical across size classes, except for plants in the smallest class of 1–4 employees. For them, the hiring response is smaller. This is particularly true for sickness leave, where the hiring response is at least half as large as the other three size groups. It may be that small plants expect sickness leaves to be of shorter duration and are therefore more likely to impose on their existing workers. Another explanation for the lack of sharp heterogeneity by establishment size is that while larger plants may indeed be able to make internal adjustments more easily, they may also face lower hiring costs if they have resources or systems in place that are regularly

[50] The median sickness leave lasts 86 days for workers in the lowest tercile of tenure, while for workers in the top tercile, the median duration is 102 days.

[51] Plants are allocated into size classes based on their modal end-of-year size across years 2012–2017. Thus, there should be no mechanical relationship between the size class and the response to the leave spell.

devoted to recruitment or if they have a larger applicant pool regardless of their recruitment efforts.

As a caveat, our selection of clean spells makes interpretation somewhat complicated. In particular, a large plant with clean spells does not experience leave-taking at the same rate (per worker) as a small plant with clean spells. Large plants with clean spells may have taken other steps to reduce exposure to leave-taking. Presumably, these would be the plants least able to deal with leave-taking internally. However, the fact that there is limited effect heterogeneity suggests sample selection is unlikely to fully explain the results. In any event, the data provide no evidence that larger plants are less responsive to a sudden worker departure.

6.6 Heterogeneity by Predicted Sickness Leave Duration

Our main results show that firm responses to sickness leave are much smaller than their responses to maternity leave. As suggested by the model, this could be driven by differences in the distribution of leave durations and the probability the leave-taker returns (corresponding to τ in the model). In Section 3.4, we showed that workers are more likely to separate permanently after maternity leave. Furthermore, while sickness leaves are longer on average than maternity leaves, a large fraction of sickness leaves are shorter than 90 days. Intuitively, employment responses to sickness leave may be muted when they are too short to bother hiring a replacement worker.

Pushing this idea further, we allow for heterogeneous employment dynamics around sickness leaves of different predicted length. Rather than condition on actual duration, a post-determined outcome, we condition on predicted duration based on leave-taker characteristics and plant fixed effects.[52] We group leaves into terciles of predicted duration. The first tercile includes leave spells predicted to last up to 102 days; the second tercile includes those predicted to last between 103 and 152 days; and, the third tercile includes spells predicted to last 153 days or more.

Figure 16 shows hiring and net employment responses are smaller in the first two months after leave onset among spells in the second predicted duration tercile compared to those in the first tercile, which aligns with the model prediction that hiring responses should be larger when the probability of the leave-taker's return is smaller. However, the response associated with spells in the third predicted duration tercile (over 152 days) is muted relative to spells in the second tercile. Those predicted to have very long sickness leaves tend to be older and have long tenure, workers that may be especially difficult to replace with external hires. In addition, Figures 16 (d) and (e) demonstrate that individuals predicted to have shorter leaves are also more likely to permanently separate from the

[52] We predict duration as a function of leave-taker gender, age, race, education, occupation, and tenure as of the start of their leave spell as well as plant fixed effects. The sample for the prediction analysis includes all sickness leaves greater than 15 consecutive days that meet our sample selection criteria, but we do not impose the clean spell criteria. We truncate leaves at 365 days.

firm at the conclusion of their leave. Thus, part of the response to short predicted leaves may reflect the likelihood that a permanent replacement is needed. These results underscore the complexities managers face as they make decisions about how to handle absences associated with sickness leave.

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.[53] 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.[54] 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.[55,56] 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 specifications similar to those in Section 5.3.2, using non-leave-taking occupations in the plant of the leave-taker as control groups.[57]

Figure 17 (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 stays 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 17 (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

[53] Unfortunately, we cannot examine how work hours change as the data do not provide a reliable measure of hours (or days) worked.

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

[55] 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 at the start and end of their leave.

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

[57] 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 (513,478) 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.

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 17 (c) shows that the wage bill of the leave-taker's occupation is stable prior to leave onset, but then drops sharply by about 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 returning to work among other margins of adjustment. When we exclude the focal leave-taker's earnings for the full event window, Figure 17 (d) shows that the wage bill is about 50–70 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 respond to a leave spell by increasing hiring, but the increase is substantially less than one-for-one replacement of the leave-taker. 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 some heterogeneity arising from differences in the labor markets in which firms operate and the jobs of leave-takers. Consistent with a basic model of managerial responses to separation risk, hiring responses are somewhat more pronounced for absences arising in production and technical occupations, in thicker labor markets, and when the absent worker has less tenure.

Overall, however, the results do not vary much across different kinds of jobs, firms, or local labor markets, suggesting there are broader impediments to hiring. We conclude that Brazilian firms must actively manage internal labor markets in anticipation of predictable labor supply disruptions. Similar findings for Denmark (Brenøe et al. 2020) and Germany (Jäger and Heining 2019) suggest our results are not driven by the labor market imperfections in Brazil documented by Engbom and Moser (2018).

We do not find evidence of meaningful increases in compensation for workers in the short run, leaving a bit of mystery regarding exactly how firms adjust to leave-taking. Institutions may play some role, particularly those constraining hours adjustments (Labanca and Pozzoli forthcoming). The extent to which incumbent hours can be adjusted is limited in Brazil. The maximum number of working hours per week is 44, the maximum length of a continuous shift of work is six hours, and overtime pay is 1.5 times the normal wage. Collective bargaining agreements likely tighten these restrictions further (Lagos 2019). Against this backdrop, firms might negotiate implicit contracts where workers tolerate fluctuating work schedules or work assignments and receive fixed earnings, somewhat in the manner proposed by Abowd and Card (1987).

Our findings have implications for the design and evaluation of policy, particularly those—like leave mandates—that cause employment disruption. Firms may design internal labor markets that directly discourage leave-taking (Bana et al. 2018), or that change the composition of workers to avoid exposure to leave-taking (Ginja et al. 2020). Either response can exacerbate disparities between men and women and more generally counteract intended policy goals (Goldin 2014). Accounting for these managerial responses should be a first-order concern in developing and enforcing leave policies.

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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	0.041	0.054
Mining	0.001	0.002	0.004	0.006	0.005
Manufacturing	0.105	0.159	0.137	0.238	0.175
Utilities: Electric/Gas	0.001	0.001	0.001	0.003	0.002
Utilities: Water/Sewage/Waste	0.002	0.003	0.004	0.011	0.006
Construction	0.017	0.015	0.040	0.061	0.060
Wholesale/Retail Trade	0.429	0.318	0.365	0.216	0.320
Transportation/Storage/Mail	0.026	0.026	0.051	0.076	0.062
Accommodation/Food	0.098	0.074	0.082	0.051	0.079
Information/Communication	0.015	0.022	0.013	0.016	0.012
Financial Services	0.032	0.034	0.025	0.020	0.025
Real Estate	0.007	0.004	0.005	0.002	0.004
Professional/Scientific/Technical	0.042	0.032	0.028	0.018	0.022
Administrative Activities	0.054	0.105	0.073	0.116	0.067
Public Admin/Defense/Social Security	0.000	0.001	0.000	0.002	0.001
Education	0.046	0.058	0.033	0.027	0.032
Health/Social Services	0.057	0.090	0.032	0.067	0.035
Art/Culture/Sports	0.009	0.006	0.008	0.005	0.007
Other Service Activities	0.041	0.036	0.039	0.026	0.034
# of Workers on Dec 31					
0	0.020	0.018	0.009	0.011	0.022
1 to 4	0.267	0.121	0.274	0.086	0.169
5 to 9	0.245	0.119	0.255	0.084	0.165
10 to 19	0.223	0.134	0.237	0.100	0.196
20 to 49	0.170	0.155	0.176	0.133	0.260
50 to 99	0.049	0.095	0.040	0.097	0.189
100 to 249	0.020	0.103	0.008	0.123	0.000
250 to 499	0.004	0.068	0.000	0.094	0.000
500 to 999	0.001	0.059	0.000	0.087	0.000
1000+	0.000	0.130	0.000	0.184	0.000
Female Employee Share	0.645	0.624	0.429	0.408	0.418
	(0.286)	(0.249)	(0.339)	(0.298)	(0.322)
Female Aged 21–35 Employee Share	0.405	0.370	0.209	0.202	0.206
	(0.271)	(0.220)	(0.231)	(0.182)	(0.209)
Plant-Occupation Characteristics					
Female Employee Share	0.776	0.717	0.430	0.408	0.417
	(0.260)	(0.246)	(0.402)	(0.351)	(0.384)
Female Aged 21–35 Employee Share	0.528	0.454	0.197	0.195	0.196
	(0.322)	(0.272)	(0.279)	(0.221)	(0.258)
Leave-Taker Characteristics					
Avg Tenure (Months)	30.417	30.876	45.226	44.841	40.502
	(29.928)	(31.570)	(54.764)	(61.177)	(54.851)
Avg Leave Length (Days)	120.425	119.854	199.926	195.789	193.533
	(21.918)	(26.107)	(283.995)	(293.576)	(284.265)
Female	1.000	1.000	0.455	0.454	0.448
1-Digit Occupation					
Public Administration & Management	0.052	0.042	0.038	0.020	0.031
Professionals in Science and Arts	0.074	0.102	0.037	0.039	0.037
Mid-level Technicians	0.072	0.097	0.056	0.079	0.059
Administrative Services	0.359	0.334	0.176	0.172	0.171
Service Workers & Vendors	0.363	0.292	0.351	0.282	0.318
Agriculture, Fishermen, & Forestry	0.011	0.011	0.056	0.043	0.048
Production/Manufacturing I	0.055	0.094	0.220	0.281	0.264
Production/Manufacturing II	0.012	0.025	0.034	0.055	0.043
Repair & Maintenance	0.002	0.003	0.031	0.029	0.029
# of Leaves	1113037	3257633	1160586	6076424	3108295

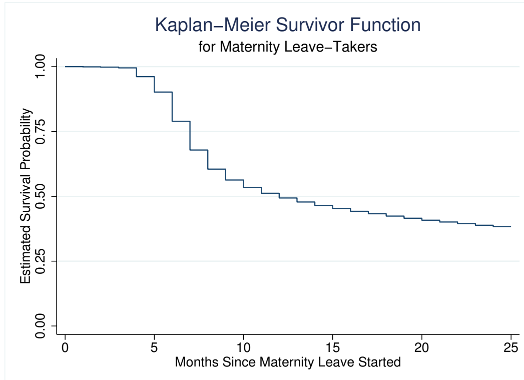
Note: All statistics are measured in the month of leave onset with the exception of establishment size, which is measured at the end of the calendar year of the leave initiation. Columns (1) and (3) include clean maternity and sickness leave spells, respectively. Columns (2) and (4) include maternity 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

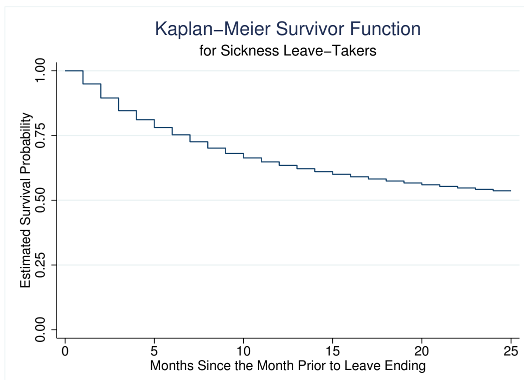
	Maternity		Sickness	
	Own Plant-Occ	Spillover Plant-Occ	Own Plant-Occ	Spillover Plant-Occ
Change in # of Contracted Employees	-0.001	-0.007	-0.012	-0.004
	(2.475)	(3.201)	(1.669)	(1.521)
# of Contracted Employees	8.866	6.659	8.694	4.446
	(20.781)	(22.278)	(13.726)	(9.767)
# of Hires	0.372	0.270	0.343	0.155
	(1.979)	(3.837)	(1.450)	(0.930)
# of Separations	0.373	0.277	0.355	0.159
	(2.295)	(3.978)	(1.560)	(1.510)
# of Plant-Occupations	1113037	1900599	1160586	1831758

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



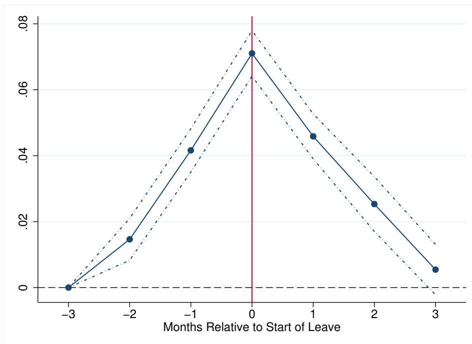
(a) Maternity Leave-Takers



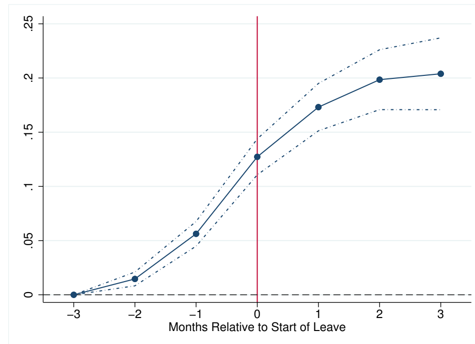
(b) Sickness Leave-Takers

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 months since the month prior to sickness leave ending. Survival is defined as still being contracted with the plant.

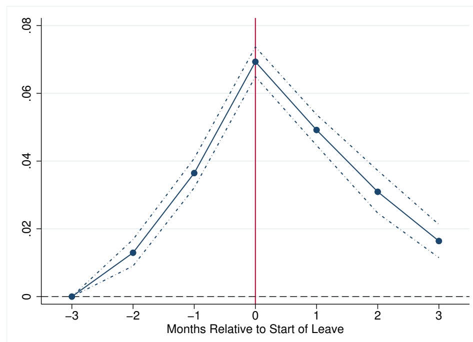
Figure 2: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker



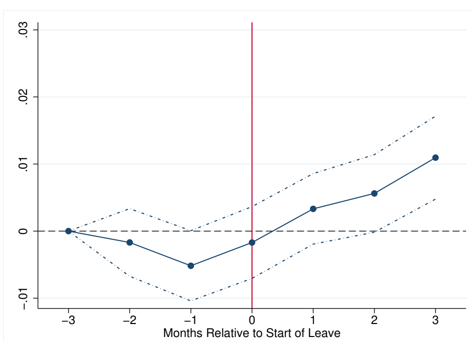
(a) Change in Number of Contracted Employees



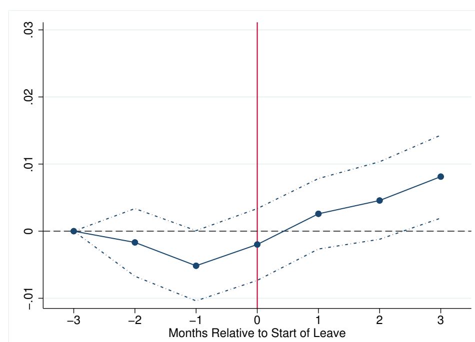
(b) Implied Number of Contracted Employees



(c) Number of Hires



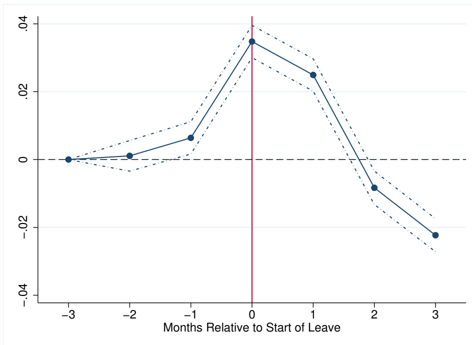
(d) Number of Separations



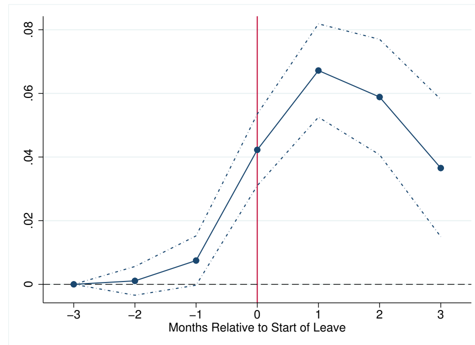
(e) Number of Separations Excluding Leave-Taker

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.

