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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 1.5 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.

JEL Codes: J23, J21, J63, J68, J13

1   Introduction

On any given day, an employer may 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. However, this is only a viable strategy if a substitute for the departing worker can be found easily and cheaply in the external market. When this is not the case, employers will instead structure internal labor markets in ways that make hiring an immediate replacement unnecessary (Doeringer and Piore 1971). For example, they could maintain excess workers as a precaution or coax coworkers to cover for an absent teammate. However, the circumstances under which employers can easily hire replacement labor are not clear.

In this paper, we measure how firms react when an employee takes maternity or non-work-related sickness leave and how those reactions are shaped by external labor market conditions and job characteristics. Our setting is Brazil, where women are guaranteed 120 days of maternity leave with full wage replacement, financed by the Brazilian government, and sickness leave is publicly financed after the first 15 days. Thus, there is little to no direct cost of leave for the employer. Using Brazilian administrative data on over 1.5 million leave spells, we estimate changes in occupational employment, hiring, and (permanent) separations in the months surrounding the initiation of maternity and sickness leave in an event study framework. 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. Therefore, our estimates capture firms' execution of their plans for managing employment flows when workers take leave.

We find that firms respond immediately to the start of leave by hiring new workers. 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 0.14–0.22 workers (including the leave-taker). After the onset of sickness leave, employment in the occupation of the leave-taker increases by 0.05–0.08 workers. For spells of sickness leave, which are more likely to come as a surprise, we generally find no evidence of employment adjustments in the months before leave starts. By contrast, employment begins increasing two months prior to the start of maternity leave. Furthermore, the hiring and employment increases are almost entirely concentrated in the occupation of the leave-taker, with little to no spillovers to other occupations within the same plant.

To help guide the analysis, we introduce a model of firms' decisions about whether to recruit or hoard labor. In the model, firms face random 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

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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 worker 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 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 increase recruiting intensity when a worker departs, but not by enough to immediately replace the worker with certainty.

Empirically, we document heterogeneous effects of leave-taking that are consistent with our model. Even so, the estimated effects are always very far from the "one-for-one" replacement-hiring benchmark. 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. Bringing these theoretical observations to the data, 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.

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 does not return at the end of their leave. Maternity leave almost always lasts the full 120 days guaranteed by law. The average duration of sickness leave is 200 days, but sickness leaves are generally shorter, with the median spell lasting 90 days. Since workers often permanently separate at the end of leave, 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. Furthermore, employers are aware of maternity leave before it begins whereas sickness leave is more of a surprise. The advance notice associated with maternity leave may allow firms to more effectively hire from the external market.

The possibility of pre-leave onset responses, especially for maternity leave, raises a challenge

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for our research design. We argue that observed increases in hiring and employment prior to maternity leave reflect anticipatory behavior by the employer, which we want to capture. However, the same pattern could arise if maternity leave is more likely to start when employment is already growing. Note, first, that our empirical model allows for arbitrary trend growth in plant-occupation employment. Hence, the selection concern is very specific: workers would need to time the onset of leave to coincide with periods of abnormally large expansions or contractions in employment (i.e., deviations from the plant-occupation trend). To allay concern about this specific type of selection, we show that our results are robust to more demanding specifications that control for arbitrary industry-by-state employment dynamics, include control units, and extend the event window.

Another concern is whether our data can fully capture firms' hiring responses. Informal employment accounts for around 40 percent of jobs in Brazil and the administrative data we use only measures formal employment. If firms hire informal workers to cover for workers on leave, our results could understate the overall hiring response. However, this appears not to be the case. 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 auxiliary data to compare hiring responses among firms that are more and less likely to use informal labor. First, we use plant-level labor inspection data and compare hiring responses between firms previously cited for informality and those that were inspected but did not have informality violations. In a second exercise, we allow for heterogeneity according to the firm's distance to the nearest local labor enforcement office, a proxy for enforcement capacity in the local market. In neither case do we find heterogeneity in employers' responses to leave-taking, suggesting informality is not an empirically important margin of adjustment.

Our work contributes to a nascent literature studying how firms respond to worker exits (e.g., Jäger and Heining 2019, Brenøe et al. 2020, Huebener et al. 2022) and a related literature on expansions in family leave policies (e.g., Gallen 2019, Bartel et al. 2021, Friedrich and Hackmann 2021, Huebener et al. 2022, Ginja et al. forthcoming). Jäger and Heining (2019) study the responses of small German firms to sudden worker deaths and find an increase in the demand for incumbent workers, concluding that firms face significant frictions in replacing workers externally. Brenøe et al. (2020) use Danish data to study the effects of women taking maternity leave on employment in small firms, and find firms hire temporary workers and temporarily increase the hours, earnings, and retention of incumbents. Gallen (2019), Bartel et al. (2021), Friedrich and Hackmann (2021), Huebener et al. (2022), and Ginja et al. (forthcoming) examine how firms react following policies that expand the generosity of family leave. Gallen (2019) finds limited effects of an unexpected and retroactively applied 2002 Danish reform that increased parental leave by 22

weeks on coworkers' employment and earnings. Ginja et al. (forthcoming) 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 new permanent and 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. Huebener et al. (2022) find German firms rely more on external hiring when there are fewer internal substitutes for maternity leave-takers, and in response to a reform that increased the generosity of paid parental leave benefits, firms hired fewer women of childbearing age into jobs with few internal substitutes. Bartel et al. (2021) surveyed small and medium-sized firms in New York and Pennsylvania and find that the introduction of paid family leave in New York led to a short-lived improvement in employers' rating of the ease of handling long employee absences, particularly for firms with 50–99 workers. The estimates from studies that exploit variation in leave benefits reflect how firms adjust to changes in leave policy, whereas we are interested in employers' immediate responses to predictable, but uncertain, absences in a stable policy environment.

We make several contributions to this literature. First, we shed light on firms' short-run responses in the months surrounding 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. With the exception of Huebener et al. (2022), the above studies examine year-by-year employment dynamics. Our monthly analysis allows us to document the precise timing of firm responses to leave. Our results suggest that the limited employment responses in years following worker departures in Jäger and Heining (2019) and Brenøe et al. (2020) do not mask stronger short-run fluctuations. Second, we document how firms react to two policy-relevant causes of workplace absence—maternity and sickness leave—in one institutional setting. The studies mentioned above largely focus on maternity or parental leave, or death in the case of 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, whereas maternity and sickness leave are more common sources of employment disruptions.[1] Studying maternity and sickness leave sheds light on the roles that uncertainty about duration and timing of onset as well as the likelihood of permanent departure play in shaping employer responses.[2] Third, we study firm responses to leave spells that are generally shorter in duration

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[1]Jäger and Heining (2019) evaluate around 1,500 deaths per year in an economy of 40 million workers. We study around 300,000 leave spells per year in a labor market only slightly larger.

[2]In this vein, 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.

than those of the other studies, with the exception of Bartel et al. (2021).[3] Fourth, different than the above-mentioned studies that focus on Europe or the United States, we document employer responses to leave-taking in a middle-income country with a large and diverse workforce. In doing so, we provide evidence on the existence and importance of labor market rigidity in Brazil.

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). A notable exception is Hensvik and Rosenqvist (2019), which uses Swedish data and shows that sickness absence is lower in occupations where there are fewer internal substitutes.[4] Their findings suggest that internal substitution can insure firms against employment absence. We are unaware of studies that analyze the hiring behavior of firms around the onset of sickness absence. There is also a very large literature 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). Only recently have studies examined firms' responses to maternity leave-taking as described above.

2   Institutional Setting

In Brazil, leave is costly for firms only because they temporarily lose access to a worker's labor. Firms do not bear the cost of wage replacement during maternity leave nor after the fifteenth day of sickness leave. Here we briefly review Brazil's policies governing maternity and sickness leave as well as the employer's costs associated with hiring 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 largely depends on internal and market-based factors rather than institutional constraints.

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[3]As noted above, almost all maternity leaves in our sample are 120 days and half of the sickness leaves are under 90 days. In contrast, the Swedish reform exploited in Ginja et al. (forthcoming) increased paid leave from 12 to 15 months. The Danish reform in Gallen (2019) extended the modal leave from 28 to 50 weeks. The Danish women who give birth in Brenøe et al. (2020) take about 9.5 months of leave. About 40 percent of German mothers in Huebener et al. (2022) returned to their employer 12 months after giving birth before the 2007 reform and a little over 20 percent did so after.

[4]Another 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 their 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.[5] Women have job protection starting from when pregnancy is confirmed up to five months after delivery, meaning there is no risk to employment, job position, or pay during that time. 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.[6] 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.[7] 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 those 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.[8]

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 deemed 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, workers are often

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

[6]The EC Program was established 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 the public sector in our analysis.

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

[8]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).

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 dismisses the new worker, they can expect to pay around 1.13 times the monthly wage as a termination cost.[9] 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 firms' retention and hiring of workers into occupation groups within plants (hereafter referred to as a plant-occupation) change in the months surrounding the initiation of maternity and 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 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 data accuracy are extremely high, as employers who fail to report face mandatory fines and risk litigation

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[9]To dismiss a worker on an open-term contract, the most common contract in Brazil that has no fixed expiration, the employer must provide cause. They must also provide 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.

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,[10] the exact date of hire, and the month and year of separation. Establishment characteristics include industry and location.

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.

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 as they correspond to the same period of absence. For the combined spell, we assign the earliest leave start and latest leave end date across the component spells.[11] 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] We also define a measure of separations 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

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

[11]In general, if fewer than seven days elapse between consecutive leave spells of the same type, we treat them as one continuous spell.

[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.

months before and after (seven months total).[14] 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. 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).[15] 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.[16] 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.[17] 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.

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 smaller plants. Second, employers see maternity 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 long on average, though half the spells last less than 90

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[14]In Section 5.3.2, we consider longer event windows.

[15]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.

[16]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 5.3.3.

[17]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.

days. It is very common for workers to not return to their job after leave, especially maternity leave.

Table 2 reports descriptive statistics for the clean maternity and sickness leave spells in our estimation sample in columns 1 and 5, respectively. We compare them to the full population of leave spells, i.e., spells regardless of whether they are clean but otherwise meet the sample restrictions (e.g., no public sector plants) in columns 2 and 6. The statistics are measured in the month of leave onset.

Our focus on clean spells is restrictive. Approximately 28 percent of maternity leaves and 15 percent of sickness leaves meet our clean definition. These low shares reflect the fact that many leaves occur at larger firms, where it is mechanically more likely that multiple leave spells begin within several months of each other (even when leave initiations are timed independently of one another). In fact, more than half of the full sample of leave spells come from plants with 50 or more employees; whereas about 90 percent of clean leave spells originate from plants with less than 50 workers. We therefore also report descriptive statistics for clean leave spells at establishments with less than 50 workers in columns 3 and 7 and for the full population of leaves that begin at these smaller establishments in columns 4 and 8. Among these smaller establishments, 54 percent of maternity and 42 percent of sickness leaves satisfy the clean definition. Thus, our clean spells reflect almost half of all leaves at plants with under 50 workers.