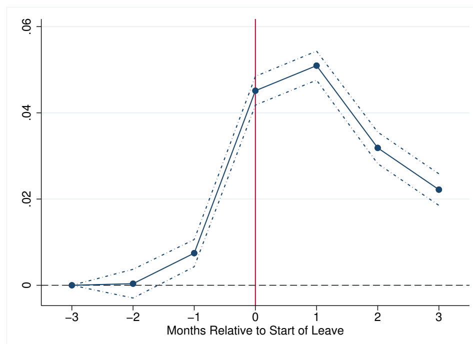
Figure 3: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker



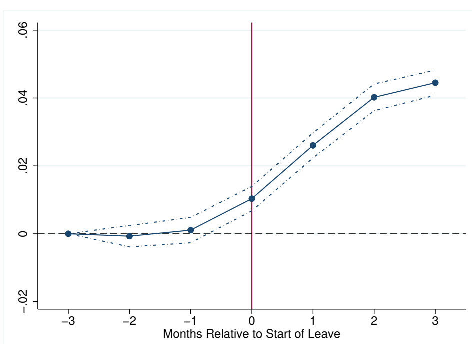
(a) Change in Number of Contracted Employees



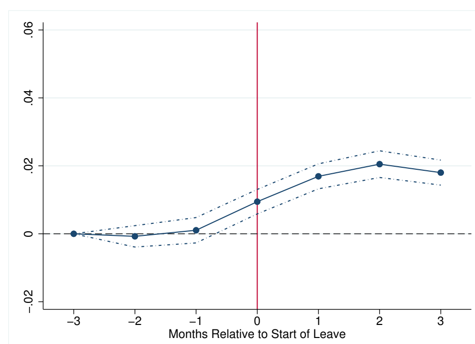
(b) Implied Number of Contracted Employees



(c) Number of Hires



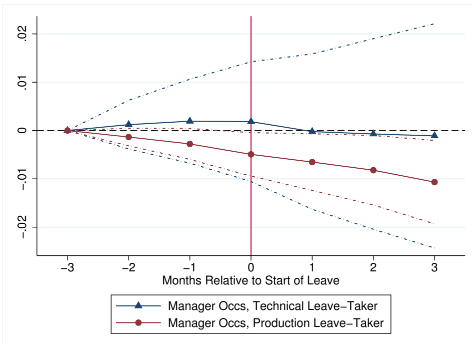
(d) Number of Separations



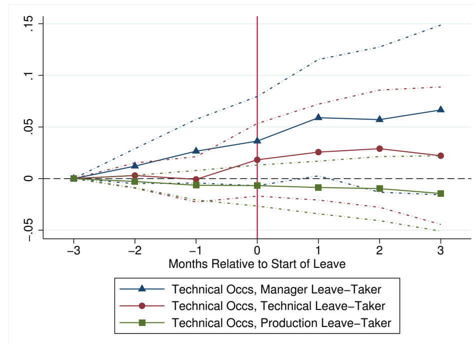
(e) Number of Separations Excluding Leave-Taker

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.

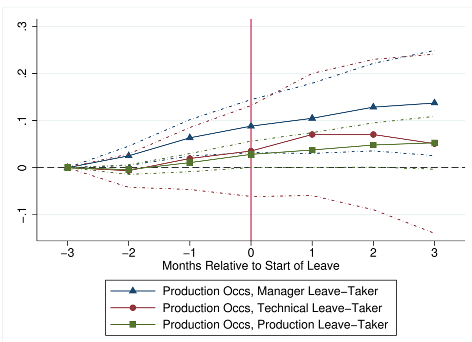
Figure 4: Implied Number of Contracted Employees around Leave Initiation in Non-Leave-Taking Occupations



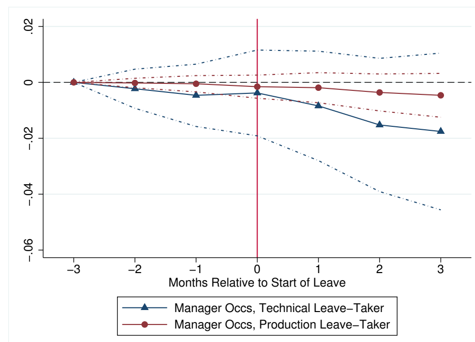
(a) Maternity: Spillovers to Managerial Occupations



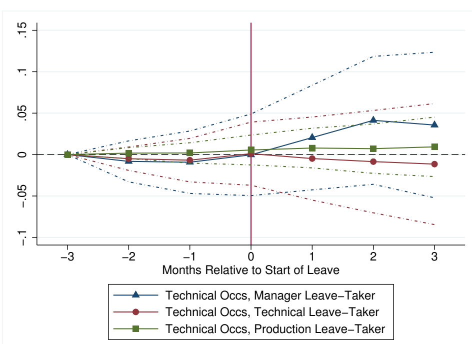
(b) Maternity: Spillovers to Technical Occupations



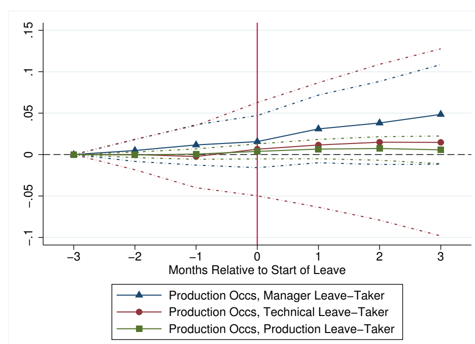
(c) Maternity: Spillovers to Production Occupations



(d) Sickness: Spillovers to Managerial Occupations



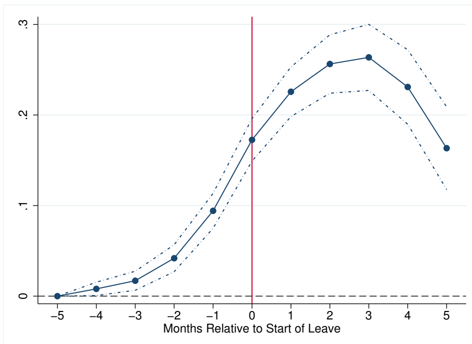
(e) Sickness: Spillovers to Technical Occupations



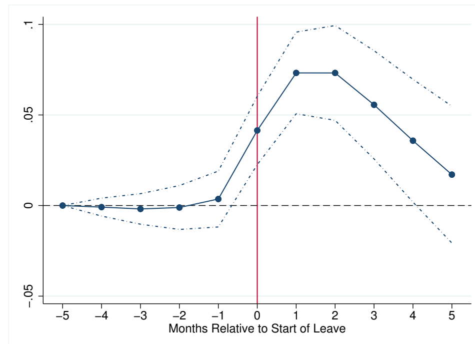
(f) Sickness: Spillovers to Production Occupations

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.

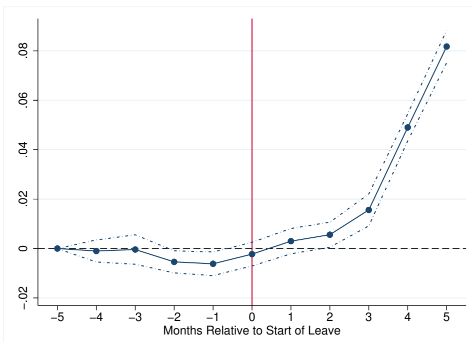
Figure 5: Employment Dynamics around Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window



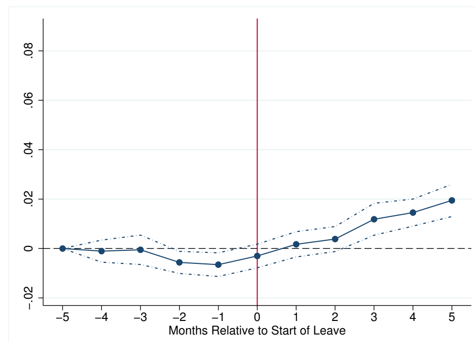
(a) Maternity: Implied Number of Contracted Employees



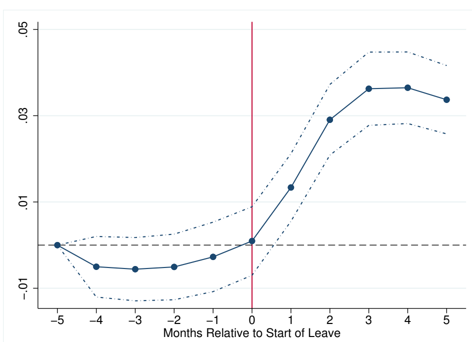
(b) Sickness: Implied Number of Contracted Employees



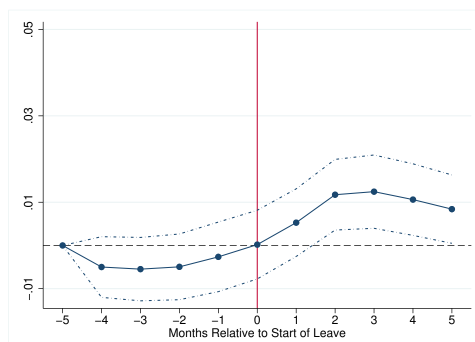
(c) Maternity: Number of Separations



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



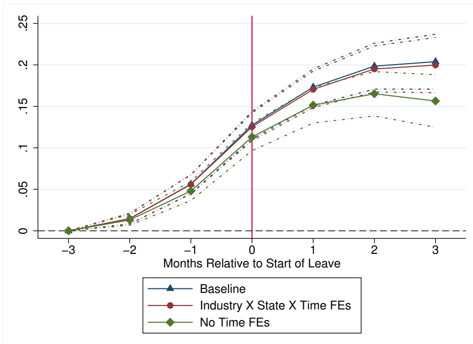
(e) Sickness: Number of Separations



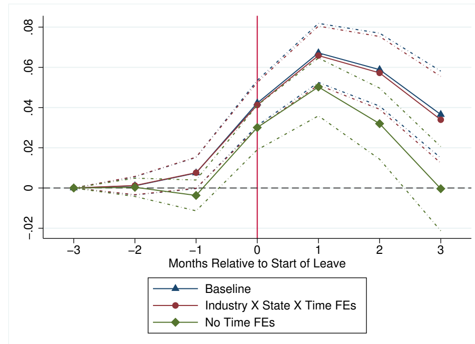
(f) Sickness: Number of Separations Excluding Leave-Taker

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.

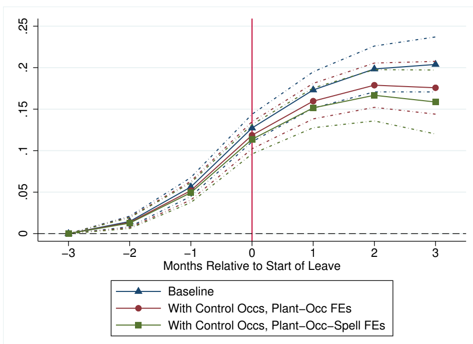
Figure 6: Robustness of Implied Number of Contracted Employees in Occupation of Leave-Taker to Different Modeling Assumptions



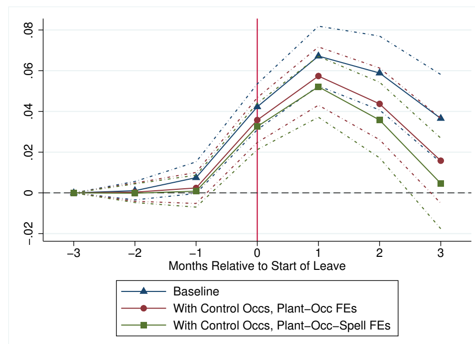
(a) Maternity: Alternate Time Controls



(b) Sickness: Alternate Time Controls



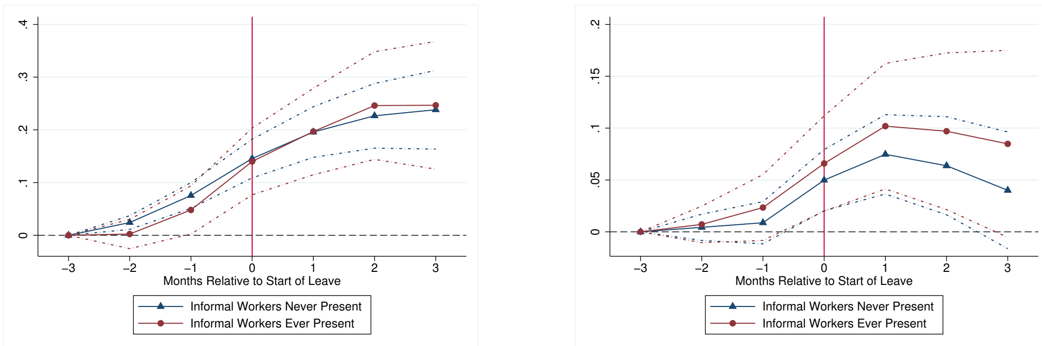
(c) Maternity: With Control Occupations



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

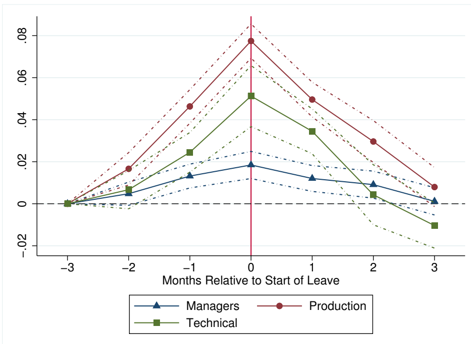


(a) Maternity: By Informal Workers Ever Present Between 2003–2011

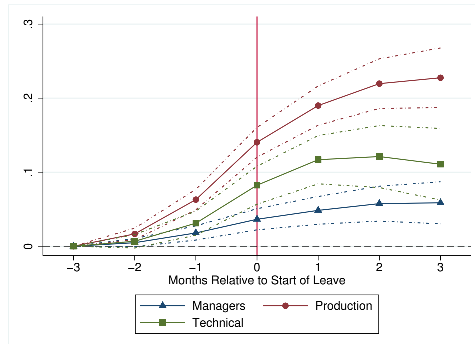
(b) Sickness: By Informal Workers Ever Present Between 2003–2011

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.

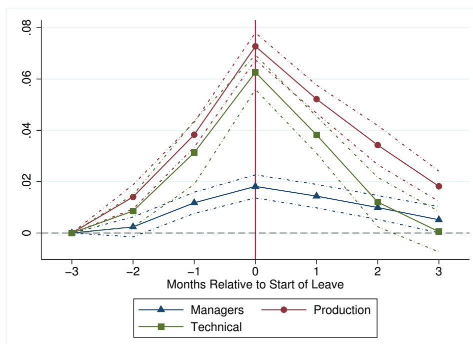
Figure 8: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Coarse Occupation Categories



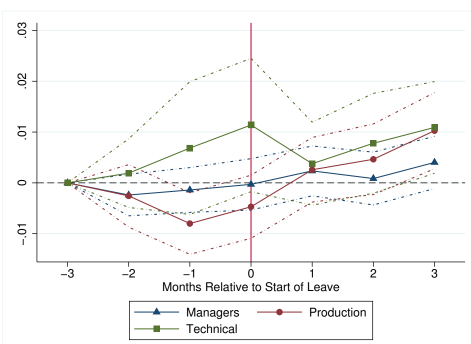
(a) Change in Number of Contracted Employees



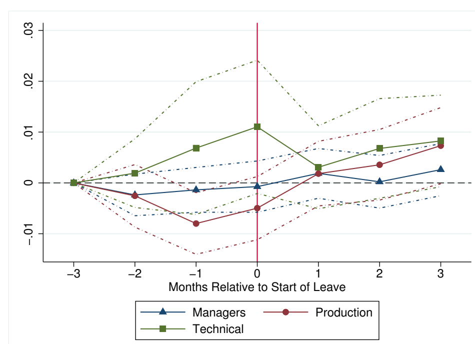
(b) Implied Number of Contracted Employees



(c) Number of Hires



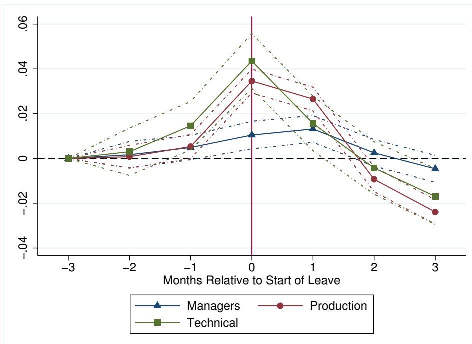
(d) Number of Separations



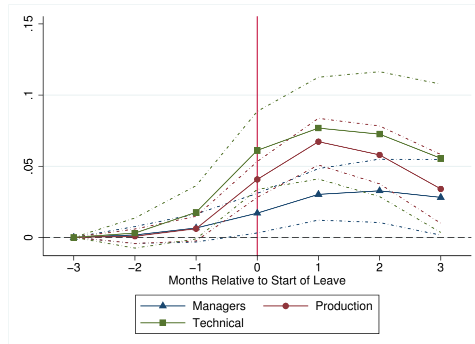
(e) Number of Separations Excluding Leave-Taker

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.

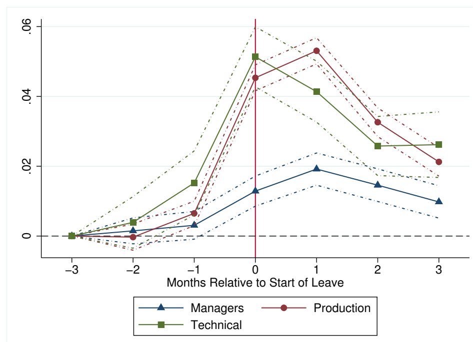
Figure 9: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Coarse Occupation Categories



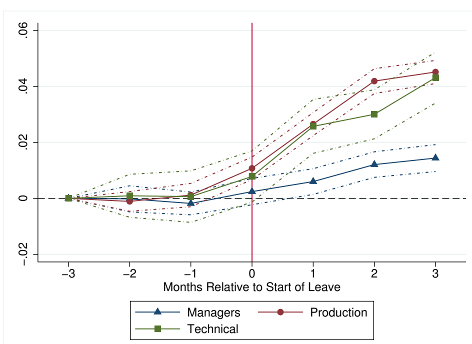
(a) Change in Number of Contracted Employees



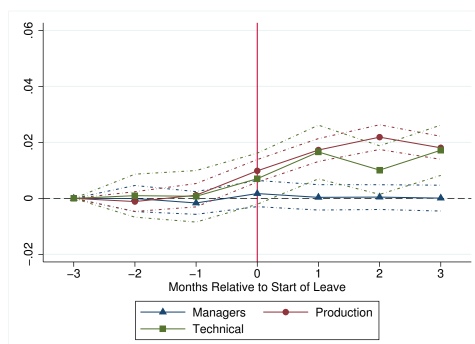
(b) Implied Number of Contracted Employees



(c) Number of Hires



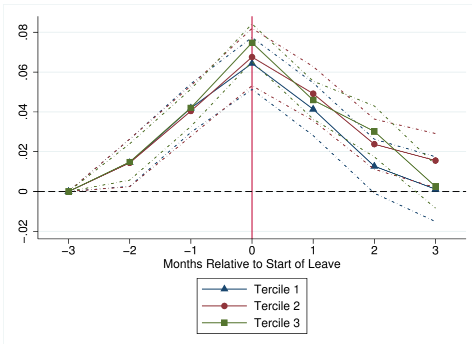
(d) Number of Separations



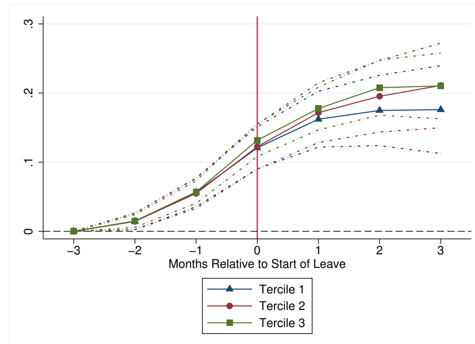
(e) Number of Separations Excluding Leave-Taker

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.

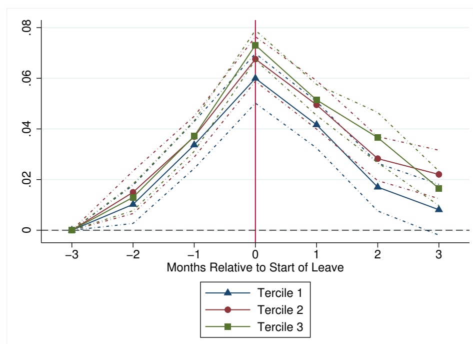
Figure 10: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Market Thickness



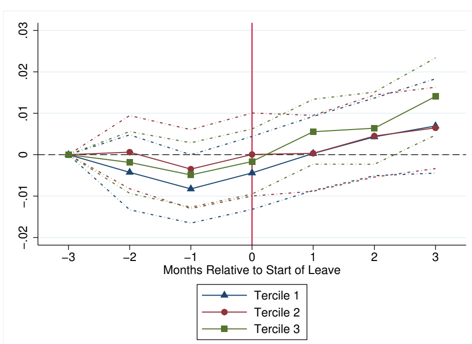
(a) Change in Number of Contracted Employees



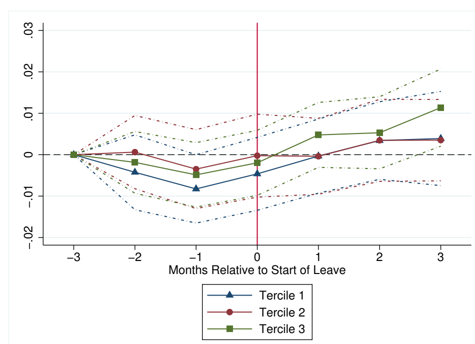
(b) Implied Number of Contracted Employees



(c) Number of Hires



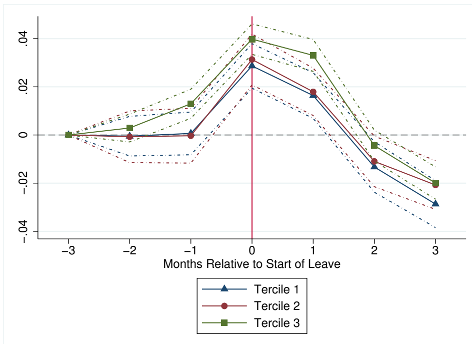
(d) Number of Separations



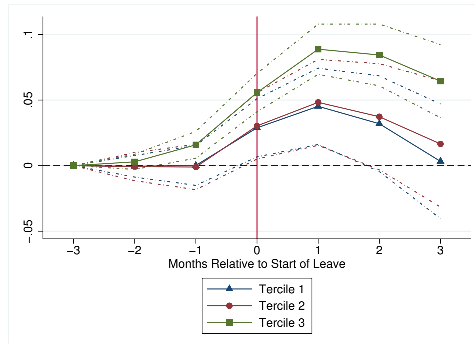
(e) Number of Separations Excluding Leave-Taker

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.

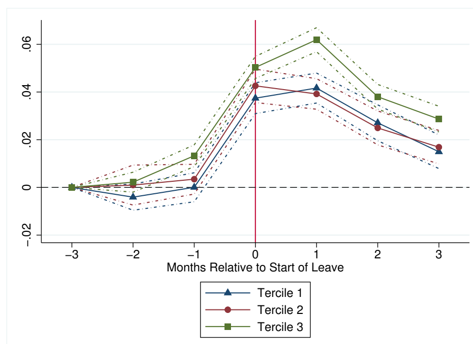
Figure 11: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Market Thickness



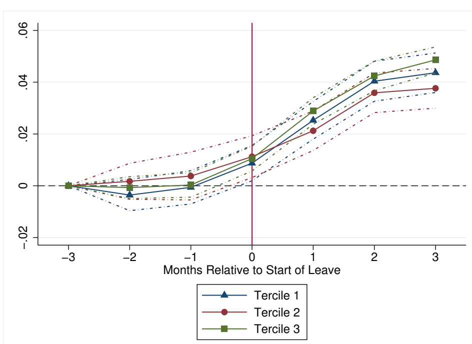
(a) Change in Number of Contracted Employees



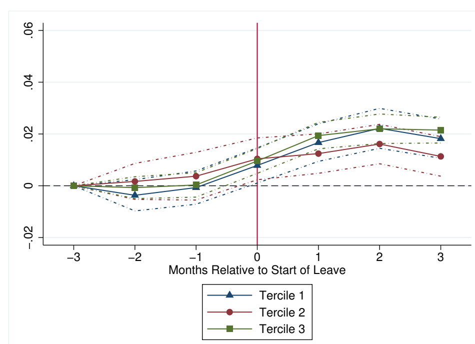
(b) Implied Number of Contracted Employees



(c) Number of Hires



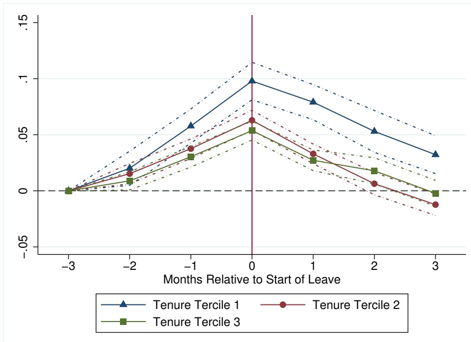
(d) Number of Separations



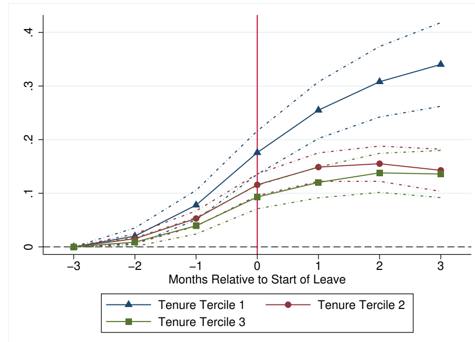
(e) Number of Separations Excluding Leave-Taker

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.

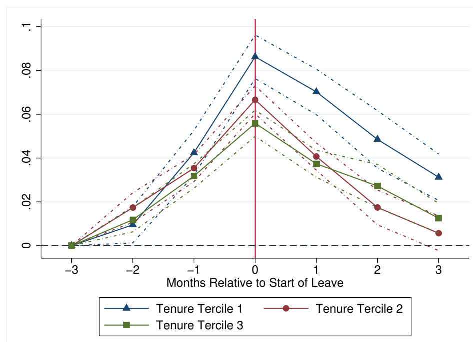
Figure 12: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Leave-Taker Tenure



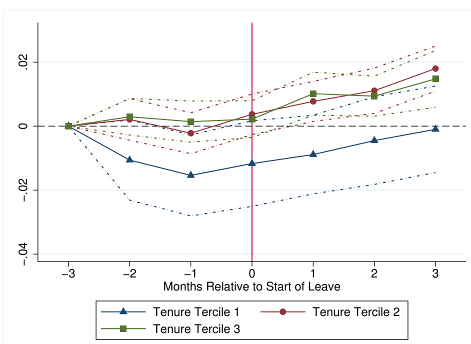
(a) Change in Number of Contracted Employees



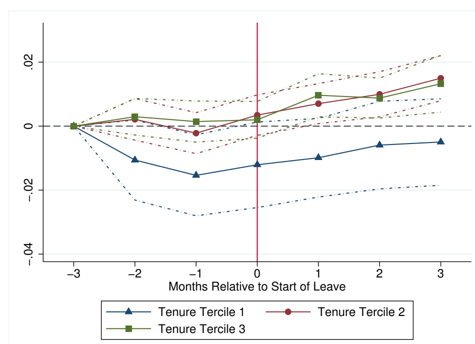
(b) Implied Number of Contracted Employees



(c) Number of Hires



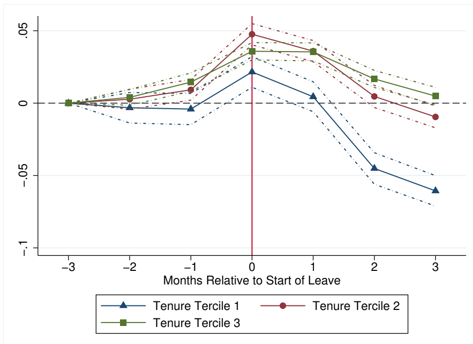
(d) Number of Separations



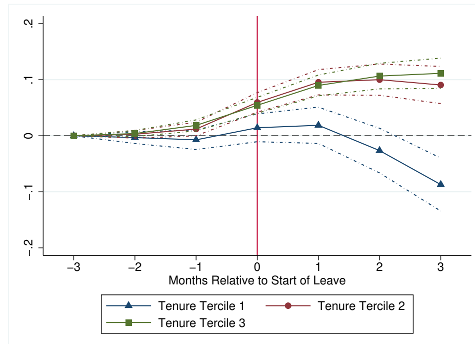
(e) Number of Separations Excluding Leave-Taker

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 13 months or less; workers in the second tercile have between 14 and 29 months of tenure; and, workers in the third tercile have more than 29 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.