Other than establishment size, the characteristics of clean maternity leave spells are similar to the full sample of leaves. The top industries represented are wholesale/retail trade and manufacturing, and about 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. Thus, our analysis largely reflects the responses of smaller firms to maternity leave-taking, but along most other dimensions, the clean leave spells are representative of maternity leaves taken during this period.[18]

Turning to sickness leaves, aside from the plant size distribution, the clean spells are also quite similar to the full sample. For example, sickness leave-takers have on average 42–47 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 almost 200 days. The median spell (not reported in the table) is 90 days. Recall that we only

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[18]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. In Section 5.3.3, we show our results are robust to modifying the clean leave definition and hence the frequency of leave occurrences.

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.[19] 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 an individual going on leave should be smaller at large firms and that much of the leave policy attention centers on small firms. Huebener et al. (2022) focus on firms with up to 50 employees for similar reasons. 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.

In addition to comparing our clean spells to the full set of leave spells, we also compare the plants in our estimation sample to all plants in Brazil regardless of whether they experience leaves. In Appendix Table A1, we show the characteristics of plants with clean leaves, plants with leaves regardless of whether they are clean, and plants regardless of whether they have any leaves in 2015 (a year in the middle of our sample).[20] To make appropriate comparisons with our estimation sample, we omit public sector plants and plants with fewer than five workers during the majority of the time they are observed in our RAIS extract from the full population of plants. Industry distribution is similar across the groups. About 81 percent of all plants had 5–49 workers at the end of the calendar year; 85 (88) percent of plants with clean maternity (sickness) leaves fell in this size range. Within this size range, the full population of plants tends to be smaller. Not surprisingly, plants with maternity leaves have a larger proportion of employees who are female and of childbearing age. Plants that experience sickness leaves are very similar to the full sample of plants on these dimensions. With a few exceptions, plants included in our estimation sample are largely similar to the full population of plants in Brazil.

We next show that departures of workers going on maternity leave and sickness leave are quite likely to become permanent. In Appendix Figure A1 (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 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'

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[19]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.

[20]The statistics and patterns are similar across all sample years.

response to an almost certain 120-day absence, but also the likelihood of the woman separating from her job shortly after her leave and associated job protection end. Similarly, in Figure A1 (b), we show Kaplan-Meier survival curves for our sample of sickness leave-takers, where the x-axis reports months since the month prior to sickness leave ending.[21] The survival probability declines significantly within 6 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, Panel A of 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 in 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.[22] On average, 11–12 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, about 0.5 workers are hired in the leave-taker's occupation, and about the same number of separations occur, consistent with 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:

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

where y_{opt} denotes outcome y for 1-digit occupation group o at plant p in month t. Our outcomes of interest include net employment growth, number of workers hired during the month, and number of workers who separate during the month. Net employment growth is simply the difference

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[21]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.

[22]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.

between new hires and separations. The variable K_{pt} measures event time relative to the month a leave spell started at plant p.[23] 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 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. 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 a combination of the firm's reaction and whatever employment dynamics lead workers to start a spell of leave. Importantly, 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.[24] This condition—exogenous timing—is satisfied as long as workers do not time leave 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 untreated. 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.

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[23]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.

[24]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 (2021), 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.

The exogenous timing and delayed timing conditions are straightforward to justify for sickness leave spells. With respect to exogenous timing, it is unlikely that workers 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 leaves are largely a surprise when they start. Although employers know there is a probability of a worker taking sickness leave in any month, in most cases they do not know exactly when that probability will be realized. We note there may be some instances, such as scheduled non-emergent surgeries or lengthy treatments for conditions like cancer, where the employer knows about the absence in advance. We do not expect our results to be driven by these cases given Barbosa-Branco et al. (2011) find injuries, musculoskeletal disorders, and mental disorders are the most common diagnoses associated with sickness absence in the Brazilian private sector. As we show later, the data confirm the delayed timing assumption: we generally observe that the hiring responses occur in the month leave starts and after.

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, and among women in paid work, 49.5 percent reported their births were unintended (Theme-Filha et al. 2016). 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 correspond with deviations in employment growth from its plant-occupation-specific trend. The delayed timing assumption is more nuanced. 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 therefore 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.[25] 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. In Section 5.3.2, we consider longer event windows and provide support for the delayed timing assumption.

A technical issue arises with respect to our ability to separately identify event time effects from

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[25]According to the World Health Organization, in 2015, 55 percent of births in Brazil occurred via caesarean section, which further supports the idea that employers have an accurate sense of when leave will begin and may make adjustments in advance.

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. Our main specification is similar to that described in Borusyak and Jaravel (2017), but different in ways that allow us to overcome this 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.[26] 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 5.3.1.

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.[27,28] 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.[29] 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 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

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[26]65 percent of the clean maternity and sickness leave spells come from plant-occupations that only contribute one spell to the sample; the remainder originate from plant-occupations that contribute multiple leave spells.

[27]We also present the estimates for those four outcomes in Appendix Table A2. For all subsequent results, we only present figures in the interest of space.

[28]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). Results are presented in Appendix Figure A2. We generally find no statistically significant effects or very tiny and not economically meaningful effects of leave-taking on temporary employment dynamics. We, therefore, do not focus on those outcomes hereafter.

[29]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 where change in employment is the outcome.

see this change exactly when leave begins, even if the firm perfectly anticipated the onset of leave.

5.1 Baseline Estimates

We first report results for maternity leaves. Figure 1 displays the estimated employment dynamics in the plant-occupation of the leave-taker around the start of leave. All effects are estimated relative to three months prior to the start of the leave. Figures 1 (a) and (b) show plant-occupations experience relatively small but statistically significant increases in employment two months before leave onset, and sharper increases as leave onset approaches. Specifically, the plant-occupation is 0.06 workers larger the month before leave onset and 0.14 workers larger the month of leave onset relative to three months prior. The plant-occupation continues to grow and is 0.22 workers larger three months after leave initiation. Given the average plant-occupation of the leave-taker had 11.7 workers three months before leave onset, our results suggest the active occupational workforce is about 7 percent smaller three months after leave onset. Figures 1 (c) and (d) show how the employment adjustment is managed through hiring and separations, respectively. Firms increase the number of hires throughout the event window, with the largest increase occurring in the month of leave onset and large increases also taking place 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 increase efforts to retain incumbent workers. Three months following leave onset, there is a small and statistically significant increase in separations. In Figure 1 (e), we present results for separations excluding the leave-taker, and find a nearly identical increase. 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. The increase in separations may also capture firms shedding replacement workers before the probationary period ends.

We contrast these results with the employment dynamics observed around the onset of sickness leave, which are reported in Figure 2. Notably, we do not find significant changes in any of the outcomes we consider two months preceding the start of the leave, with point estimates very close to zero. We find a slight increase in employment and hiring the month before sickness leave onset, with plant-occupations adding 0.007 workers on net (Figure 2 (a)). We attribute this tiny pre-leave response to some sickness leaves being anticipated, such as those related to non-emergency scheduled surgeries or treatments for conditions like cancer, and employers adjusting in advance of the absence. The general lack of pre-leave onset responses supports our identifying assumption that sickness leave is not strategically timed based on idiosyncratic plant dynamics, and is consistent with the exact start of most sickness leaves coming as a surprise to the firm. Figures 2 (a)–(c)

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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 0.05 workers larger in the month the leave starts and 0.08 workers larger the following month relative to three months before leave onset. Relative to the average size of the plant-occupation three months prior to leave onset (11.3 workers), the active occupational workforce is about 8 percent smaller in the month of and one month after leave onset. Separations steadily increase after the leave initiates, peaking at 0.046 three months after the onset of leave (Figure 2 (d)). When we consider separations excluding the leave-taker (Figure 2 (e)), they increase and plateau around 0.02. Thus, the increase in separations reflects, in part, the leave-taker separating and potentially the leave-taker's replacement separating. By three months after the start of the sickness leave, the plant-occupation is 0.045 workers larger compared to three months prior to leave onset.

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 almost certainly aware of maternity leave in advance and that it will 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, other occupations within the plant might be impacted. For example, the firm might hire replacement workers in a closely-related occupation. We explore whether there are spillover effects by estimating equation (1) on the other (non-leave-taking) 1-digit occupations within the same plant as the leave-taker. Figures 3 and 4 display the estimated results, along with the effects on the leave-taker's occupation for comparison.

The results suggest limited spillovers of maternity leave-taking (Figure 3). There is no change in employment or hiring two months before leave onset. Thereafter, occupational employment grows slightly. Three months after the leave begins, the non-leave-taking occupations are about

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0.04 workers larger relative to three months prior to leave onset (whereas the occupation of the leave-taker is 0.22 workers larger). There is little evidence that hiring drives this modest growth. Instead, there are small and marginally significant declines in separations in the non-leave-taking occupations, suggesting that employers increase their retention efforts plant-wide during a maternity leave spell. We generally find no statistically significant or economically meaningful spillover effects of sickness leave (Figure 4).[30]

The spillover analysis yields some important insights. First, the effects of leave-taking are concentrated almost entirely within the 1-digit occupation of the leave-taker. The lack of spillover effects to other occupations (in both absolute and relative terms) is similar to the results in Jäger and Heining (2019), Brenøe et al. (2020), and Huebener et al. (2022). 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 across occupations within the same plant 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.

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). 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   Interpretation of Pre-Leave Onset Responses

One question that emerges from our baseline results is whether the increase in hiring and employment in the occupation of the leave-taker prior to maternity leave onset (and to a much lesser extent sickness leave onset) reflect anticipatory behavior by the employer, which we want to measure, or individuals timing leave to coincide with a large acceleration of employment growth (i.e., selection

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[30]We also allow the employment dynamics to differ 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 that get masked by pooling the data. In Appendix Figures A3 and A4, we display the results for occupational employment changes and implied levels. We find little evidence of spillovers with one exception. 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. That the most notable spillovers occur when managers take maternity leave is perhaps not surprising, as women with leadership positions within firms may be more deliberate about timing their absence. Only 5 percent of our clean maternity leaves are taken by managers.

into leave). We know employers are aware of maternity leave spells (and perhaps some sickness leave spells) before they occur, and expect that they will alter hiring and separation decisions in advance. Thus, anticipatory responses are especially likely in the case of maternity leave. We cannot formally distinguish whether the estimated pre-leave responses reflect anticipatory behavior or selection. However, we explore the sensitivity of our estimates to alternative specifications and the results provide a collage of evidence that supports the anticipatory interpretation. Complete results of these robustness exercises appear in Appendix Figures A5–A8. To keep the presentation concise, we discuss results for employment in levels in the main text.

Our baseline specification includes calendar time fixed effects to account for the possibility that individuals time their leave based on aggregate labor market conditions. We reestimate equation (1) including granular industry-state-specific calendar time fixed effects to address concerns that employees time the start of their leave to fluctuating regional industry conditions.[31] The results are displayed in Figures A5 and A6. We also display the baseline estimates and estimates from models without any calendar time effects for comparison. 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 results, including the pre-leave onset responses, are nearly identical to our baseline results, alleviating concerns about selection into leave based on local sector-specific labor market conditions.

We next estimate our baseline specification including non-leave-taking occupations at the plant where the leave occurred as control units. We use these 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. As discussed above, the general lack of response in these non-leave-taking occupations prior to leave onset supports the idea that individuals do not select into leave based on plant-specific conditions and that the pre-leave onset responses in the occupation of the leave-taker indeed reflect anticipatory behavior on the part of the employer. Nevertheless, if the relatively modest increases in employment in non-leave-taking occupations in response to maternity leave reflect changes in plant conditions and women time their leave to these conditions, then using non-leave-taking occupations as control units will purge the treatment effects of this selection. If instead the modest increases reflect true spillover effects, using these occupations as control units will attenuate the estimated treatment effects on the leave-taker's occupation.

Another advantage of including non-leave-taking occupations as control units is we can include

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

plant-occupation-leave spell fixed effects. Our baseline model in equation (1) 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. As mentioned earlier, our baseline model does not suffer from this problem because it 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. By including control units, we can relax this somewhat arbitrary assumption as those units separately identify the calendar time effects. Importantly, the inclusion of plant-occupation-spell effects allows plant-occupation unobserved heterogeneity that is correlated with responses to leave to change across leave spells. This flexibility is important if managerial practices or firm technology change as firms experience more leaves.

Figures A7 and A8 show estimates with plant-occupation fixed effects and estimates with richer plant-occupation-spell fixed effects. For both maternity and sickness leave, the employment dynamics are quantitatively similar to our baseline results, albeit somewhat muted, when we include control occupations, with the gap between the baseline estimates and estimates from the inclusion of control groups growing over time. For example, three months after maternity (sickness) leave onset, the baseline estimates imply the plant-occupation of the leave-taker is 0.22 (0.045) workers larger relative to three months prior, while the estimates with control units imply the occupation is 0.17–0.19 (0.01–0.02) workers larger. The slight dampening of the effects is not surprising given that the non-leave-taking occupations experienced very modest growth. 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. Overall, these exercises point to the anticipatory interpretation of the pre-leave onset increases in hiring and employment, not selection into leave.[32,33]

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[32]As mentioned earlier, Sun and Abraham (2021) 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 A9 and A10, we show that estimated event time coefficients from the first half of our estimation period are indistinguishable from those in the second half.

[33]We also re-estimated our baseline maternity leave specifications excluding maternity leaves that began soon after the woman took a sickness leave. About 10 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.

5.3.2 Longer Event Windows

Two closely-linked concerns with our analysis are the relatively short pre-event period and the assumption that employment outcomes are untreated three months before leave initiation. These concerns are especially salient for our maternity leave analysis as women have an incentive to notify their employer early since job protection begins at that point, and the employer could respond immediately upon learning of the pregnancy. To gauge the reasonableness of our assumption that outcomes three months prior to leave onset are untreated, we estimate models with longer event windows of four and five months around leave initiation.[34]

The results are presented in Appendix Figures A11–A14. In all cases, we normalize the earliest event time coefficient to zero. For maternity leave spells, the 4-month window analysis yields no statistically or economically meaningful increases in employment or hiring three months before leave onset (Figure A11). In the 5-month window analysis, the plant-occupation of the leave-taker is less than 0.02 workers larger three months before leave onset (Figure A13). Taken together, our assumption that outcomes three months prior to maternity leave onset are untreated appears reasonable. If anything, measuring the employment dynamics relative to a period where there were already tiny increases in employment will lead us to slightly underestimate the treatment effects. Notably, in both the 4-month and 5-month window analyses, there are relatively sharp employment upticks two months and one month before leave onset, similar to our baseline results, and the employment dynamics in the month of leave onset and few months after are also similar.

The results for sickness leave fully support the idea that three months prior to sickness leave onset is untreated. In both the 4-month and 5-month window analyses, we find no statistically significant changes in employment three months before leave onset (Figures A12 and A14). Furthermore, almost all the event time coefficients prior to leave onset are very close to zero and statistically insignificant, which is consistent with the notion that sickness leaves are generally not anticipated. The lone exception is in the 4-month window analysis, there is a 0.006 increase in hiring the month before sickness leave begins, which as mentioned earlier is likely driven by a small set of sickness leaves that may occur with some notice. 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 mater-

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[34]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.

nity leave initiation, which is predominantly driven by the leave-taker separating from the firm after the full amount of leave and job protection allowed by law. 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 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.[35]

5.3.3 Modifications of the Clean Leave Definition

As described earlier, we focus on clean leaves, meaning the event window of that leave does not overlap with the event window of another leave initiation of that type at the plant. In the case of our baseline analysis where the event window includes three months before, the month of, and three months after leave onset, ensuring no overlap of event windows means a clean leave must be the only leave start of that type six months before and six months after it begins. There may be concerns about external validity if the response of employers to leave differs by the frequency with which leaves occur.[36]

Our results in the prior section, however, suggest that employers' responses to leave-taking are robust to modifications of the clean leave definition (and hence the frequency of leave). In particular, as noted earlier, the data from the 4-month and 5-month window analyses are nested subsets of the main estimation sample. As we increase the event window, the clean leave definition becomes more stringent, requiring that the focal leave is the only one in the eight (ten) months before and eight (ten) months after it starts in the case of the 4-month (5-month) window. In

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[35]For both maternity and sickness leaves, about 75 percent of the spells in our baseline analysis meet the requirements for the 4-month window analyses; about 55 percent meet them for the 5-month window.

[36]Our focus on plants that experience only one leave of a given type over the course of several months is not without precedent in this literature. For example, Jäger and Heining (2019) exclude firms with multiple worker deaths in a given year. Gallen (2019) focuses on firms where there was exactly one worker who gave birth between October 1, 2001 and March 31, 2002. Huebener et al. (2022) focus on firms that had exactly one birth and no other births occurred two years before or after. Brenøe et al. (2020) consider treated women as those who gave birth but had no other birth the year before and after, and control women are those without a birth in the same set of years (the event year and the year before and after).

Section 5.3.2, we found the pattern of employment dynamics in the longer window analyses were quantitatively similar to those from our 3-month (baseline) analysis. In Appendix Figures A15 and A16, we present estimates from the 3-month window specification (i.e., equation (1)) using the spells in the 4-month and 5-month window samples, and results are nearly identical. Thus, our results are quite stable for plants where leaves occur at about a 1–2 year frequency.

Given the lack of spillover effects on other occupations in the plant of the leave-taker, we consider a relaxation of the clean spell definition, defining clean spells at the plant-occupation level rather than the plant level. That is, we redefine a clean maternity (sickness) leave as 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. About 1.12 million (1.31 million) maternity (sickness) leave spells satisfy this relaxed definition, compared to 774,000 (819,000) maternity (sickness) leaves in our baseline analysis, a non-trivial increase. In fact, 40 (24) percent of all maternity (sickness) leaves and nearly 70 (54) percent of maternity (sickness) leaves that start at plants with less than 50 workers are clean under this definition. In Appendix Figures A17 and A18, we present estimates of equation (1) using the relaxed clean spell definition, and they are nearly identical to our baseline estimates. While external validity concerns cannot be dismissed, overall our results are quite robust to non-trivial changes that strengthen and relax the clean spell definition, and hence change the set of plant-occupations included in estimation and the frequency with which they experience leaves.

5.3.4 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 firms (i.e., firms registered with the tax authorities) and formal workers (i.e., workers with a signed labor card); 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, informal employment tends to be concentrated in informal (i.e., unregistered) firms (Almeida and Carneiro 2012). Second, firms primarily use informal contracts to avoid complying with features of the labor code, especially the termination costs described in Section 2.3, and there is evidence that informal workers trade off the benefits of formal employment against higher wages (Almeida and Carneiro 2012). A firm can avoid these termination costs when they formally hire a replacement by making judicious use of the 90-day probationary period and terminating the worker within the 3-month limit.

To empirically investigate the importance of informal contracts, we use plant-level data on labor inspections from 2003–2016. For each inspection, the data contain the date of inspection, the plant being inspected, aspects of the labor law inspected, and whether the plant was found in violation

of the relevant law. 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 (the period before the start of the leaves in our sample) 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.[37] 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 are more likely to use informal workers as a substitute for hiring formal workers. Informal worker violation history is a reasonable proxy for a firm's propensity to use informal workers as plants with a history of such citations are more likely to have future violations.[38]

The results are presented in Appendix Figures A19 and A20 for maternity and sickness leave, respectively. In the case of maternity leave, the employment dynamics are very similar across firms regardless of their violation history. If anything, employment after leave starts is larger for firms with a history of informality violations, though the differences are not statistically significant. For spells of sickness leave, employment levels and hiring are slightly larger, though not significantly, for firms that never had an informality violation. Thus, it does not appear that firms with a history of informality violations exhibit meaningfully different formal employment responses to leave-taking compared to firms that never had such violations.

We next use publicly available data from Almeida and Carneiro (2012), which contain driving distance (measured in hours) between municipalities and the nearest local labor enforcement office (*subdelegacia*) in 2002. Almeida and Carneiro (2012) find municipalities further from the nearest local labor office are less likely to receive a visit from a labor inspector; thus, distance to the nearest office proxies for enforcement capacity in a local market. We compare employment dynamics among firms less than one hour versus an hour or more from a local enforcement office. About three-quarters of the plants in our sample are within an hour distance. Results are presented in Appendix Figures A21 and A22. We find little difference in employment dynamics. If anything, firms further from enforcement offices have larger formal hiring responses to sickness leave.

These analyses are not dispositive of the implications of informal sector employment for our analysis of employer responses to leave. However, they provide a collage of evidence that our results would not be substantially different were we able to observe both informal and formal contracts.

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[37]In Appendix Table A3, we show plant characteristics associated with clean leaves separately by inspection status.

[38]Among private sector plants that have 5 or more workers during the majority of time they are observed, 14 percent of those with a history of informal worker violations before 2012 have subsequent informal worker violations, whereas only 3 percent of those without a violation history go on to have informal worker citations. When we condition on being inspected after 2011, the equivalent statistics are 32 percent for plants with a violation history and 16 percent for plants without.

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 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. 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, $\tau = 1$.[39]

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

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

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

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

The spot market outcome corresponds to the case where $s_1 = 0$ and $s_0 = \bar{s}$; the manager does not do any precautionary recruiting, 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

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

and

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

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

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

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

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

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[39]In a more elaborate setup, firms are exposed to 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.

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 allowing for heterogeneity in the responses to leave-taking by workers in managerial, technical, and production jobs.[40] 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 5 suggest that the employment and hiring responses among managerial occupations are far more muted, especially relative to production occupations.

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[40] 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).

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 6, we again find that the hiring and employment responses among managerial occupations are muted relative to production and technical occupations, especially in the month of and after leave onset. Through the lens of our model, the smaller response to leaves of managers is consistent with a higher marginal cost of recruiting managerial skill.

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 estimating our baseline model allowing for heterogeneity by the thickness of the local labor market. We divide markets into terciles based on a thickness measure that captures the availability of workers in the same occupation (of the leave-taker). Specifically, following Jäger and Heining (2019), we break Brazil into 137 mesoregions, and for each 1-digit occupation, we measure the share of mesoregion employment in that occupation relative to its share in the state where that region is located.[41] 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 7 and 8, respectively. For maternity leaves, there is some evidence that the hiring 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 8 shows substantial contrasts in employment dynamics by market thickness. In the thickest markets, the employment response peaks at 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. (forthcoming), 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

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[41]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 Estatística* (IBGE).

replacement, suggesting firms largely handle absence through channels other than external hiring.