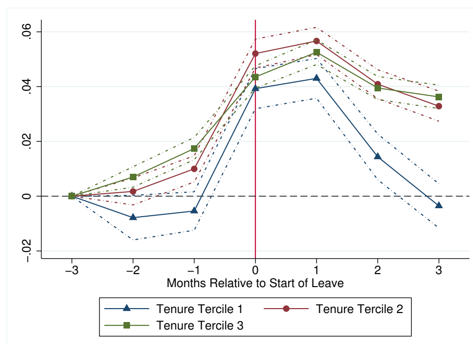
Figure 13: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Leave-Taker Tenure



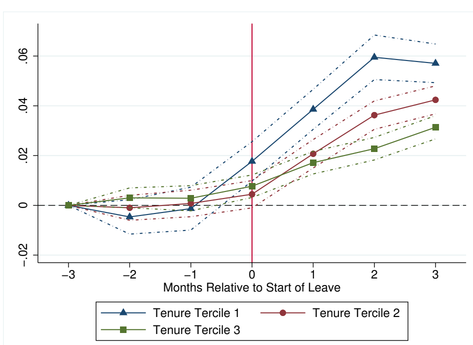
(a) Change in Number of Contracted Employees



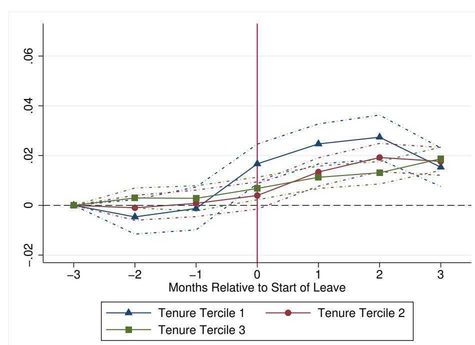
(b) Implied Number of Contracted Employees



(c) Number of Hires



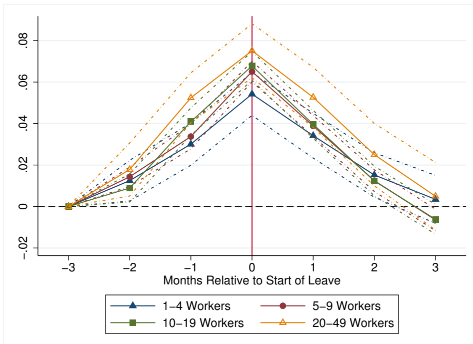
(d) Number of Separations



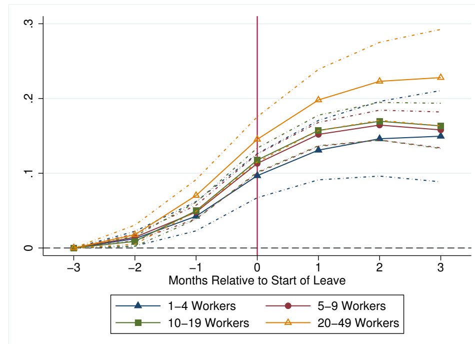
(e) Number of Separations Excluding Leave-Taker

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 15 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.

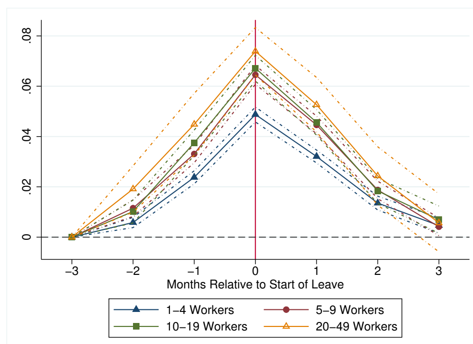
Figure 14: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Plant Size



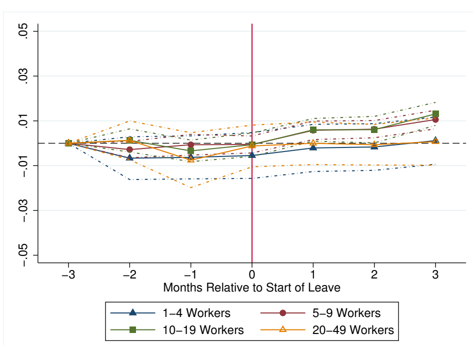
(a) Change in Number of Contracted Employees



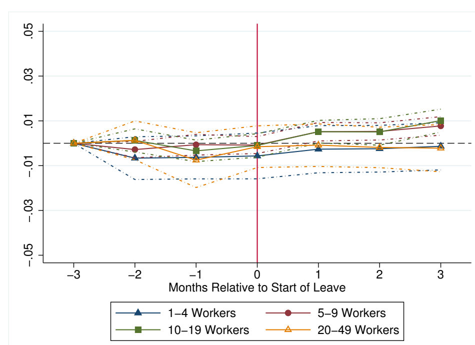
(b) Implied Number of Contracted Employees



(c) Number of Hires



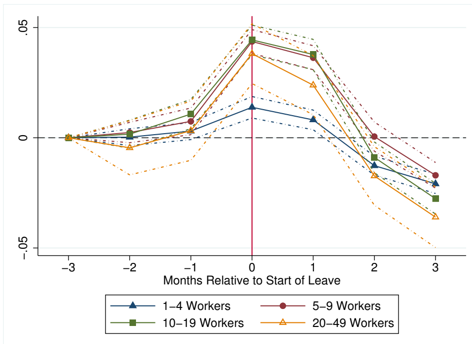
(d) Number of Separations



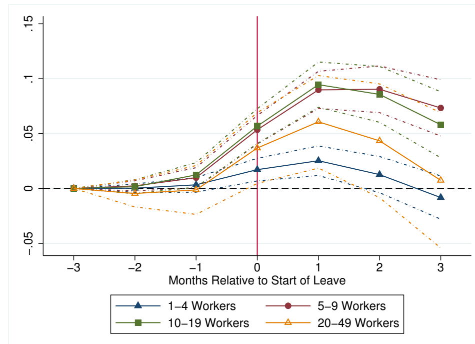
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately for each plant size category. We assign establishments to size categories based on their modal end-of-calendar year size from 2012–2017. We only consider modal plant sizes between 1 and 49 workers, which represent more than 90% of spells. 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.

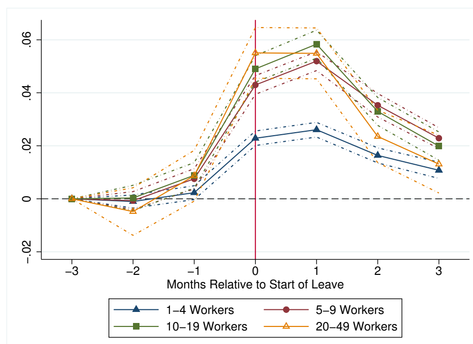
Figure 15: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Plant Size



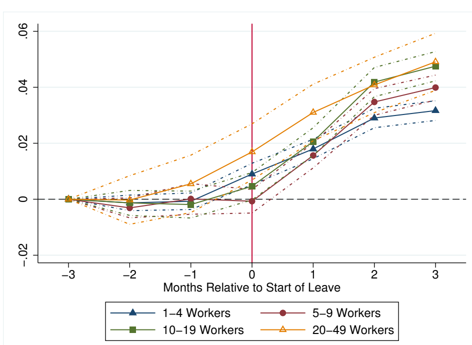
(a) Change in Number of Contracted Employees



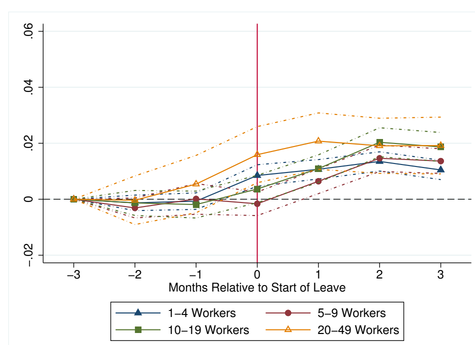
(b) Implied Number of Contracted Employees



(c) Number of Hires



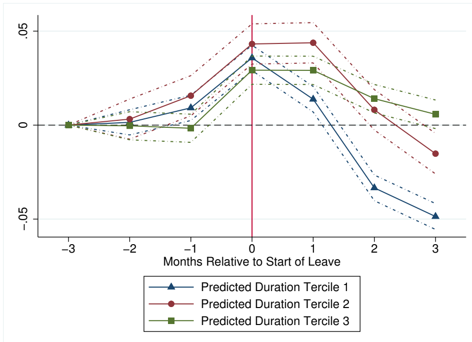
(d) Number of Separations



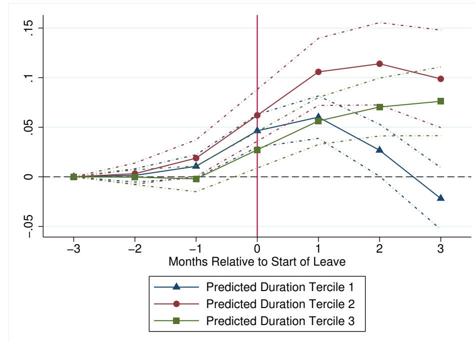
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients and associated 95% confidence intervals from equation 1 estimated separately for each plant size category. We assign establishments to size categories based on their modal end-of-calendar year size from 2012–2017. We only consider modal plant sizes between 1 and 49 workers, which represent more than 90% of spells. 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.

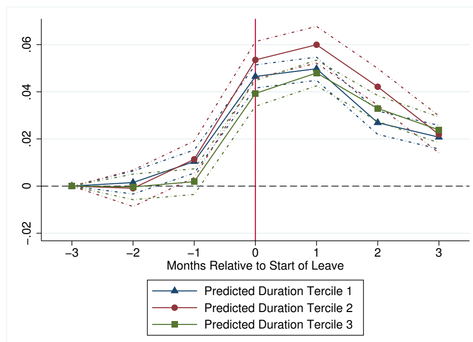
Figure 16: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Predicted Leave Duration



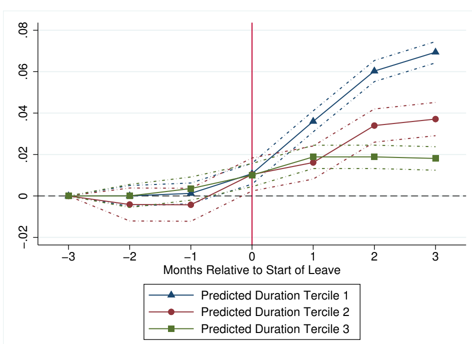
(a) Change in Number of Contracted Employees



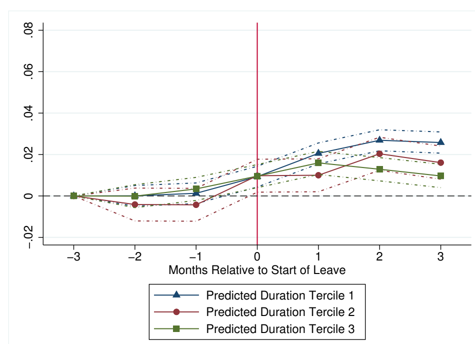
(b) Implied Number of Contracted Employees



(c) Number of Hires



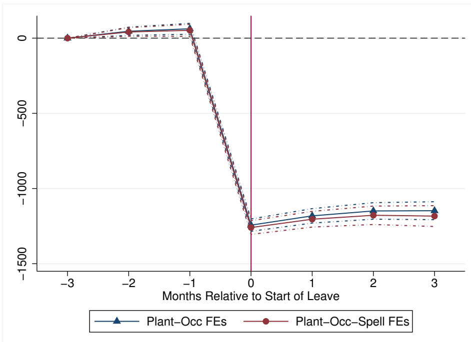
(d) Number of Separations



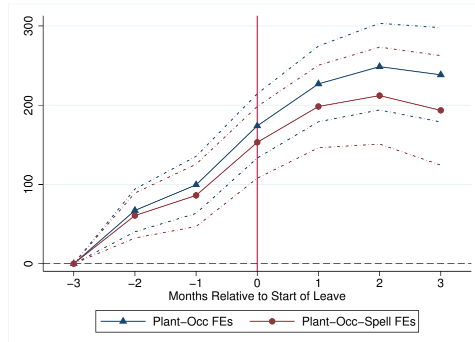
(e) Number of Separations Excluding Leave-Taker

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 predicted leave duration, grouping predicted duration into terciles. Tercile 1 corresponds to 102 days or less; tercile 2 corresponds to between 103 and 153 days; and, tercile 3 corresponds to more than 153 days. 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.

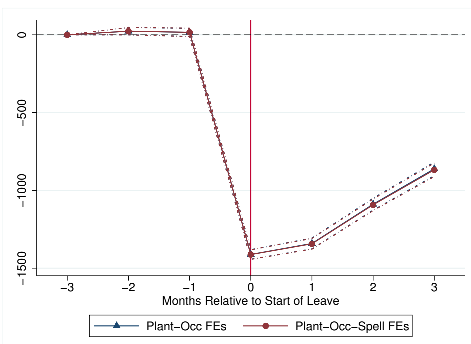
Figure 17: Wage Bill Dynamics around Leave Initiation in Occupation of Leave-Taker Including Control Groups



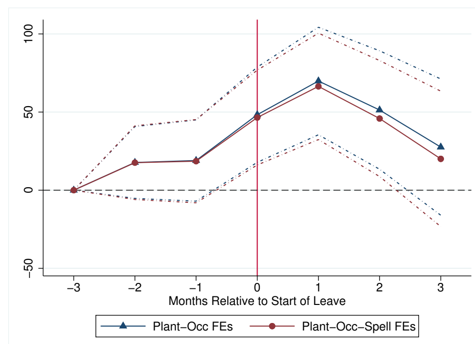
(a) Maternity: Wage Bill Excluding Earnings During Periods of Leave



(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}	0.00109	0.000373	-0.000713	-0.000751
	(0.00230)	(0.00170)	(0.00161)	(0.00161)
β_{-1}	0.00639***	0.00745***	0.00107	0.00104
	(0.00243)	(0.00161)	(0.00190)	(0.00190)
β_0	0.0348***	0.0451***	0.0103***	0.00946***
	(0.00243)	(0.00170)	(0.00184)	(0.00184)
β_1	0.0249***	0.0510***	0.0260***	0.0169***
	(0.00243)	(0.00172)	(0.00188)	(0.00188)
β_2	-0.00833***	0.0319***	0.0402***	0.0205***
	(0.00252)	(0.00187)	(0.00201)	(0.00201)
β_3	-0.0223***	0.0222***	0.0445***	0.0180***
	(0.00249)	(0.00187)	(0.00189)	(0.00188)
R^2	0.109	0.406	0.410	0.411
N	8124102	8124102	8124102	8124102

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	0.084	0.013	0.035	0.031
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	0.114	0.174	0.166	0.168
Utilities: Electric/Gas	0.000	0.001	0.001	0.002	0.001	0.001	0.002	0.002
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	0.034	0.035	0.050	0.048
Wholesale/Retail Trade	0.415	0.400	0.432	0.489	0.347	0.355	0.368	0.444
Transportation/Storage/Mail	0.023	0.027	0.030	0.031	0.054	0.055	0.048	0.037
Accommodation/Food	0.098	0.087	0.099	0.097	0.079	0.071	0.084	0.095
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	0.023	0.023	0.032	0.023
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	0.033	0.031	0.021	0.014
Administrative Activities	0.062	0.061	0.049	0.031	0.086	0.090	0.061	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	0.034	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	0.019
# of Workers on Dec 31								
0	0.027	0.014	0.011	0.008	0.011	0.006	0.006	0.005
1 to 4	0.367	0.233	0.146	0.088	0.371	0.247	0.145	0.093
5 to 9	0.275	0.276	0.221	0.159	0.283	0.286	0.228	0.168
10 to 19	0.200	0.247	0.262	0.246	0.207	0.252	0.286	0.279
20 to 49	0.109	0.176	0.245	0.282	0.110	0.173	0.259	0.316
50 to 99	0.018	0.042	0.079	0.125	0.015	0.032	0.064	0.107
100 to 249	0.003	0.011	0.030	0.071	0.002	0.004	0.013	0.029
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	670150	59119	277527	153790

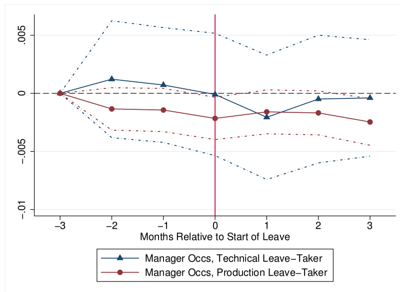
Note: We categorize maternity and sickness leave spells by the inspection status of the associated plant and provide plant-level characteristics associated with each spell. In columns (1) and (5), spells are associated with plants that were never inspected from 2003–2011. In columns (2) and (6), spells 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

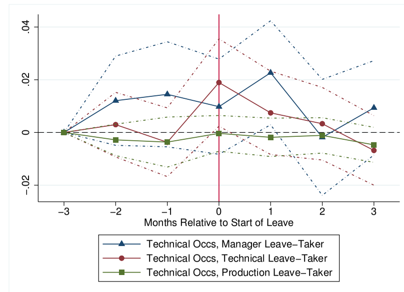
	Maternity		Sickness	
	Own Plant-Occ	Control Plant-Occ	Own Plant-Occ	Control Plant-Occ
Wage Bill	15210.738	13865.463	15153.531	9561.760
	(54155.618)	(55848.095)	(42526.073)	(30311.909)
Wage Bill Excluding Leave-Taker Earnings	13797.404		13694.492	
	(53784.036)		(42136.932)	
# of Plant-Occupations	501607	857263	513478	828738

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.