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; the second tercile have between 14 and 31 months of tenure; the third tercile have more than 31 months of tenure. For sickness leave-takers, jobs in the first tercile have 15 months of tenure or less; the second tercile have between 16 and 42 months of tenure; the third tercile have more than 42 months of tenure.

Figure 9 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.

The patterns are different for sickness leave, as illustrated in Figure 10. 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 0.1 workers smaller relative to three months prior to the leave start, while occupations where the leave-taker has relatively high tenure are more than 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.[42] We explore this idea in Section 6.6.

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[42]The median sickness leave lasts 83 (99) days for workers in the lowest (top) tercile of tenure.

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 11 shows results where we allow employment dynamics around maternity leave to vary across plants with 5–9, 10–19, 20–49 workers, and 50–99 workers.[43] These size classes account for over 90 percent of our clean spells. Figure 12 shows the analogous results for sickness leave.

For maternity leave, there is no indication that very large plants are less responsive. If anything, firms with 50–99 workers exhibit larger employment responses, though the confidence intervals are wide. The larger responses are driven by a sustained reduction in separations, consistent with larger firms relying on the effort of incumbent workers. The employment and hiring estimates are nearly identical for firms with 5–9, 10–19, and 20–49 workers. The most noticeable difference is plants with 20–49 workers do not see a significant change in separations, whereas smaller plants see small increases. Turning to sickness leave, the responses of the largest firms are dampened but very imprecisely estimated. Firms with 20–49 workers exhibit smaller employment and hiring increases at and after sickness leave onset compared to plants with less than 20 workers. Thus, there is modest heterogeneity by plant size, with larger plants exhibiting behaviors, such as increased retention in the case of maternity leave and smaller hiring responses in the case of sickness leave, consistent with internal employment adjustments.

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 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.

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 duration and the probability the leave-taker returns (corresponding to τ in the model). In

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[43]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.

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, half 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.[44] We group leaves into terciles of predicted duration. The first tercile includes leave spells predicted to last up to 105 days; the second tercile includes those predicted to last between 106 and 151 days; and, the third tercile includes spells predicted to last 152 days or more.

Figure 13 shows hiring and net employment responses are larger at and after leave onset among spells in the second 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 tercile (over 151 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 13 (d) and (e) demonstrate that individuals predicted to have shorter leaves are also more likely to permanently separate from the 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 handling 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.[45] Our RAIS data include workers' actual monthly compensation, which may be distinct from their contracted monthly salary, for three years: 2015–2017. Using this information, we construct monthly measures of the wage bill in each plant-occupation.[46] We focus on the monthly wage bill including the earnings of all contracted workers except during periods of leave, the monthly wage bill excluding the earnings of the focal leave-taker for the entire

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[44]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. We include 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.

[45]Unfortunately, the data do not include a reliable measure of hours (or days) worked.

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

event window, and the monthly wage bill of incumbent workers.[47] Given the limited years available for this analysis, it is rare to observe the same plant-occupation contribute more than one clean leave. We therefore estimate specifications similar to those in Section 5.3.1, using non-leave-taking occupations in the plant of the leave-taker as control groups and including plant-occupation-spell effects.[48] Panel B of Table 3 reports descriptive statistics for this sample separately by leave-taking occupations and control occupations three months prior to leave onset.

7.1 Maternity Leave and the Wage Bill

Figure 14 (a) shows that prior to maternity leave onset, there is little economically meaningful change in the wage bill of the occupation of the leave-taker relative to other occupations at the same plant. In the month leave begins, the wage bill drops by about 1,400 reais ($395 in 2016) and stays about 1,300 reais lower for the following months, a 6 percent decline relative to the average plant-occupation wage bill three months prior to leave onset. The decline is a bit smaller than the average monthly earnings of maternity leave-takers (1,572 reais). Thus, the sustained drop in the wage bill largely reflects that the leave-taker falls off the payroll, since the government effectively pays the leave-taker's salary for 120 days. In Figure 14 (b), we exclude the leave-taker's earnings for the whole event window and find that the net wage bill follows dynamics similar to the path of employment. The wage bill rises by less than 90 reais in the months before leave onset, and by two months after the leave starts, the wage bill is 200 reais higher than at baseline. These increases could reflect both the hiring of external workers as well as intensive margin adjustments if firms increase the work hours or remuneration of incumbent workers.

Jäger and Heining (2019) emphasize that when external market adjustments are difficult, firms respond to worker departures with increased demand for the labor of incumbent workers. We therefore focus on changes in the wage bill for incumbent workers, where incumbents are defined as those contracted with the plant-occupation throughout the entire event window. Figure 14 (c) shows that the incumbent wage bill increases by about 50 reais in the months before leave onset, with slightly larger increases after the start of leave. By three months after leave onset, the incumbent wage bill is about 110 reais larger than three months before onset, less than a 1 percent increase in the wage bill of these workers relative to baseline.

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[47]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.

[48]Furthermore, we cannot use all clean spells that begin during the 2015–2017 period as we 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 346,771 (363,725) clean maternity (sickness) leave spells, and 85 (84) percent come from plant-occupations that only contribute one clean spell during the period.

We could have alternatively defined incumbents as those contracted with the plant-occupation three months prior to leave onset and not conditioned on staying with the employer; however, given the substantial labor market churn in Brazil,[49] the main variation in wage bills of incumbents defined in that way comes from their separation from the plant, leading to a general decline in wage bills. We find it more informative to focus on wage bills of workers who stay with the plant throughout the event window, acknowledging that this is a selected sample.

7.2 Sickness Leave and the Wage Bill

Turning to sickness leave, Figure 14 (d) shows that the wage bill of the leave-taker's occupation is stable prior to leave onset, but then drops by about 1,550 reais (about $435 in 2016) at the start of the leave, which is almost exactly the average wage of sickness leave-takers (1,605 reais). The wage bill remains lower compared to its pre-leave level, but begins to increase, which may reflect the leave-taker returning to work among other margins of adjustment. When we exclude the focal leave-taker's earnings for the full event window, Figure 14 (e) shows that the wage bill is about 10–40 reais larger in the month of leave onset and the two months following, but only the increase one month after onset is statistically significant ($p < 0.10$).

When we focus on incumbent workers, we observe increases in earnings prior to leave onset and somewhat larger increases following onset. Three months after leave onset, the incumbent wage bill is 255 reais (about 2 percent) larger than three months before onset. We attribute the rise in incumbent wage bills prior to leave onset to the fact that this is a selected sample of workers who stay with the plant-occupation, and employers may differentially retain workers in response to sickness leave. Our results are consistent with employers retaining workers experiencing earnings growth prior to leave onset, perhaps a signal of their productivity and ability to take on the responsibilities of the leave-taker.

7.3 Discussion

Overall, the wage bill analyses suggest that firms modestly increase the demand for incumbent workers in the same occupation as the leave-taker. Incumbent worker wage bills increase by 7–16 percent relative to the average leave-taker's earnings, and the increased reliance on incumbent workers yields little change in overall labor costs. The small external hiring responses along with the small increases in incumbent wage bills are similar to the estimated responses of German firms to worker deaths in Jäger and Heining (2019) and maternity leave in Huebener et al. (2022), and Danish firms to women giving birth in Brenøe et al. (2020). Our analysis shows that their findings hold even in the very short run, focusing on the months right around the start of leave, and when

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[49]Recall, in the leave-taker's occupation, on average, 0.5 workers separate three months prior to leave onset.

considering both maternity and sickness leaves in a middle-income country.

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, one-fifth of a worker to replace an employee 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, Huebener et al. 2022) suggest our results are not driven by the labor market imperfections in Brazil documented by Engbom and Moser (2018).

We find modest increases in the compensation of incumbent 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 2022). The extent to which incumbent hours can be adjusted is limited in Brazil. The maximum number of work 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 (Huebener et al. 2022, Ginja et al. forthcoming). 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 of Industrial Goods and Services
8	Production of Industrial Goods and Services
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. Group 7 employees work in production systems that tend to be more discreet and deal with the shape of the product rather than the physical-chemical content, while group 8 employees work in production systems that tend to be continuous (e.g., chemical, steel).

Table 2: Descriptive Statistics of Maternity and Sickness Leave Spells

	Maternity		Maternity (Small Plants Only)		Sickness		Sickness (Small Plants Only)	
	Clean	All	Clean	All	Clean	All	Clean	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Plant Characteristics								
Industry								
Agriculture/Forestry/Fishing	0.018	0.015	0.016	0.013	0.043	0.034	0.044	0.043
Mining	0.002	0.002	0.001	0.001	0.004	0.007	0.004	0.005
Manufacturing	0.128	0.176	0.113	0.113	0.158	0.254	0.155	0.175
Utilities: Electric/Gas	0.001	0.002	0.001	0.000	0.002	0.003	0.001	0.001
Utilities: Water/Sewage/Waste	0.003	0.003	0.002	0.001	0.004	0.011	0.004	0.005
Construction	0.019	0.015	0.013	0.011	0.041	0.061	0.039	0.050
Wholesale/Retail Trade	0.402	0.291	0.425	0.419	0.349	0.198	0.359	0.333
Transportation/Storage/Mail	0.030	0.028	0.023	0.019	0.050	0.078	0.049	0.057
Accommodation/Food	0.104	0.072	0.112	0.117	0.085	0.047	0.088	0.091
Information/Communication	0.018	0.023	0.017	0.016	0.016	0.016	0.014	0.013
Financial Services	0.040	0.037	0.042	0.043	0.033	0.021	0.034	0.034
Real Estate	0.005	0.003	0.005	0.005	0.004	0.002	0.004	0.003
Professional/Scientific/Technical	0.038	0.029	0.039	0.037	0.027	0.016	0.027	0.021
Administrative Activities	0.053	0.113	0.048	0.046	0.071	0.121	0.070	0.061
Public Admin/Defense/Social Security	0.000	0.001	0.000	0.000	0.000	0.002	0.000	0.000
Education	0.057	0.063	0.055	0.067	0.041	0.028	0.037	0.034
Health/Social Services	0.039	0.089	0.040	0.044	0.027	0.070	0.027	0.030
Art/Culture/Sports	0.009	0.005	0.009	0.008	0.008	0.004	0.008	0.007
Other Service Activities	0.037	0.033	0.038	0.037	0.036	0.024	0.036	0.034
# of Contracted Workers								
1 to 4	0.030	0.012	0.034	0.026	0.033	0.009	0.036	0.028
5 to 9	0.263	0.102	0.299	0.224	0.280	0.068	0.303	0.207
10 to 19	0.329	0.155	0.373	0.342	0.341	0.106	0.370	0.322
20 to 49	0.260	0.185	0.294	0.408	0.267	0.146	0.290	0.443
50 to 99	0.078	0.114	0.000	0.000	0.064	0.108	0.000	0.000
100 to 249	0.032	0.124	0.000	0.000	0.013	0.140	0.000	0.000
250 to 499	0.006	0.081	0.000	0.000	0.001	0.107	0.000	0.000
500 to 999	0.002	0.070	0.000	0.000	0.000	0.099	0.000	0.000
1000+	0.000	0.159	0.000	0.000	0.000	0.218	0.000	0.000
Female Employee Share	0.575	0.594	0.605	0.644	0.420	0.405	0.423	0.425
	(0.271)	(0.236)	(0.262)	(0.246)	(0.307)	(0.284)	(0.311)	(0.309)
Female Aged 21–35 Employee Share	0.336	0.339	0.358	0.390	0.214	0.203	0.216	0.215
	(0.210)	(0.178)	(0.209)	(0.203)	(0.201)	(0.167)	(0.204)	(0.197)
Plant-Occupation Characteristics								
Female Employee Share	0.721	0.687	0.749	0.765	0.420	0.406	0.423	0.425
	(0.266)	(0.240)	(0.254)	(0.239)	(0.379)	(0.338)	(0.383)	(0.379)
Female Aged 21–35 Employee Share	0.468	0.419	0.491	0.505	0.203	0.197	0.204	0.204
	(0.287)	(0.238)	(0.289)	(0.275)	(0.258)	(0.208)	(0.262)	(0.254)
Leave-Taker Characteristics								
Avg Tenure (Months)	31.763	31.877	31.190	29.213	46.712	45.968	46.734	41.908
	(31.091)	(32.287)	(30.472)	(29.697)	(56.731)	(62.678)	(56.602)	(56.208)
Avg Leave Length (Days)	120.915	120.671	120.802	119.370	196.430	194.965	197.163	195.053
	(23.136)	(26.065)	(22.918)	(25.362)	(283.788)	(295.486)	(284.725)	(287.904)
Female	1.000	1.000	1.000	1.000	0.451	0.455	0.454	0.457
1-Digit Occupation								
Public Administration & Management	0.052	0.039	0.053	0.054	0.035	0.017	0.036	0.033
Professionals in Science and Arts	0.082	0.109	0.076	0.083	0.043	0.041	0.039	0.038
Mid-level Technicians	0.074	0.102	0.069	0.075	0.061	0.083	0.058	0.057
Administrative Services	0.368	0.334	0.366	0.343	0.182	0.175	0.181	0.175
Service Workers & Vendors	0.331	0.269	0.348	0.350	0.343	0.274	0.351	0.335
Agriculture, Fishermen, & Forestry	0.011	0.011	0.010	0.008	0.038	0.038	0.039	0.039
Production/Manufacturing I	0.066	0.104	0.062	0.070	0.232	0.286	0.230	0.254
Production/Manufacturing II	0.014	0.027	0.013	0.014	0.038	0.059	0.038	0.042
Repair & Maintenance	0.003	0.003	0.002	0.002	0.029	0.028	0.029	0.028
# of Leaves	773,967	2,790,761	682,690	1,263,826	818,666	5,510,727	754,493	1,812,389

Note: All statistics are measured in the month of leave onset. Columns (1) and (5) include clean maternity and sickness leave spells, respectively. Columns (2) and (6) 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. Columns (3) and (7) include the subset of spells from columns (1) and (5), respectively, at establishments with less than 50 contracted employees at the month of leave onset. Columns (4) and (8) include the subset of spells from columns (2) and (6), respectively, at establishments with less than 50 contracted employees at the month of leave onset. Standard deviations of non-categorical variables are reported in parentheses.

Table 3: Pre-Leave Plant-Occupation Employment and Wage Bill Descriptive Statistics

	Maternity		Sickness	
	Own Plant-Occ	Spillover Plant-Occ	Own Plant-Occ	Spillover Plant-Occ
Panel A: Employment Outcomes				
Change in # of Contracted Employees	-0.002	-0.008	-0.016	-0.005
	(2.565)	(3.344)	(1.956)	(1.582)
# of Contracted Employees	11.754	7.143	11.298	4.703
	(24.336)	(23.271)	(15.492)	(10.155)
# of Hires	0.501	0.290	0.455	0.165
	(2.345)	(4.011)	(1.695)	(0.977)
# of Separations	0.503	0.298	0.471	0.170
	(2.300)	(4.161)	(1.820)	(1.582)
# of Plant-Occupations	773,967	1,733,283	818,666	1,677,139
Panel B: Wage Bills				
Wage Bill	20,891.34	15,068.28	20,125.87	10,226.42
	(64,468.42)	(58,618.19)	(49,136.63)	(31,576.81)
Wage Bill Excluding Focal Leave-Taker Earnings	19,344.90		18,551.50	
	(64,055.97)		(48,705.25)	
Incumbent Wage Bill	14,746.51	11,804.14	14,094.16	8,185.53
	(52,311.57)	(46,556.54)	(39,615.01)	(25,653.08)
# of Plant-Occupations	346,771	778,399	363,725	758,399

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. Panel A shows plant-occupation employment, hiring, and separations. Panel B shows plant-occupation wage bills. Incumbents are defined as those contracted with the plant-occupation throughout the event window (i.e., three months prior to, the month of, and three months after leave onset). Wage bills are measured in Brazilian reais.

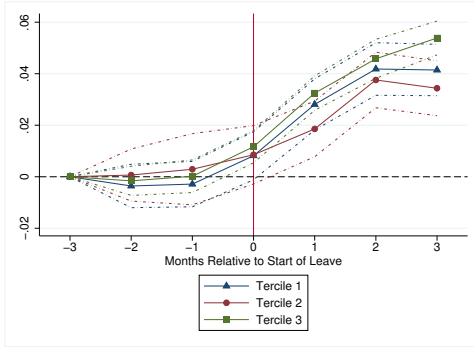Figure 1: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker

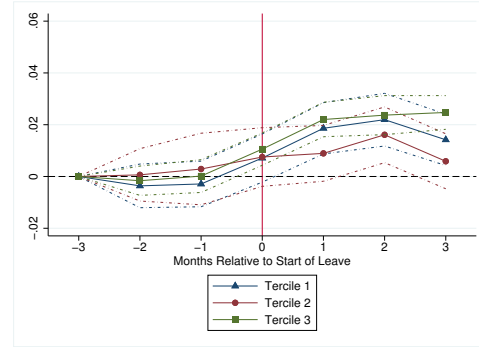(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations
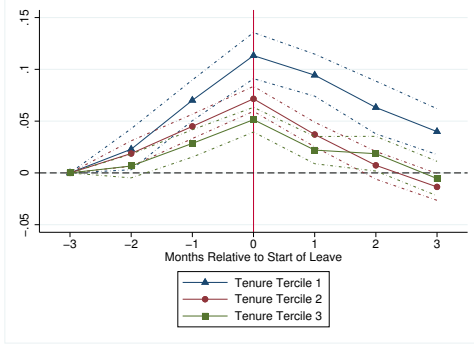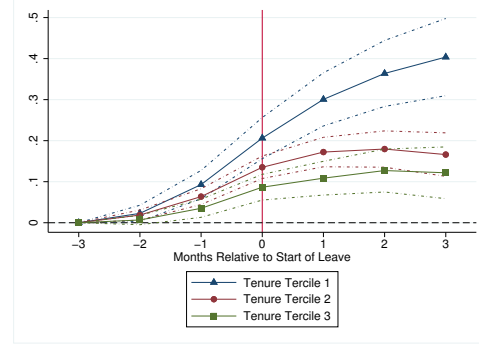
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients 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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
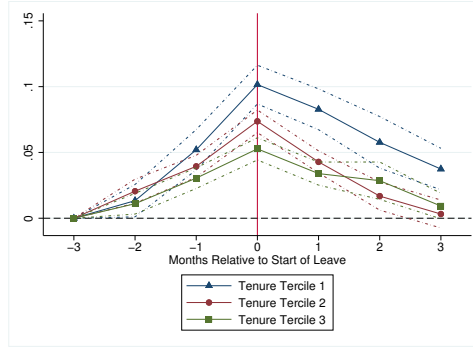
Figure 2: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker
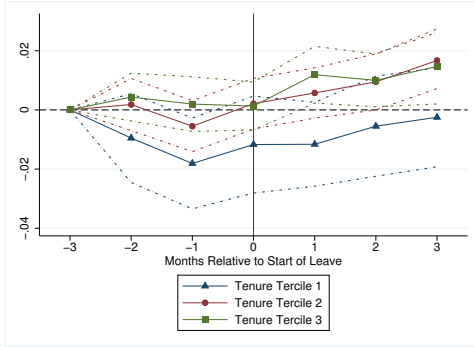

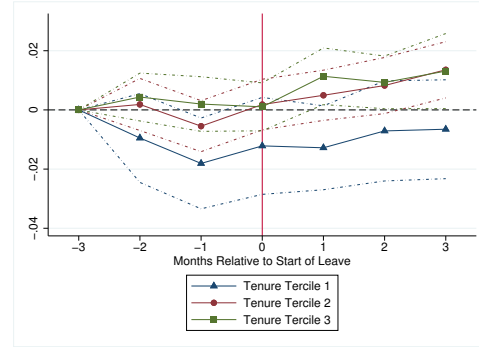(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees

(c) Number of Hires
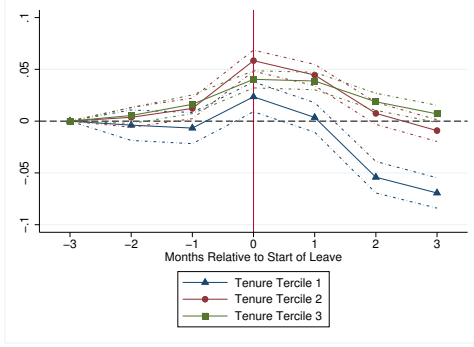
(d) Number of Separations

(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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Figure 3: Employment Dynamics around Maternity Leave Initiation in Non-Leave-Taking Occupations