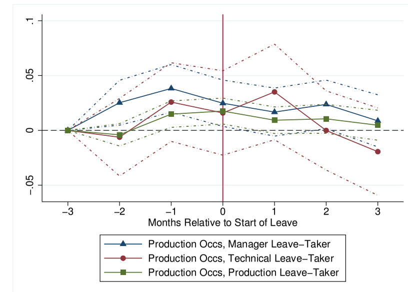
Figure A1: Employment Dynamics around Maternity Leave Initiation in Non-Leave-Taking Occupations



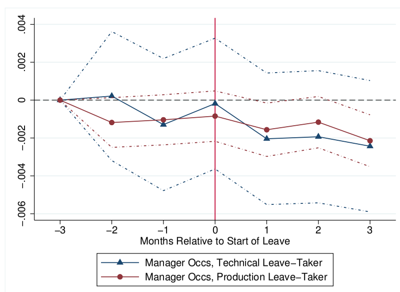
(a) Change in Number of Contracted Employees



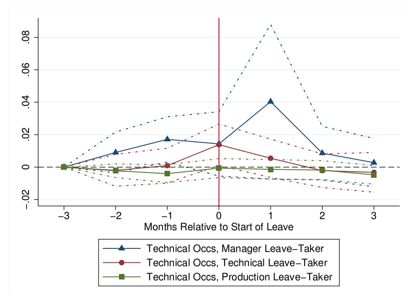
(b) Change in Number of Contracted Employees



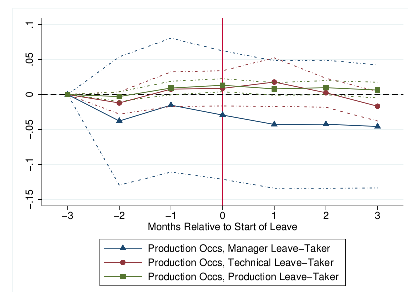
(c) Change in Number of Contracted Employees



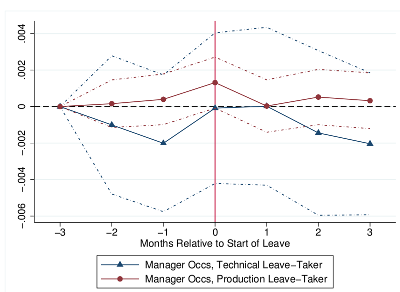
(d) Number of Hires



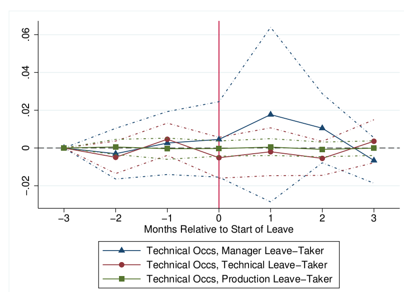
(e) Number of Hires



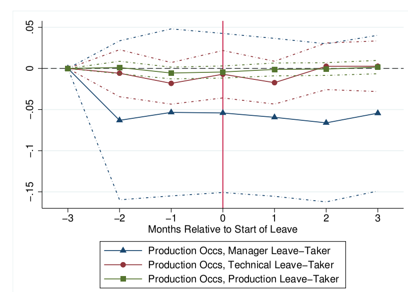
(f) Number of Hires



(g) Number of Separations



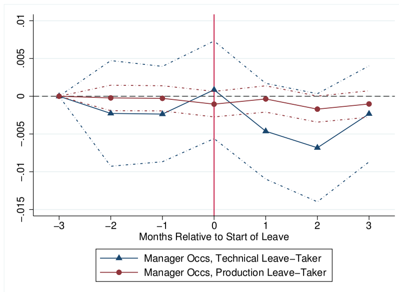
(h) Number of Separations



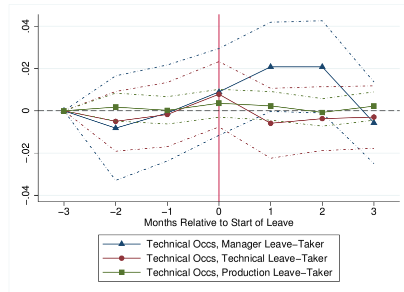
(i) Number of Separations

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.

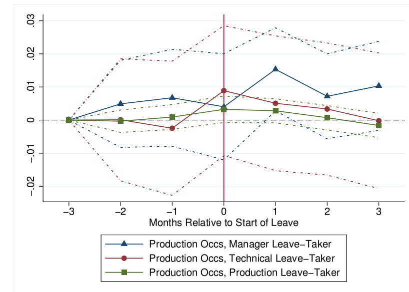
Figure A2: Employment Dynamics around Sickness Leave Initiation in Non-Leave-Taking Occupations



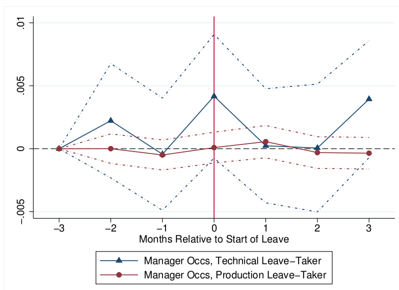
(a) Change in Number of Contracted Employees



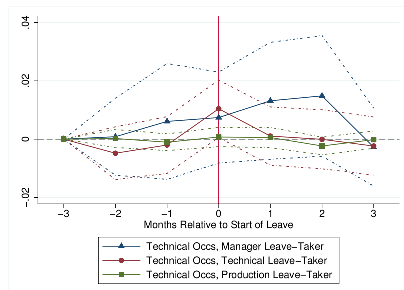
(b) Change in Number of Contracted Employees



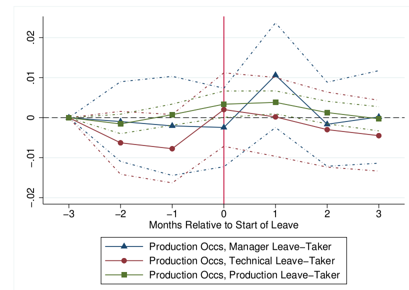
(c) Change in Number of Contracted Employees



(d) Number of Hires

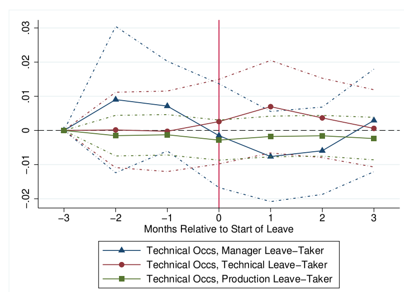


(e) Number of Hires

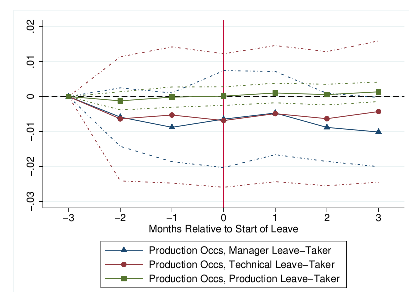


(f) Number of Hires

(g) Number of Separations



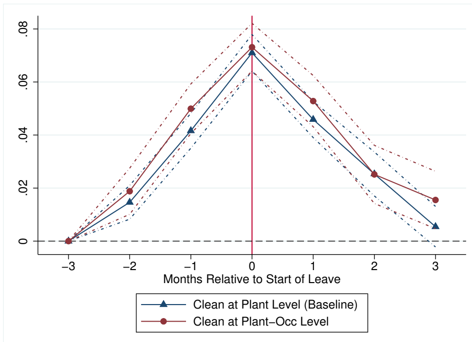
(h) Number of Separations



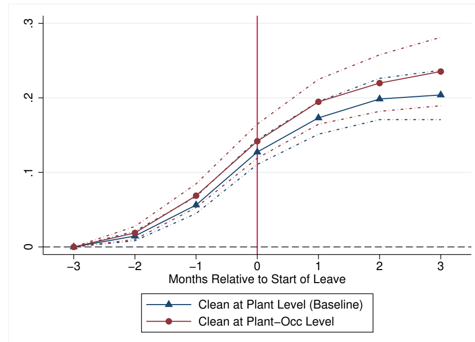
(i) Number of Separations

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.

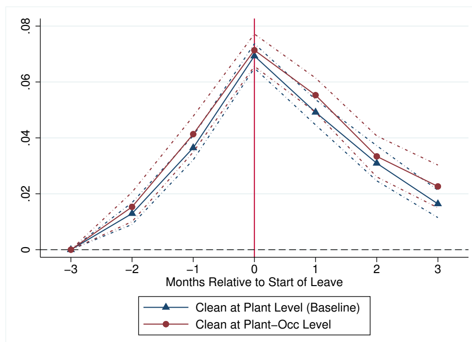
Figure A3: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker Using a Relaxed Clean-Spell Definition



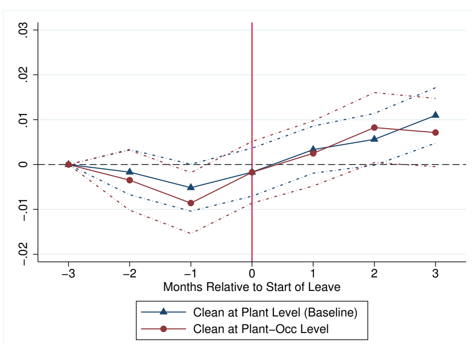
(a) Change in Number of Contracted Employees



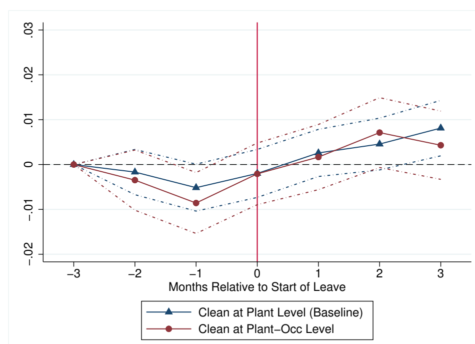
(b) Implied Number of Contracted Employees



(c) Number of Hires



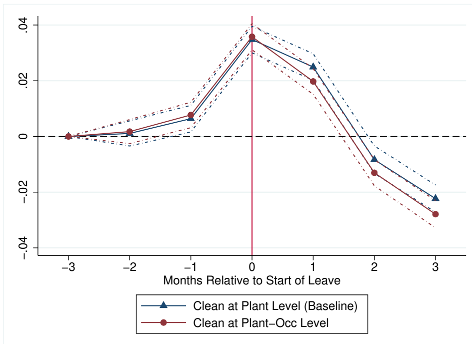
(d) Number of Separations



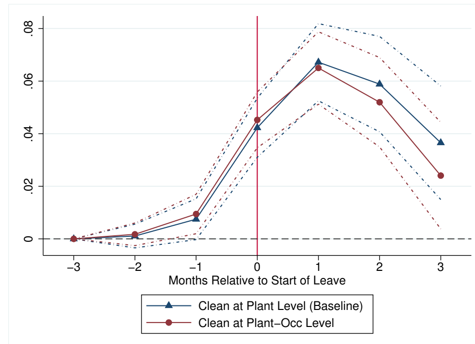
(e) Number of Separations Excluding Leave-Taker

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.

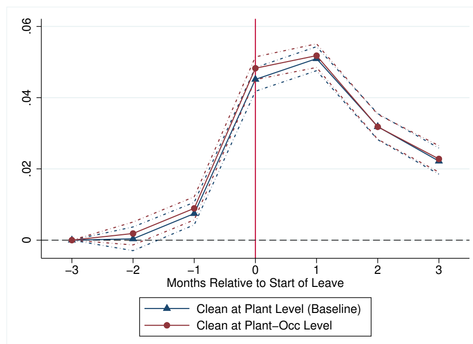
Figure A4: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Using a Relaxed Clean-Spell Definition



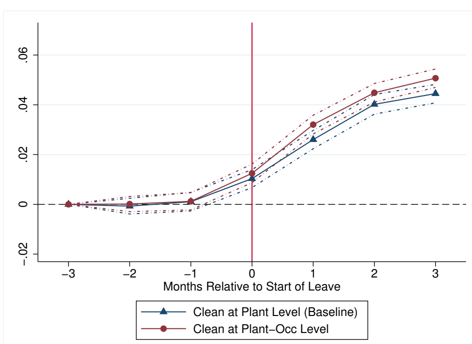
(a) Change in Number of Contracted Employees



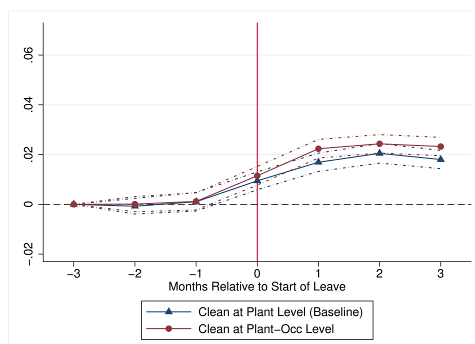
(b) Implied Number of Contracted Employees



(c) Number of Hires



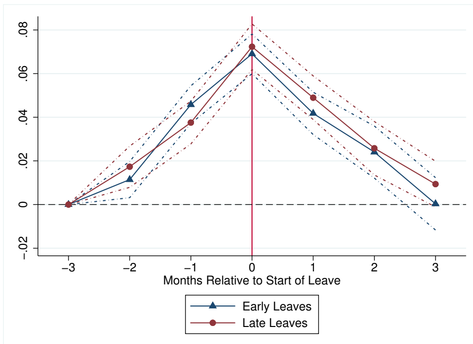
(d) Number of Separations



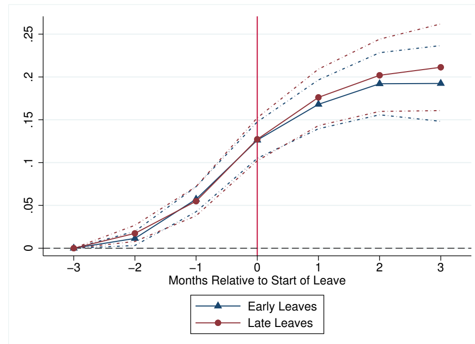
(e) Number of Separations Excluding Leave-Taker

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.