(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations

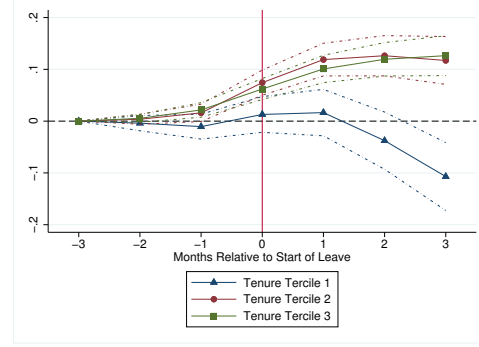*Note:* The panels display regression coefficients from equation 1 estimated separately for the plant-occupation of the maternity leave-taker (i.e., the baseline estimates) and the spillover occupations (i.e., other occupations in the same plant as the maternity leave-taker). Coefficients in $k = -3$ are normalized to zero. The dashed lines denote 95% confidence intervals based on standard errors clustered at the leave spell level. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
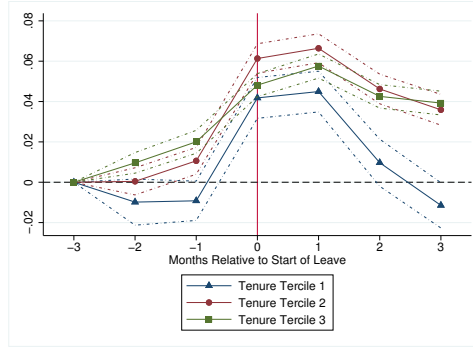
Figure 4: Employment Dynamics around Sickness Leave Initiation in Non-Leave-Taking Occupations
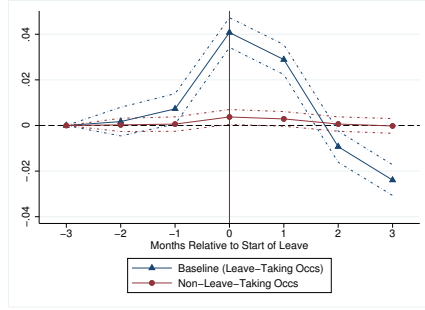
(a) Change in Number of Contracted Employees
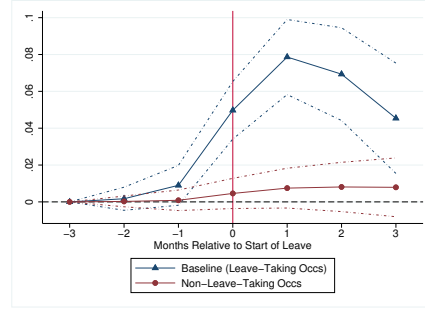
(b) Number of Contracted Employees
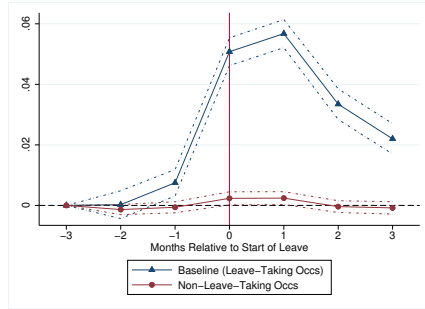
(c) Number of Hires
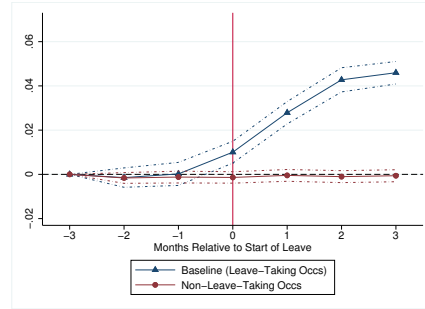
(d) Number of Separations

Note: The panels display regression coefficients from equation 1 estimated separately for the plant-occupation of the sickness leave-taker (i.e., the baseline estimates) and the spillover occupations (i.e., other occupations in the same plant as the sickness leave-taker). 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 panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

Figure 5: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Coarse Occupation Categories
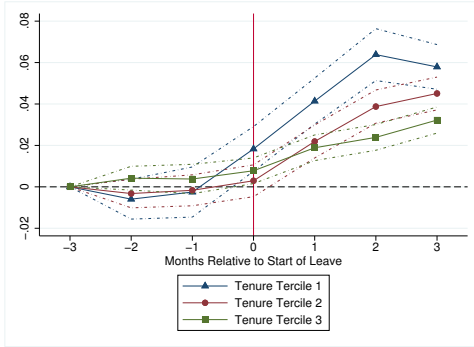

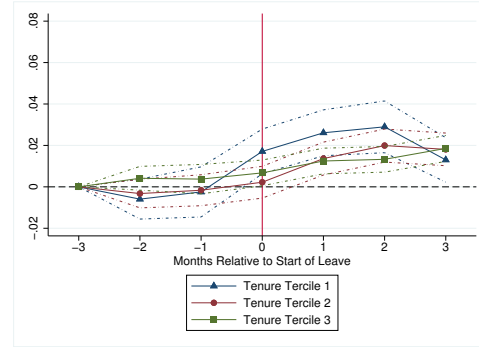(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees
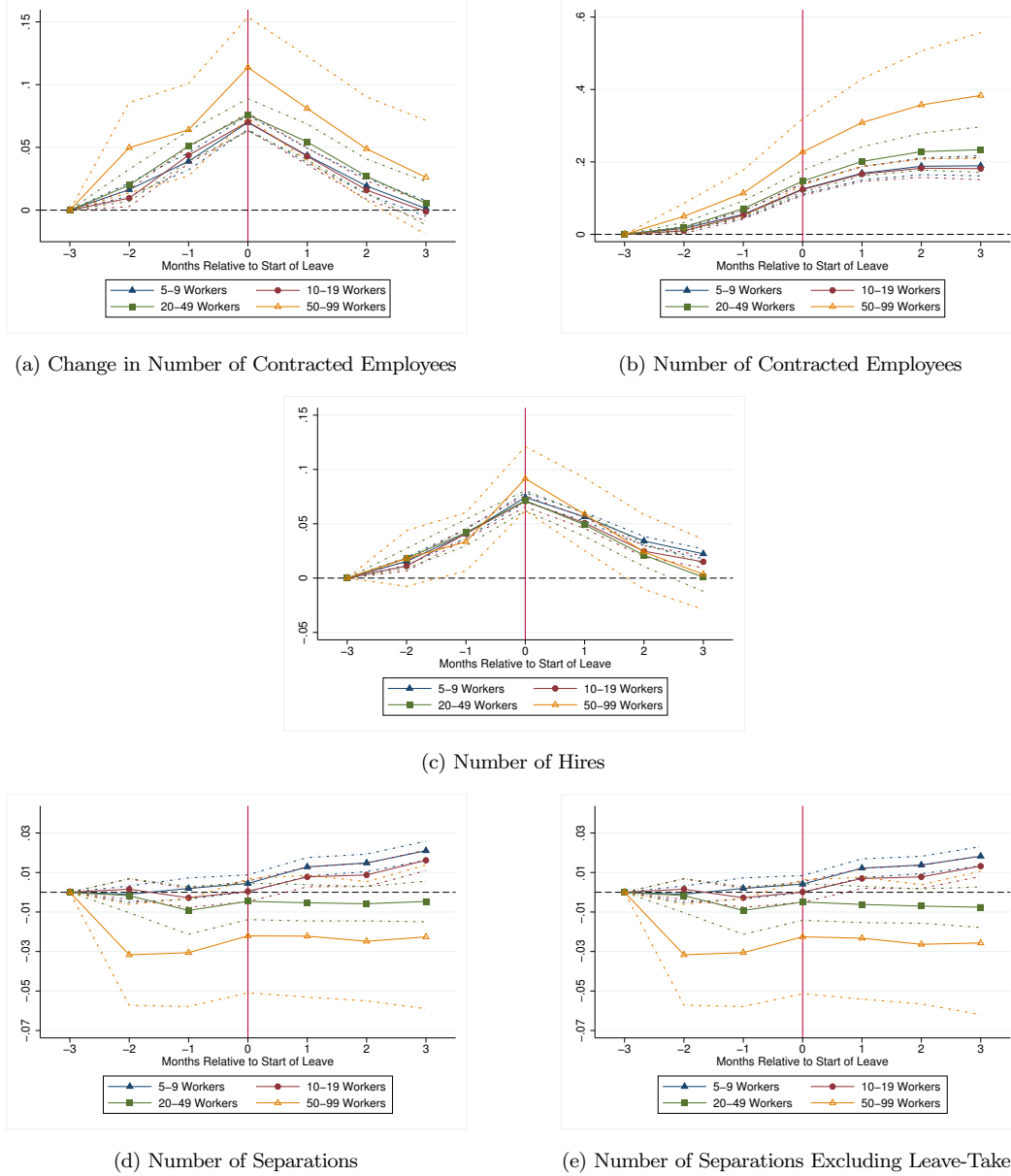
(c) Number of Hires

(d) Number of Separations

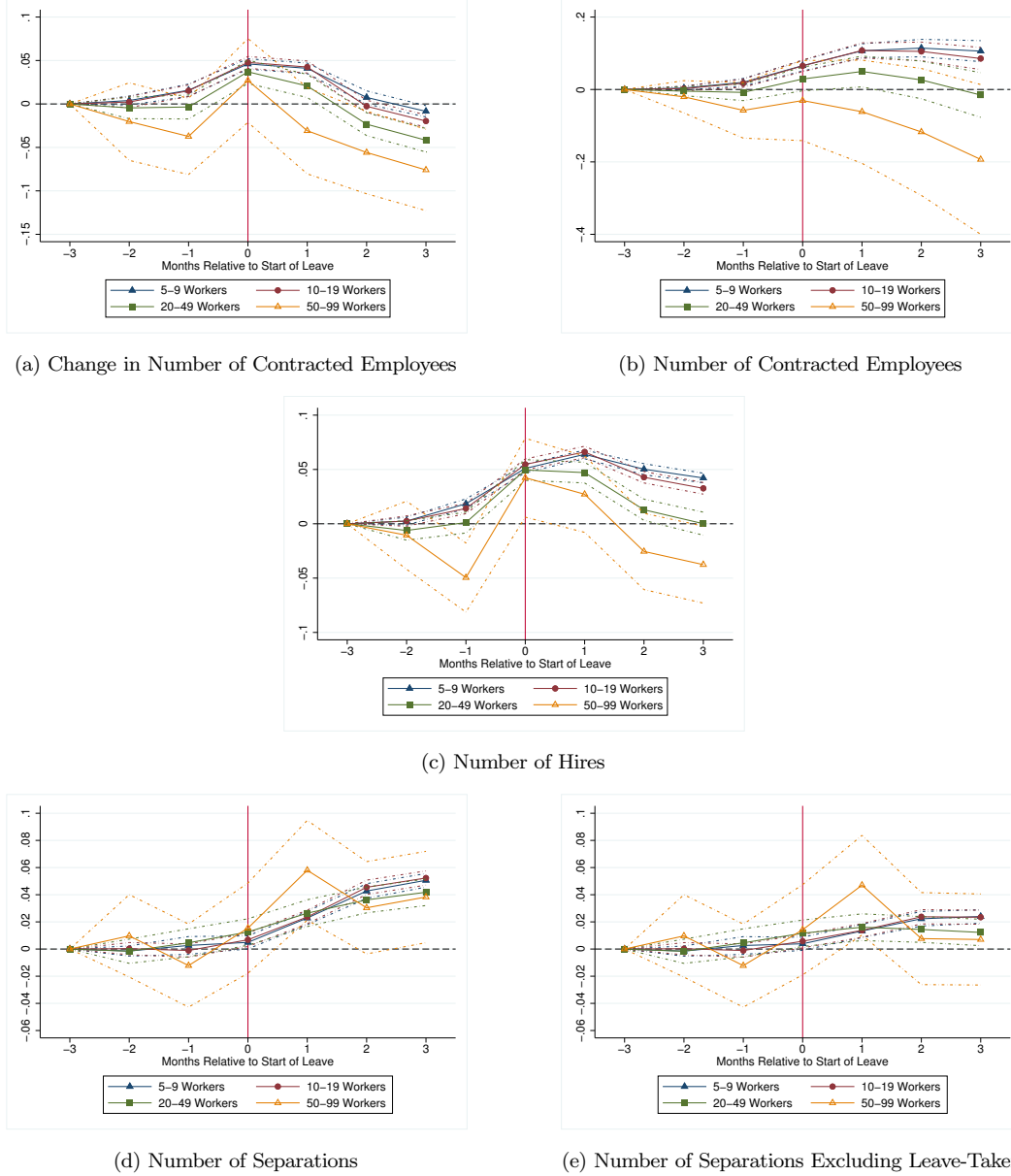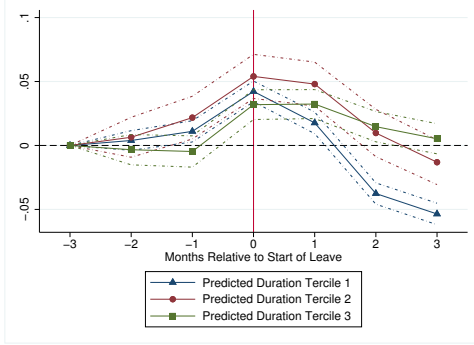(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ for each coarse occupation group (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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

Figure 6: 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) 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 from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ for each coarse occupation group (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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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

(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations
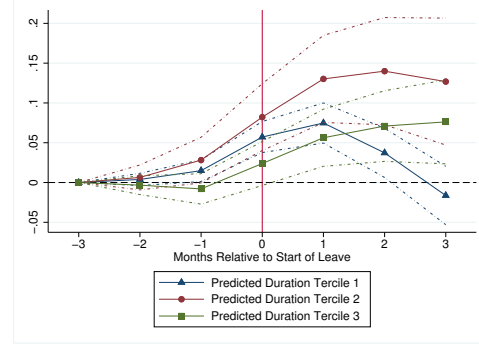
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ with the thickness of the local labor market. Labor market thickness is defined as the relative market share of the occupation (of the leave-taker) in the local labor market, and we group the thickness measures into terciles. 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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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


(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees
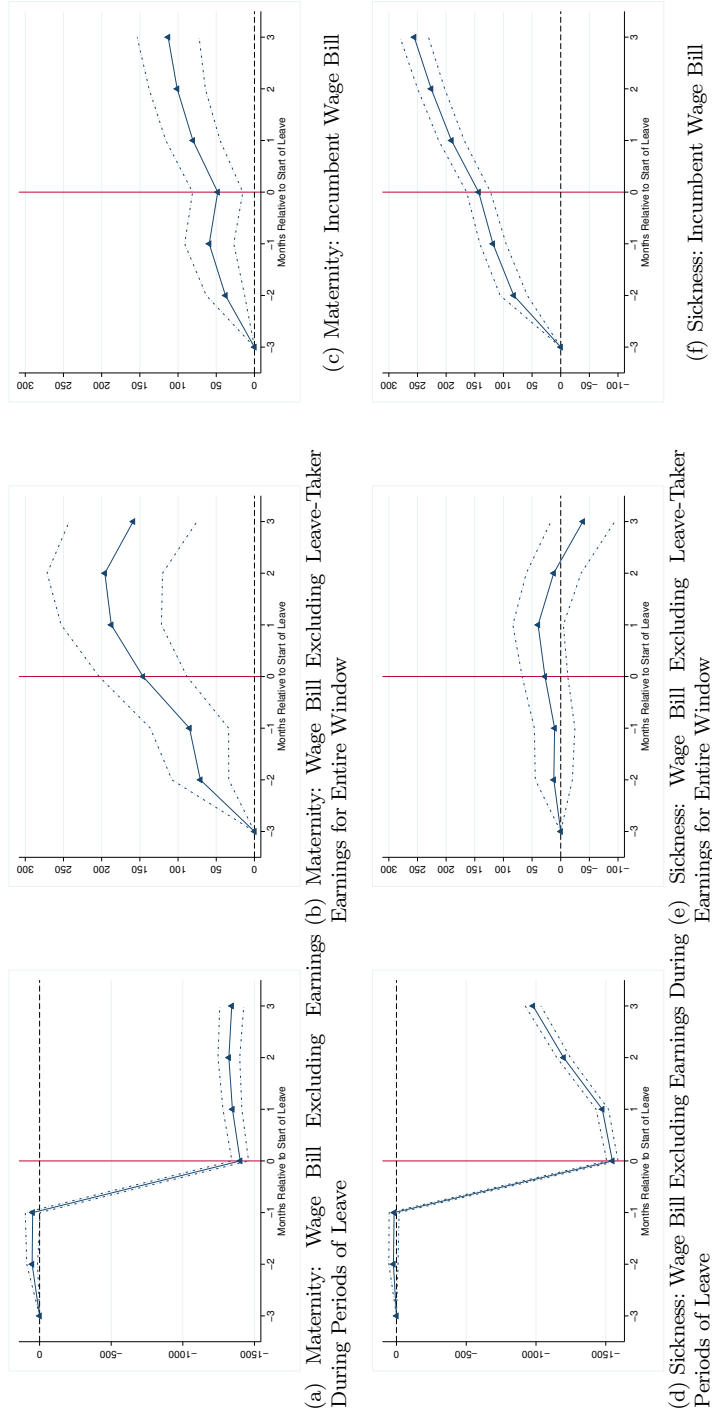
(c) Number of Hires

(d) Number of Separations

(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ with the thickness of the local labor market. Labor market thickness is defined as the relative market share of the occupation (of the leave-taker) in the local labor market, and we group the thickness measures into terciles. 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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Figure 9: 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) 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 from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ with the tenure of the leave-taker (measured at the start of their leave), grouping tenure into terciles. The first tercile corresponds to 13 months or less; the second tercile corresponds to between 14 and 31 months; and, the third tercile corresponds to more than 31 months. 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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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Figure 10: 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) 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 from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ with the tenure of the leave-taker (measured at the start of their leave), grouping tenure into terciles. The first tercile corresponds to 15 months or less; the second tercile corresponds to between 16 and 42 months; and, the third tercile corresponds to more than 42 months. 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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

(a) Change in Number of Contracted Employees

(b) 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 from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ by plant size category. We assign establishments to size categories based on their modal end-of-calendar year size from 2012–2017. We omit plants with a modal size of less than 5 workers (3% of leave spells) or greater than 99 workers (less than 5% of leave spells). 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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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

(a) Change in Number of Contracted Employees

(b) 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 from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ by plant size category. We assign establishments to size categories based on their modal end-of-calendar year size from 2012–2017. We omit plants with a modal size of less than 5 workers (3% of leave spells) or greater than 99 workers (less than 5% of leave spells). 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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

(a) Change in Number of Contracted Employees
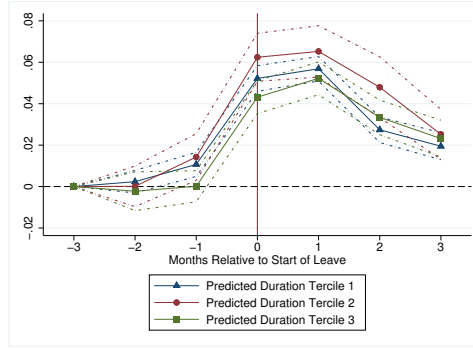
(b) Number of Contracted Employees

(c) Number of Hires
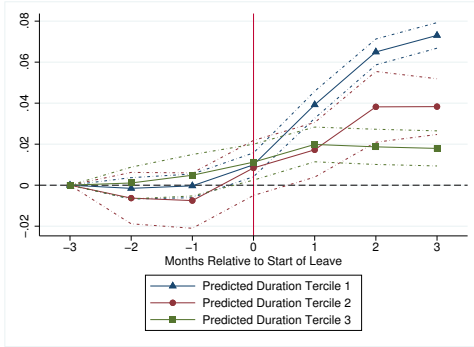
(d) Number of Separations
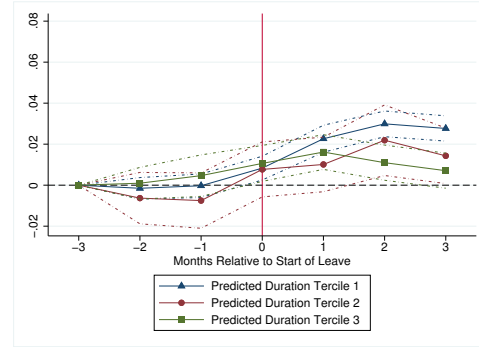
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from an augmented version of equation 1 where the employment dynamics (i.e., the β_k coefficients) differ with predicted leave duration, grouping predicted duration into terciles. Tercile 1 corresponds to 105 days or less; tercile 2 corresponds to between 106 and 151 days; and, tercile 3 corresponds to more than 151 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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

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

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

(c) Maternity: Incumbent Wage Bill

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

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

(f) Sickness: Incumbent Wage Bill

Note: The panels display regression coefficients 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 and plant-occupation-spell fixed effects are included rather than plant-occupation 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 occupation groups in the same plant as the leave-taker during the event window. Incumbents are defined as those contracted with the plant-occupation throughout the event window.

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Appendix Tables and Figures

Table A1: Descriptive Statistics of Plants in 2015

	Maternity		Sickness		All
	Clean	All	Clean	All	Plants
	(1)	(2)	(3)	(4)	(5)
Industry					
Agriculture/Forestry/Fishing	0.017	0.016	0.042	0.040	0.044
Mining	0.002	0.002	0.004	0.005	0.004
Manufacturing	0.128	0.139	0.158	0.183	0.137
Utilities: Electric/Gas	0.001	0.001	0.002	0.002	0.002
Utilities: Water/Sewage/Waste	0.003	0.003	0.004	0.006	0.004
Construction	0.020	0.018	0.042	0.054	0.048
Wholesale/Retail Trade	0.403	0.380	0.346	0.312	0.363
Transportation/Storage/Mail	0.030	0.029	0.051	0.061	0.047
Accommodation/Food	0.105	0.100	0.085	0.077	0.083
Information/Communication	0.017	0.018	0.016	0.015	0.018
Financial Services	0.039	0.038	0.034	0.031	0.026
Real Estate	0.005	0.004	0.004	0.004	0.005
Professional/Scientific/Technical	0.038	0.035	0.028	0.023	0.031
Administrative Activities	0.052	0.058	0.072	0.073	0.083
Public Admin/Defense/Social Security	0.000	0.000	0.000	0.000	0.000
Education	0.057	0.066	0.041	0.040	0.038
Health/Social Services	0.038	0.047	0.027	0.034	0.026
Art/Culture/Sports	0.009	0.008	0.008	0.007	0.009
Other Service Activities	0.037	0.036	0.035	0.032	0.033
# of Contracted Workers					
1 to 4	0.039	0.032	0.043	0.034	0.103
5 to 9	0.278	0.204	0.285	0.191	0.386
10 to 19	0.327	0.267	0.337	0.259	0.273
20 to 49	0.249	0.259	0.259	0.270	0.156
50 to 99	0.072	0.113	0.061	0.122	0.045
100 to 249	0.028	0.076	0.013	0.079	0.024
250 to 499	0.005	0.027	0.001	0.026	0.007
500 to 999	0.001	0.013	0.000	0.011	0.003
1000+	0.000	0.009	0.000	0.008	0.002
Female Employee Share	0.570	0.583	0.419	0.412	0.420
	(0.276)	(0.264)	(0.307)	(0.300)	(0.320)
Female Aged 21–35 Employee Share	0.326	0.333	0.211	0.207	0.219
	(0.212)	(0.203)	(0.200)	(0.189)	(0.220)
# of Plants	158,387	277,931	157,613	339,847	1,251,121

Note: All statistics are measured as of the end of 2015. Columns (1) and (3) include plants with clean maternity and sickness leave spells in 2015, respectively. Columns (2) and (4) include plants with maternity and sickness leave spells in 2015, respectively, regardless of whether they meet the clean definition as long as the other sample selection criteria are met. Column (5) includes plants in 2015 regardless of whether they experienced a leave as long as the other sample selection criteria are met. Standard deviations of non-categorical variables are reported in parentheses.