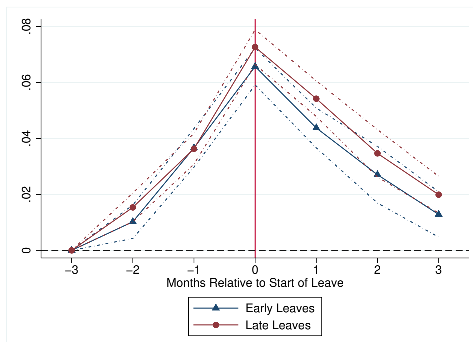
Figure A5: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker in Early and Later Years



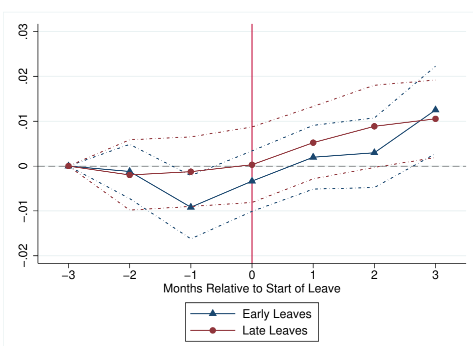
(a) Change in Number of Contracted Employees



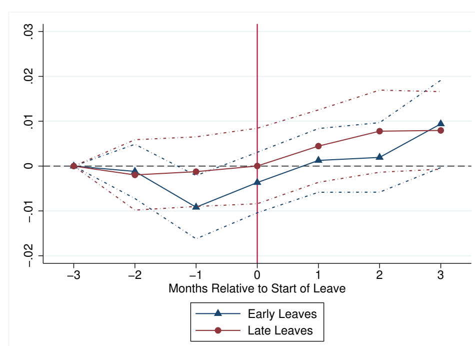
(b) Implied Number of Contracted Employees



(c) Number of Hires



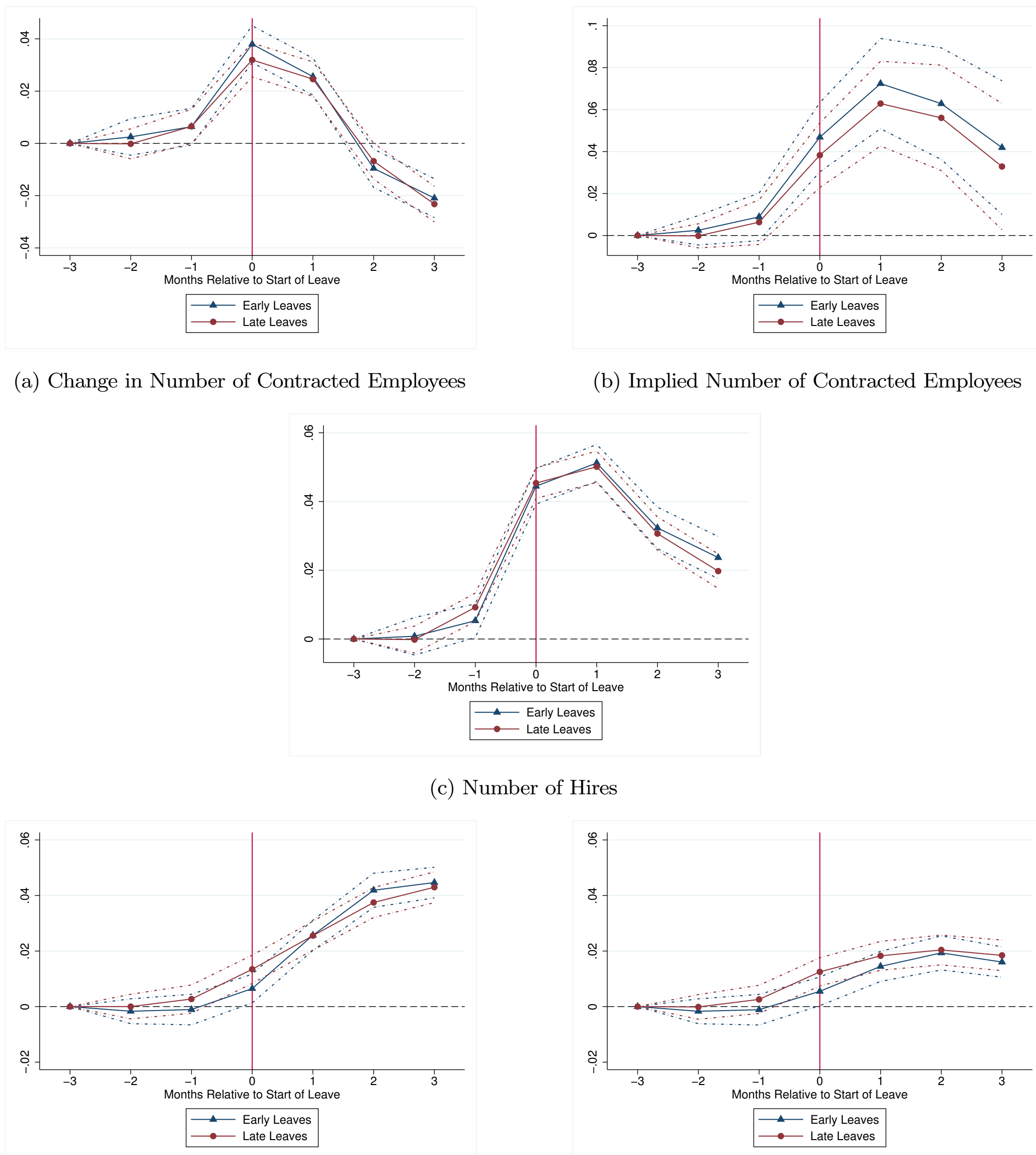
(d) Number of Separations



(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 A6: 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

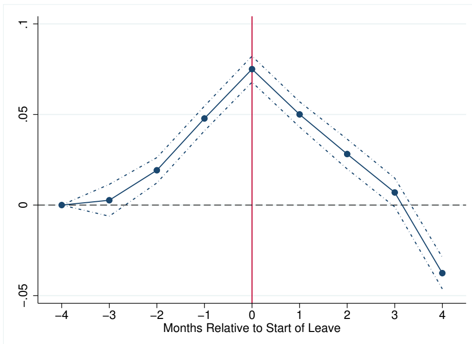
(c) Number of Hires

(d) Number of Separations

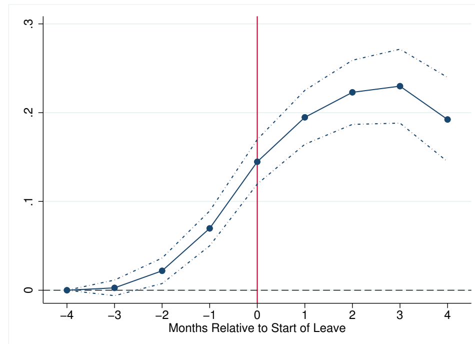
(e) Number of Separations Excluding Leave-Taker

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.

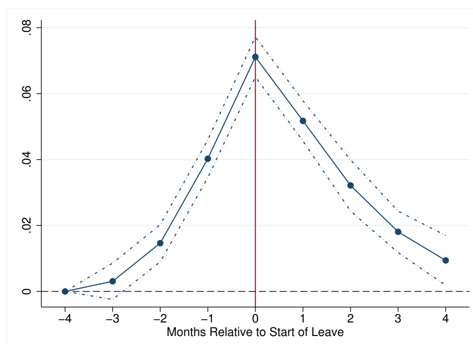
Figure A7: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 4-Month Event Window



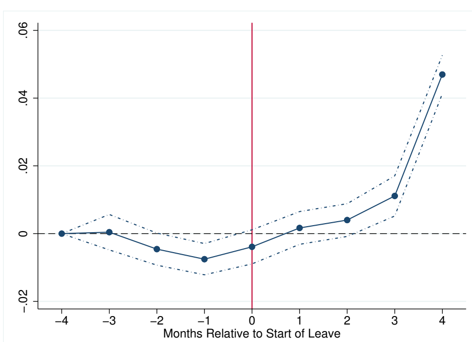
(a) Change in Number of Contracted Employees



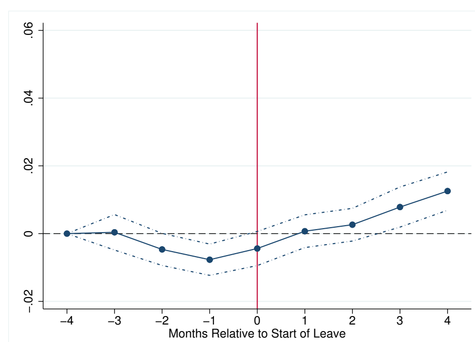
(b) Implied Number of Contracted Employees



(c) Number of Hires



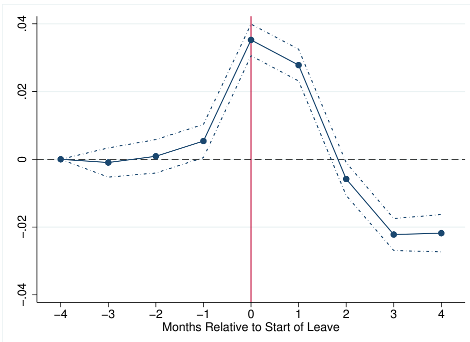
(d) Number of Separations



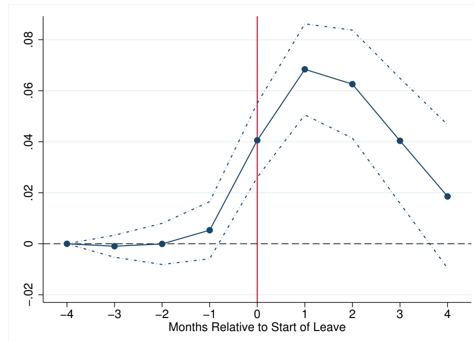
(e) Number of Separations Excluding Leave-Taker

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.

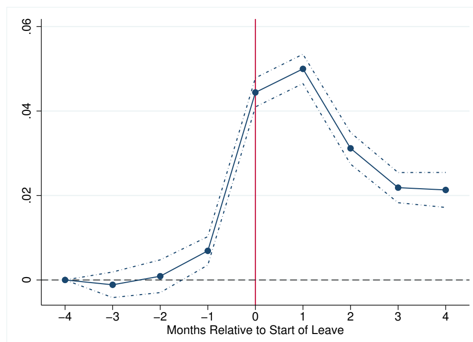
Figure A8: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 4-Month Event Window



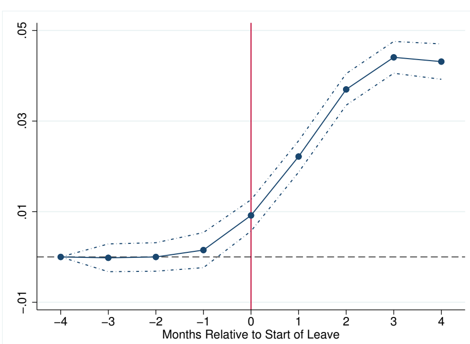
(a) Change in Number of Contracted Employees



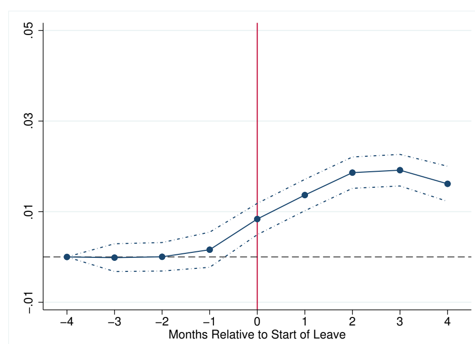
(b) Implied Number of Contracted Employees



(c) Number of Hires



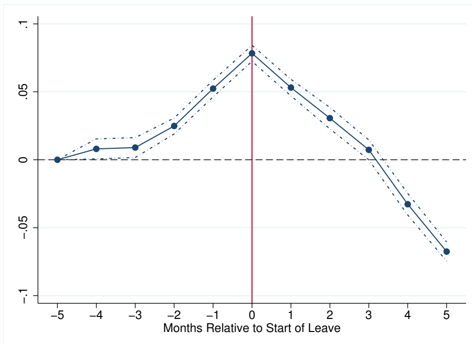
(d) Number of Separations



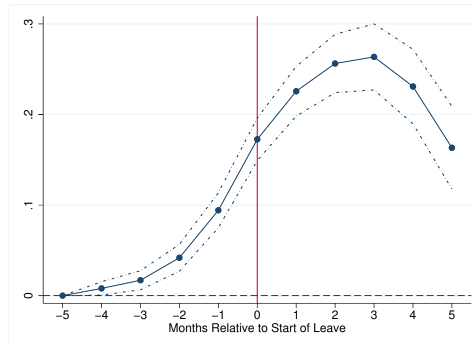
(e) Number of Separations Excluding Leave-Taker

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.

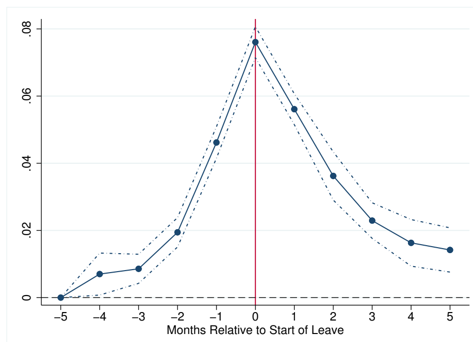
Figure A9: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window



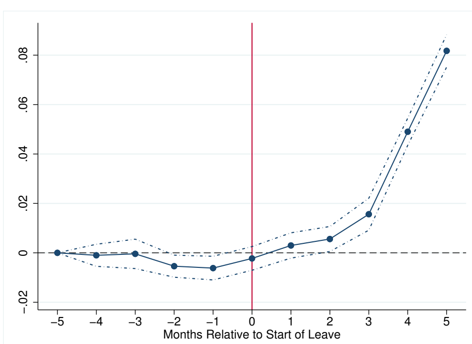
(a) Change in Number of Contracted Employees



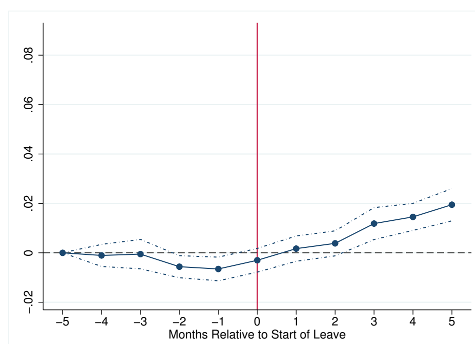
(b) Implied Number of Contracted Employees



(c) Number of Hires



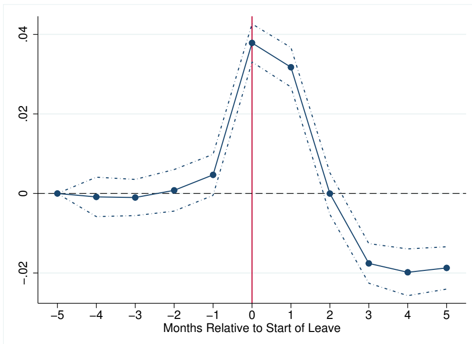
(d) Number of Separations



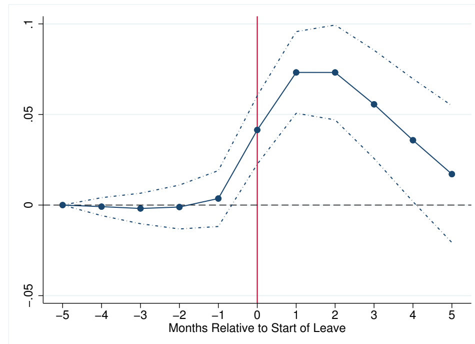
(e) Number of Separations Excluding Leave-Taker

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.

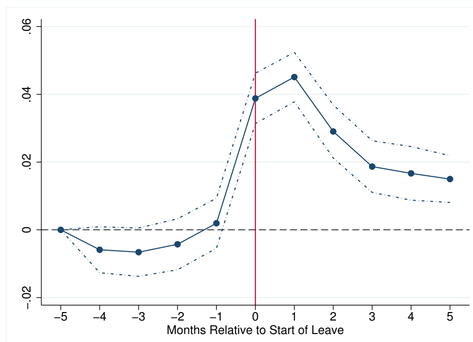
Figure A10: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window



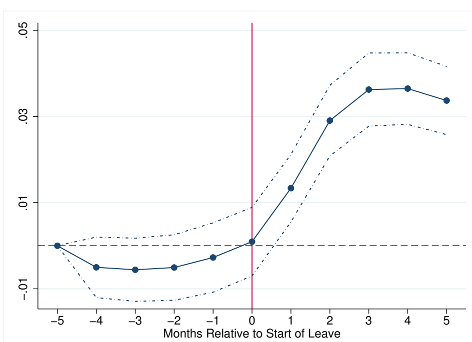
(a) Change in Number of Contracted Employees



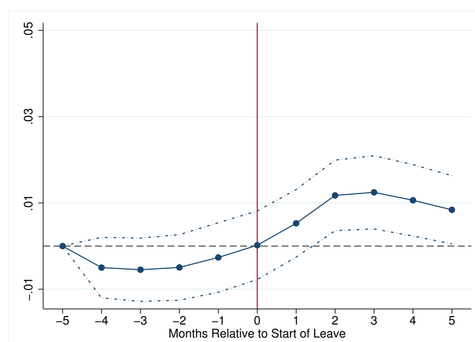
(b) Implied Number of Contracted Employees



(c) Number of Hires



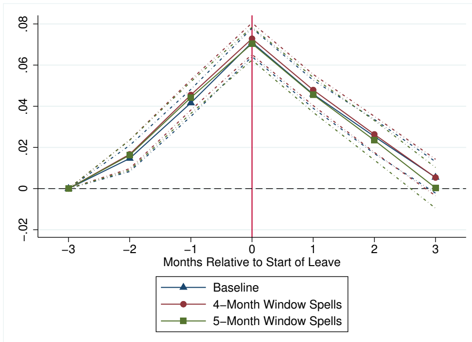
(d) Number of Separations



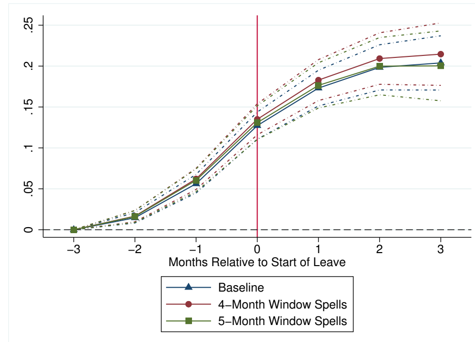
(e) Number of Separations Excluding Leave-Taker

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.

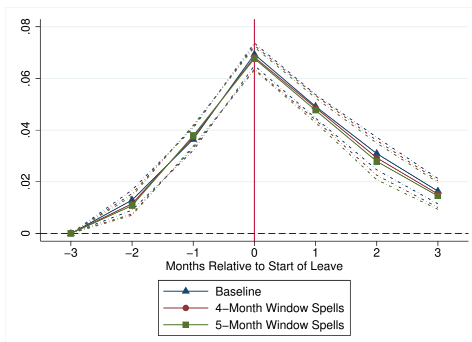
Figure A11: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 3-Month Event Window Using 4-Month and 5-Month Window Spells



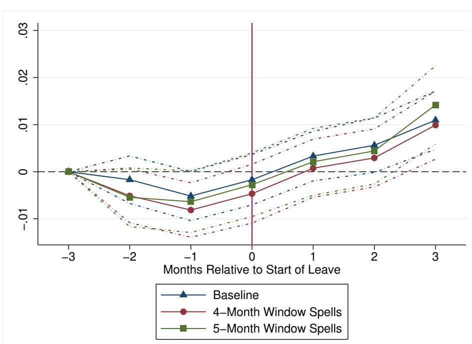
(a) Change in Number of Contracted Employees



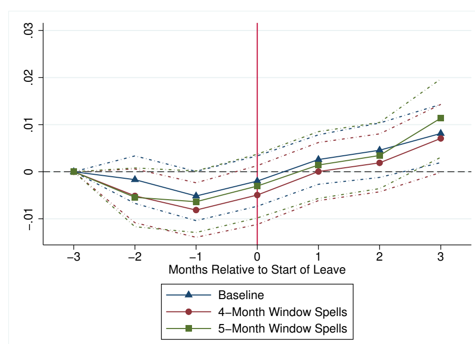
(b) Implied Number of Contracted Employees



(c) Number of Hires



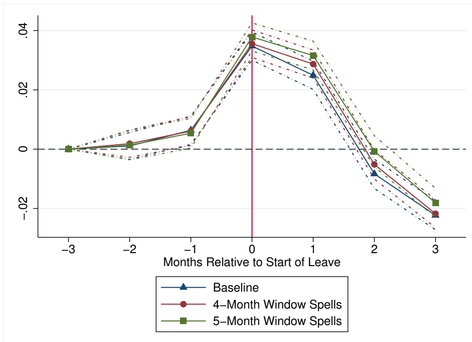
(d) Number of Separations



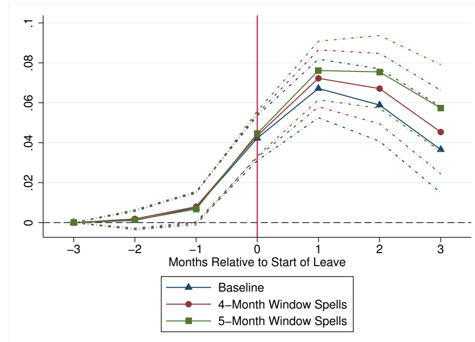
(e) Number of Separations Excluding Leave-Taker

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.

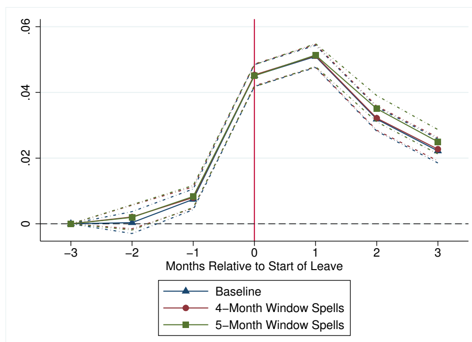
Figure A12: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 3-Month Event Window Using 4-Month and 5-Month Window Spells



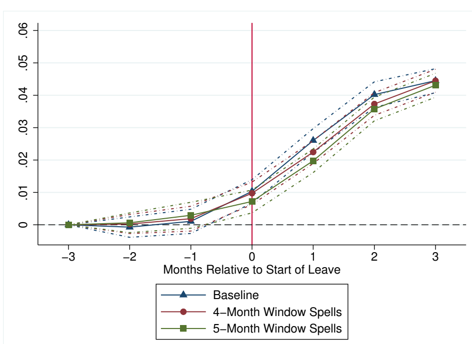
(a) Change in Number of Contracted Employees



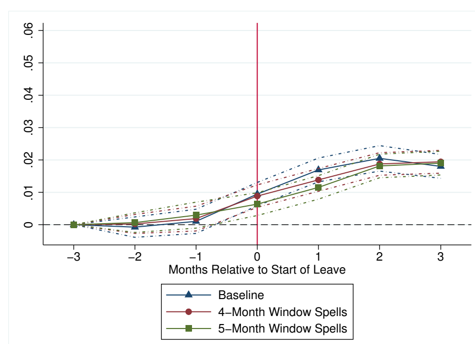
(b) Implied Number of Contracted Employees



(c) Number of Hires



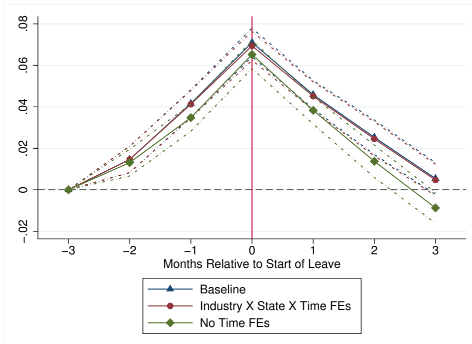
(d) Number of Separations



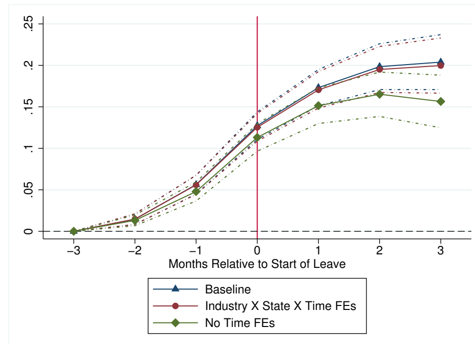
(e) Number of Separations Excluding Leave-Taker

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.

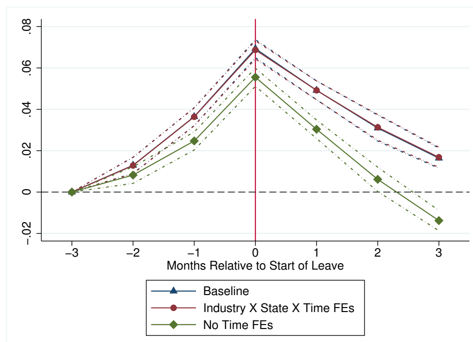
Figure A13: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with Different Calendar Time Controls



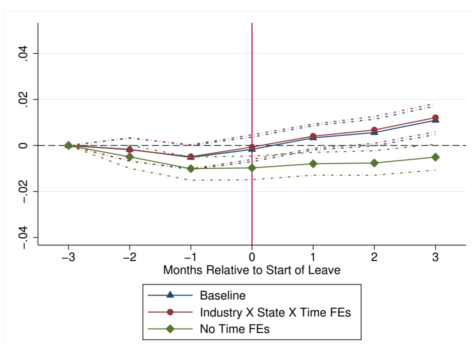
(a) Change in Number of Contracted Employees



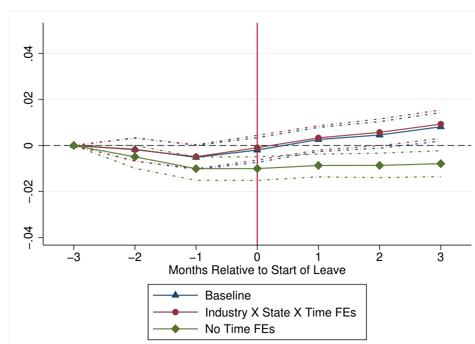
(b) Implied Number of Contracted Employees



(c) Number of Hires



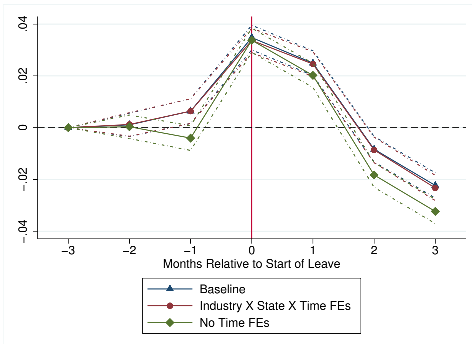
(d) Number of Separations



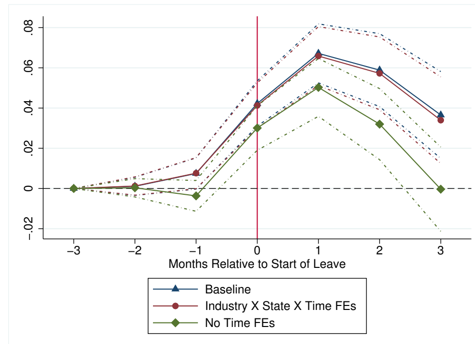
(e) Number of Separations Excluding Leave-Taker

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-state-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.

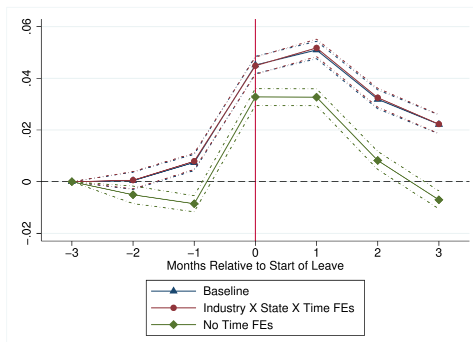
Figure A14: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with Different Calendar Time Controls



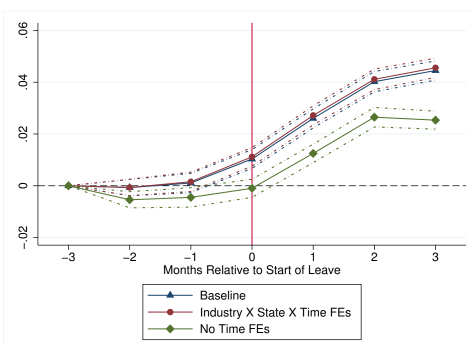
(a) Change in Number of Contracted Employees



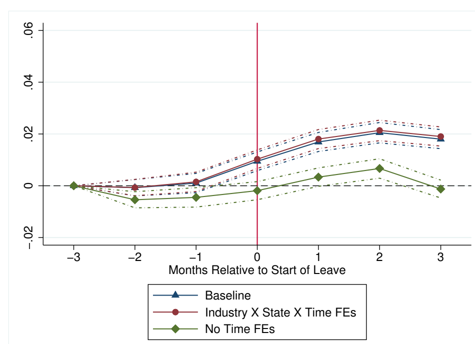
(b) Implied Number of Contracted Employees



(c) Number of Hires



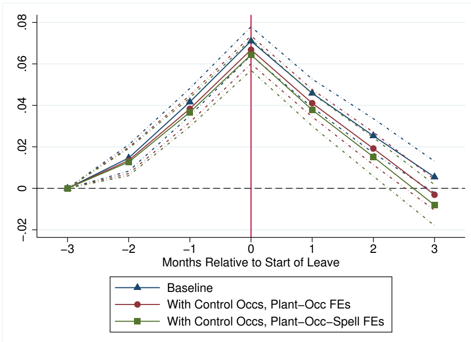
(d) Number of Separations



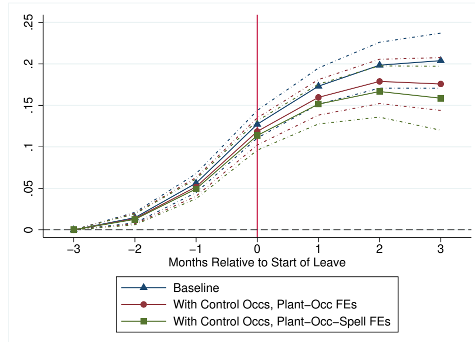
(e) Number of Separations Excluding Leave-Taker

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-state-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.

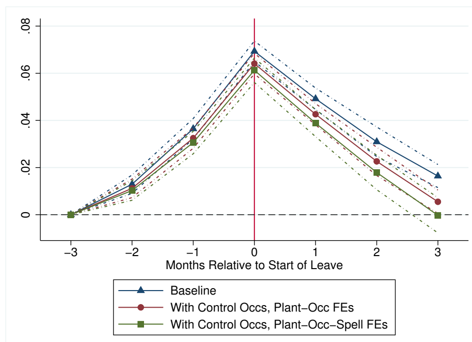
Figure A15: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker Including Control Groups



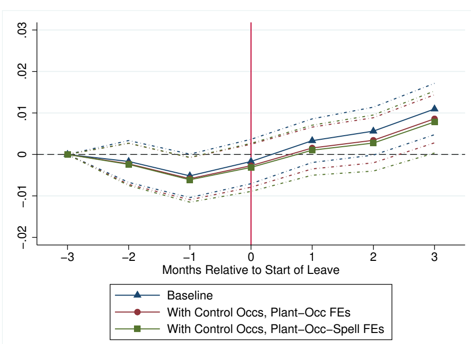
(a) Change in Number of Contracted Employees



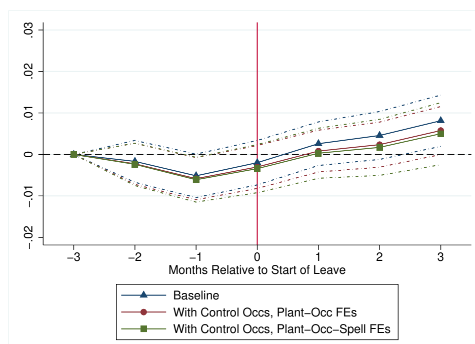
(b) Implied Number of Contracted Employees



(c) Number of Hires



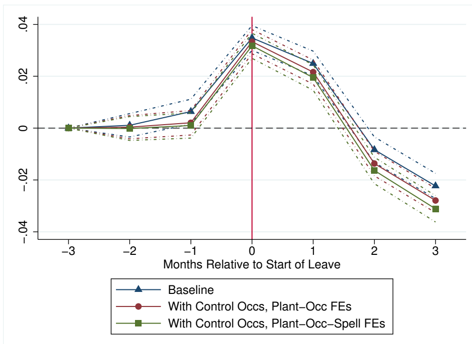
(d) Number of Separations



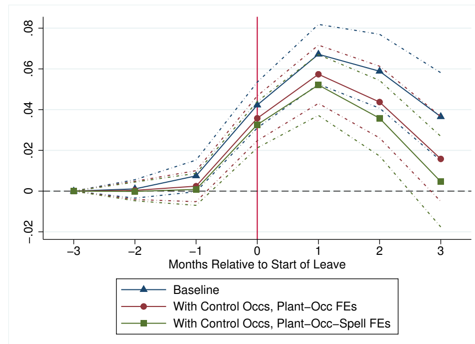
(e) Number of Separations Excluding Leave-Taker

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.

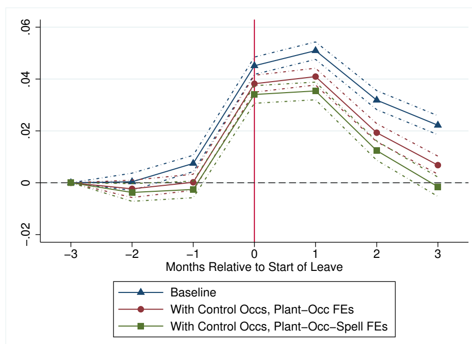
Figure A16: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Including Control Groups



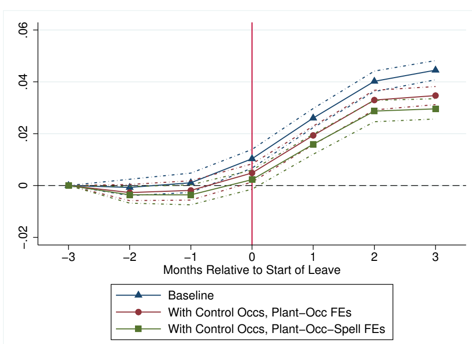
(a) Change in Number of Contracted Employees



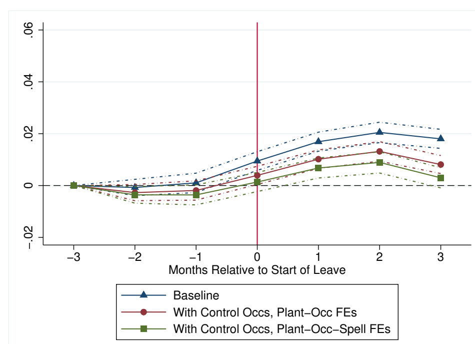
(b) Implied Number of Contracted Employees



(c) Number of Hires



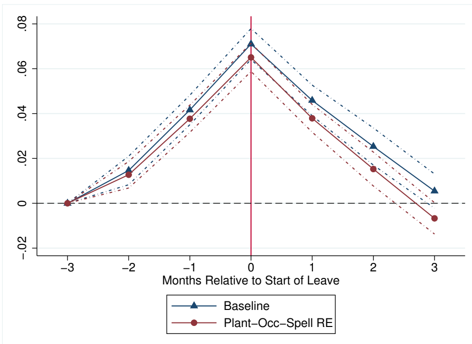
(d) Number of Separations



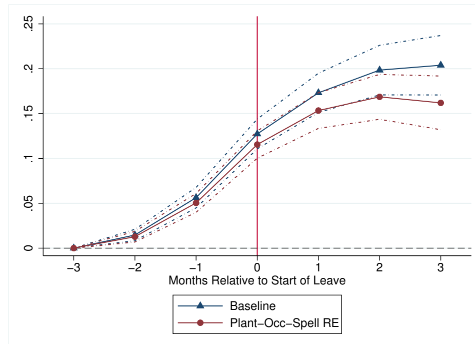
(e) Number of Separations Excluding Leave-Taker

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.

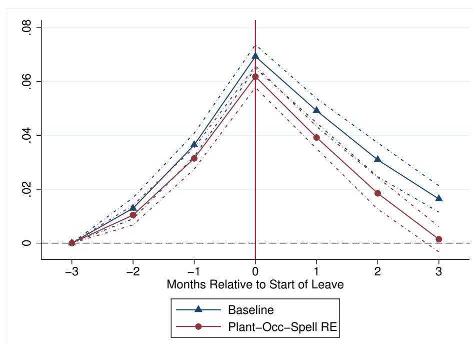
Figure A17: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker Including Plant-Occupation-Spell Random Effects



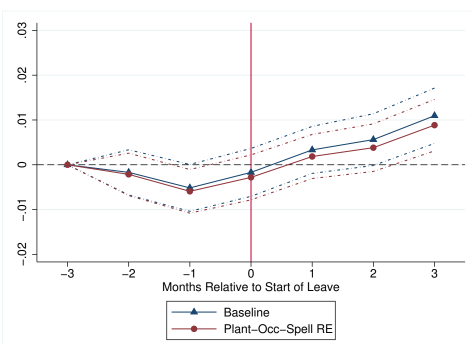
(a) Change in Number of Contracted Employees



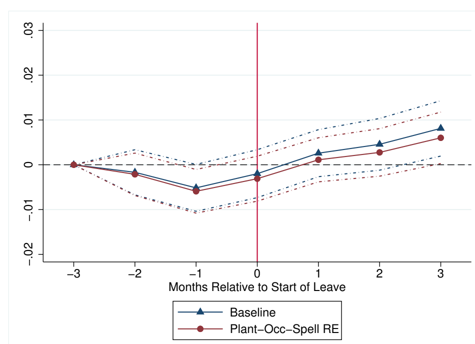
(b) Implied Number of Contracted Employees



(c) Number of Hires



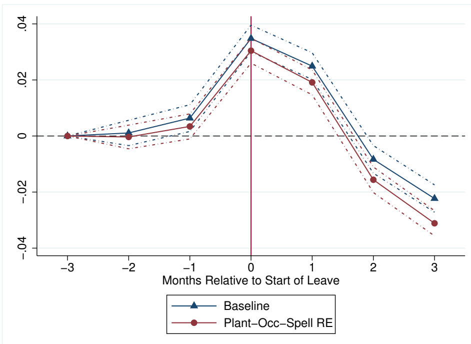
(d) Number of Separations



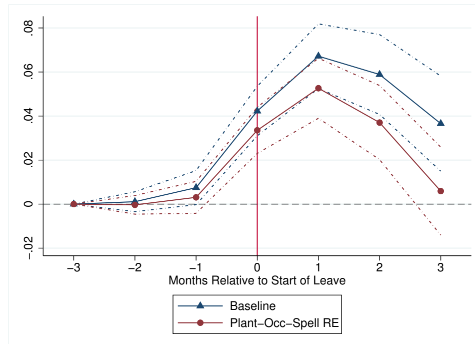
(e) Number of Separations Excluding Leave-Taker

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.

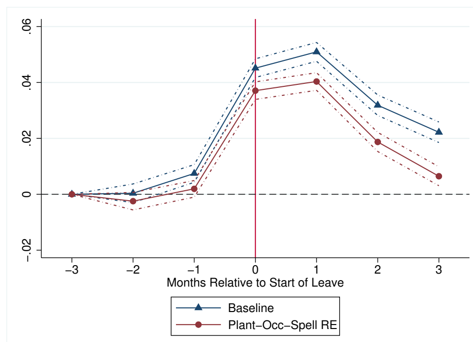
Figure A18: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Including Plant-Occupation-Spell Random Effects



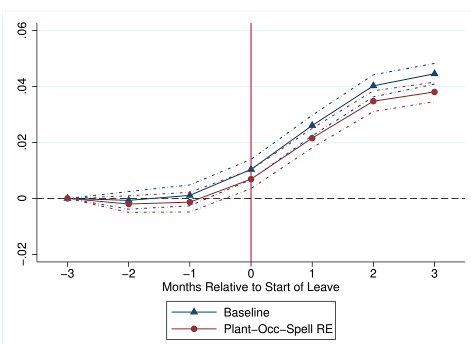
(a) Change in Number of Contracted Employees



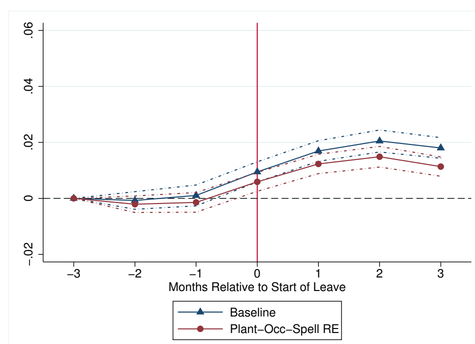
(b) Implied Number of Contracted Employees



(c) Number of Hires



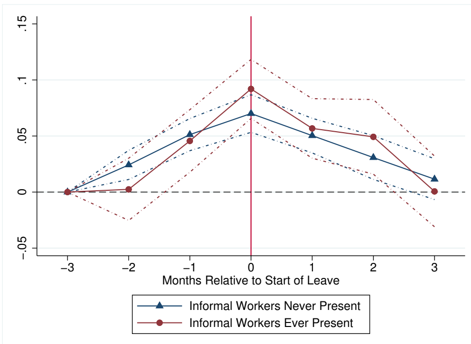
(d) Number of Separations



(e) Number of Separations Excluding Leave-Taker

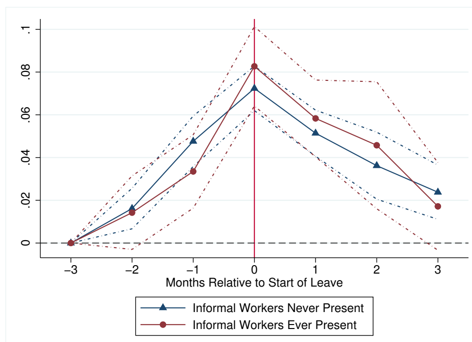
Note: The panels display regression coefficients and associated 95% confidence intervals from a modified version of equation 1 where plant-occupation fixed effects are replaced with plant-occupation-spell random effects. Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence 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 A19: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Inspection Status from 2003–2011

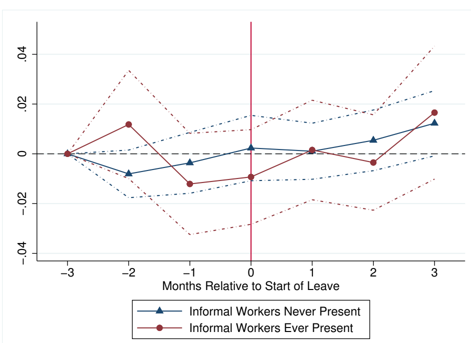


(a) Change in Number of Contracted Employees

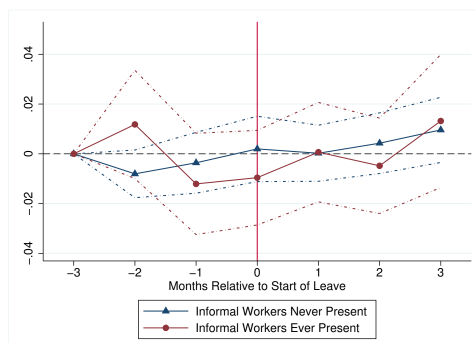
(b) Implied Number of Contracted Employees



(c) Number of Hires



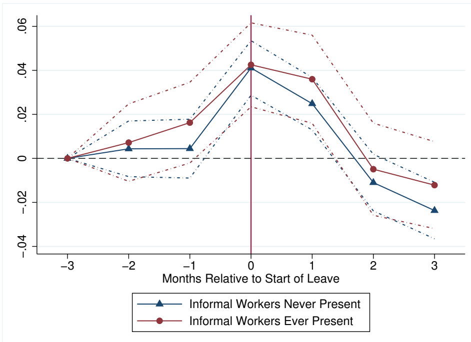
(d) Number of Separations



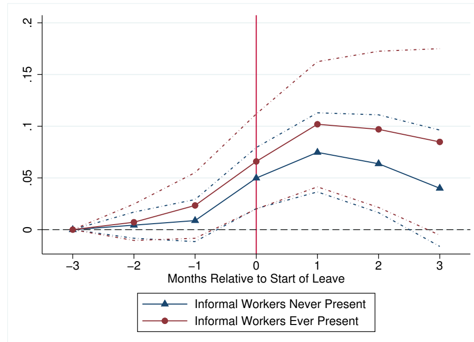
(e) Number of Separations Excluding Leave-Taker

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.

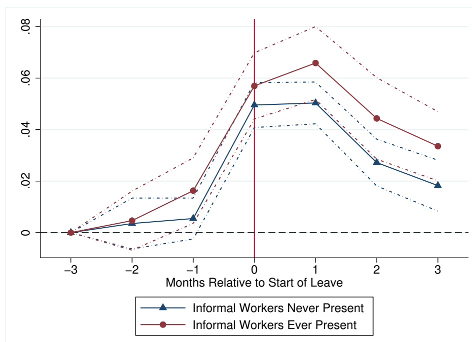
Figure A20: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Inspection Status from 2003–2011



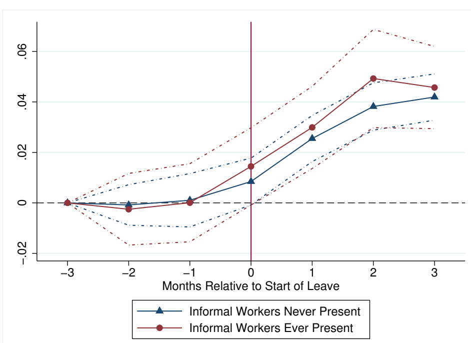
(a) Change in Number of Contracted Employees



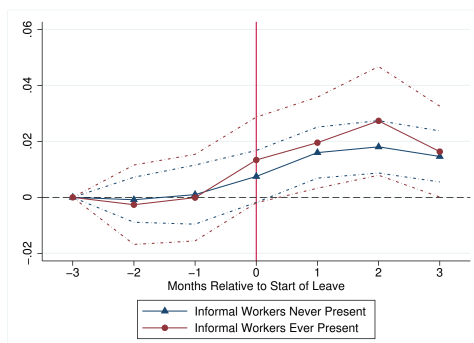
(b) Implied Number of Contracted Employees



(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.