Table A2: 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}	0.0159***	0.0152***	-0.000765	-0.000752
	(0.00423)	(0.00282)	(0.00318)	(0.00318)
β_{-1}	0.0471***	0.0403***	-0.00678**	-0.00677**
	(0.00438)	(0.00312)	(0.00333)	(0.00333)
β_0	0.0775***	0.0753***	-0.00226	-0.00261
	(0.00462)	(0.00314)	(0.00338)	(0.00338)
β_1	0.0496***	0.0522***	0.00265	0.00182
	(0.00453)	(0.00326)	(0.00326)	(0.00326)
β_2	0.0282***	0.0334***	0.00527	0.00410
	(0.00568)	(0.00453)	(0.00369)	(0.00369)
β_3	0.00550	0.0159***	0.0104***	0.00750*
	(0.00511)	(0.00350)	(0.00401)	(0.00401)
R^2	0.113	0.406	0.405	0.405
N	5,417,769	5,417,769	5,417,769	5,417,769
Panel B: Sickness Leave				
β_{-2}	0.00171	0.000246	-0.00146	-0.00150
	(0.00320)	(0.00235)	(0.00224)	(0.00224)
β_{-1}	0.00728**	0.00749***	0.000205	0.000168
	(0.00340)	(0.00223)	(0.00266)	(0.00266)
β_0	0.0408***	0.0508***	0.0100***	0.00899***
	(0.00333)	(0.00234)	(0.00251)	(0.00251)
β_1	0.0289***	0.0568***	0.0279***	0.0179***
	(0.00334)	(0.00237)	(0.00258)	(0.00258)
β_2	-0.00929***	0.0335***	0.0428***	0.0213***
	(0.00348)	(0.00257)	(0.00279)	(0.00278)
β_3	-0.0239***	0.0220***	0.0460***	0.0174***
	(0.00343)	(0.00256)	(0.00260)	(0.00258)
R^2	0.108	0.403	0.407	0.408
N	5,730,662	5,730,662	5,730,662	5,730,662

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 A3: 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	0.020	0.006	0.014	0.016	0.051	0.011	0.029	0.032
Mining	0.001	0.002	0.003	0.003	0.004	0.004	0.006	0.005
Manufacturing	0.108	0.142	0.155	0.171	0.140	0.191	0.187	0.187
Utilities: Electric/Gas	0.001	0.001	0.001	0.002	0.002	0.002	0.002	0.001
Utilities: Water/Sewage/Waste	0.003	0.003	0.003	0.002	0.005	0.004	0.003	0.002
Construction	0.014	0.013	0.022	0.038	0.039	0.030	0.043	0.052
Wholesale/Retail Trade	0.396	0.334	0.387	0.463	0.338	0.295	0.345	0.424
Transportation/Storage/Mail	0.028	0.031	0.035	0.032	0.054	0.052	0.049	0.035
Accommodation/Food	0.109	0.091	0.094	0.100	0.084	0.072	0.081	0.099
Information/Communication	0.019	0.022	0.017	0.012	0.016	0.017	0.015	0.012
Financial Services	0.043	0.035	0.046	0.017	0.035	0.029	0.039	0.017
Real Estate	0.006	0.008	0.004	0.003	0.004	0.006	0.004	0.003
Professional/Scientific/Technical	0.046	0.051	0.026	0.013	0.033	0.034	0.019	0.012
Administrative Activities	0.060	0.063	0.045	0.032	0.080	0.106	0.060	0.032
Public Admin/Defense/Social Security	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Education	0.059	0.076	0.058	0.040	0.040	0.051	0.045	0.037
Health/Social Services	0.037	0.055	0.048	0.030	0.025	0.034	0.034	0.024
Art/Culture/Sports	0.010	0.009	0.008	0.006	0.008	0.009	0.008	0.006
Other Service Activities	0.041	0.057	0.034	0.020	0.040	0.050	0.032	0.019
# of Workers on Dec 31								
0	0.010	0.007	0.006	0.006	0.005	0.003	0.004	0.004
1 to 4	0.050	0.045	0.037	0.032	0.053	0.053	0.046	0.041
5 to 9	0.334	0.260	0.201	0.154	0.344	0.294	0.223	0.171
10 to 19	0.333	0.328	0.299	0.261	0.341	0.335	0.328	0.300
20 to 49	0.210	0.266	0.301	0.305	0.212	0.255	0.306	0.339
50 to 99	0.047	0.071	0.103	0.134	0.038	0.051	0.077	0.112
100 to 249	0.013	0.019	0.043	0.080	0.006	0.008	0.016	0.031
250 to 499	0.002	0.002	0.008	0.020	0.000	0.000	0.001	0.002
500 to 999	0.000	0.001	0.002	0.006	0.000	0.000	0.000	0.000
1000+	0.000	0.000	0.001	0.002	0.000	0.000	0.000	0.000
# of Leaves	483,173	24,676	155,821	110,297	511,430	28,961	173,076	105,199

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 between 2003–2011. In columns (2) and (6), spells are associated with plants that were inspected at some point between 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 between 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 between 2003–2011, and informal workers were present.

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Figure A1: Kaplan-Meier Survival Functions for Maternity and Sickness Leave-Takers
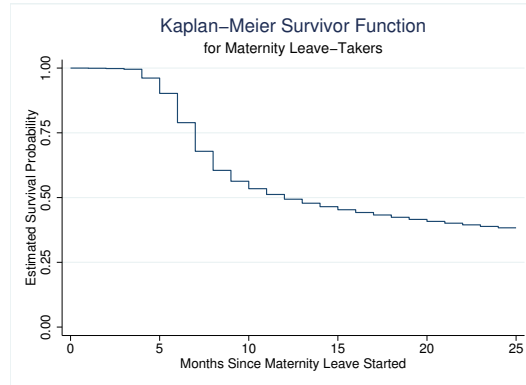
(a) Maternity Leave-Takers
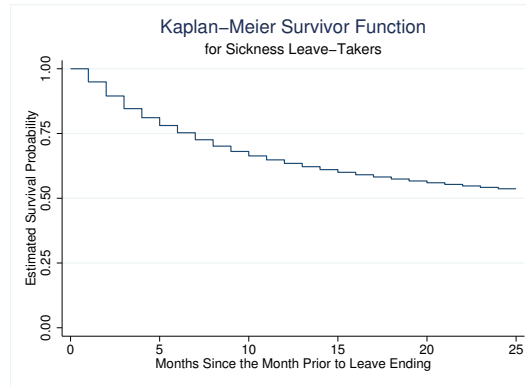
(b) Sickness Leave-Takers

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

Figure A2: Temporary Employment Dynamics around Leave Initiation in Occupation of Leave-Taker
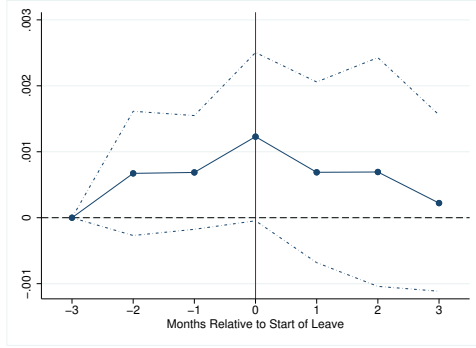
(a) Maternity: Change in Number of Temporary Employees
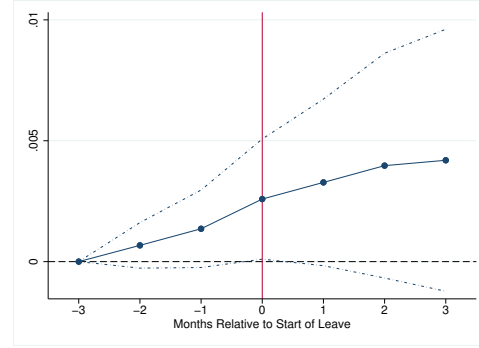
(b) Maternity: Number of Temporary Employees
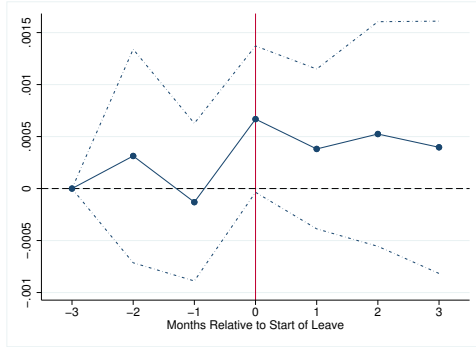
(c) Sickness: Change in Number of Temporary Employees
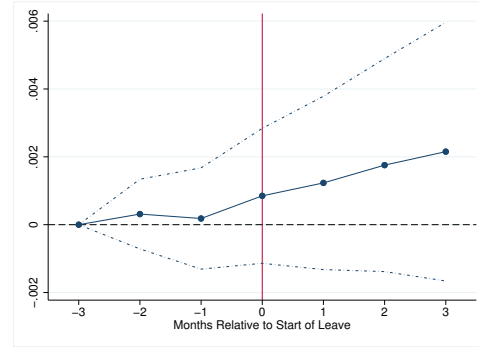
(d) Sickness: Number of Temporary Employees

Note: The panels display regression coefficients 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 the plant-occupation of the leave-taker during the event window. In panels (b) and (d), we show the number of temporary employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panels (a) and (c), respectively.

Figure A3: 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 Contracted Employees
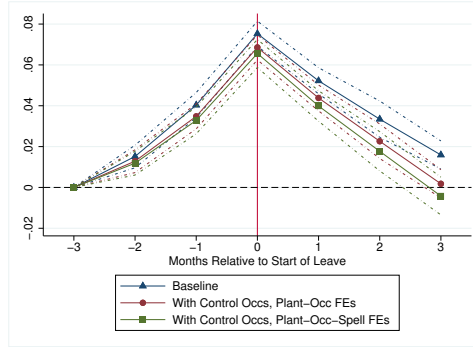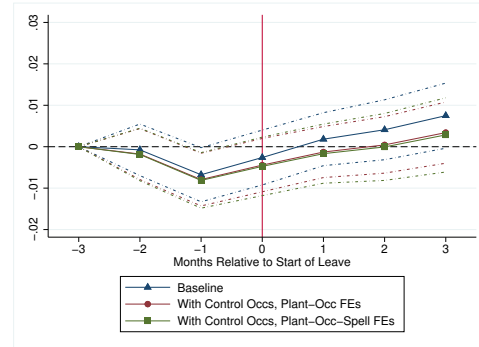
(e) Number of Contracted Employees

(f) Number of Contracted Employees

Note: The panels display regression coefficients 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 the non-maternity-leave-taking plant-occupation groups during the event window. In panels (d)–(f), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panels (a)–(c).

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Figure A4: Employment Dynamics around Sickness Leave Initiation in Non-Leave-Taking Occupations
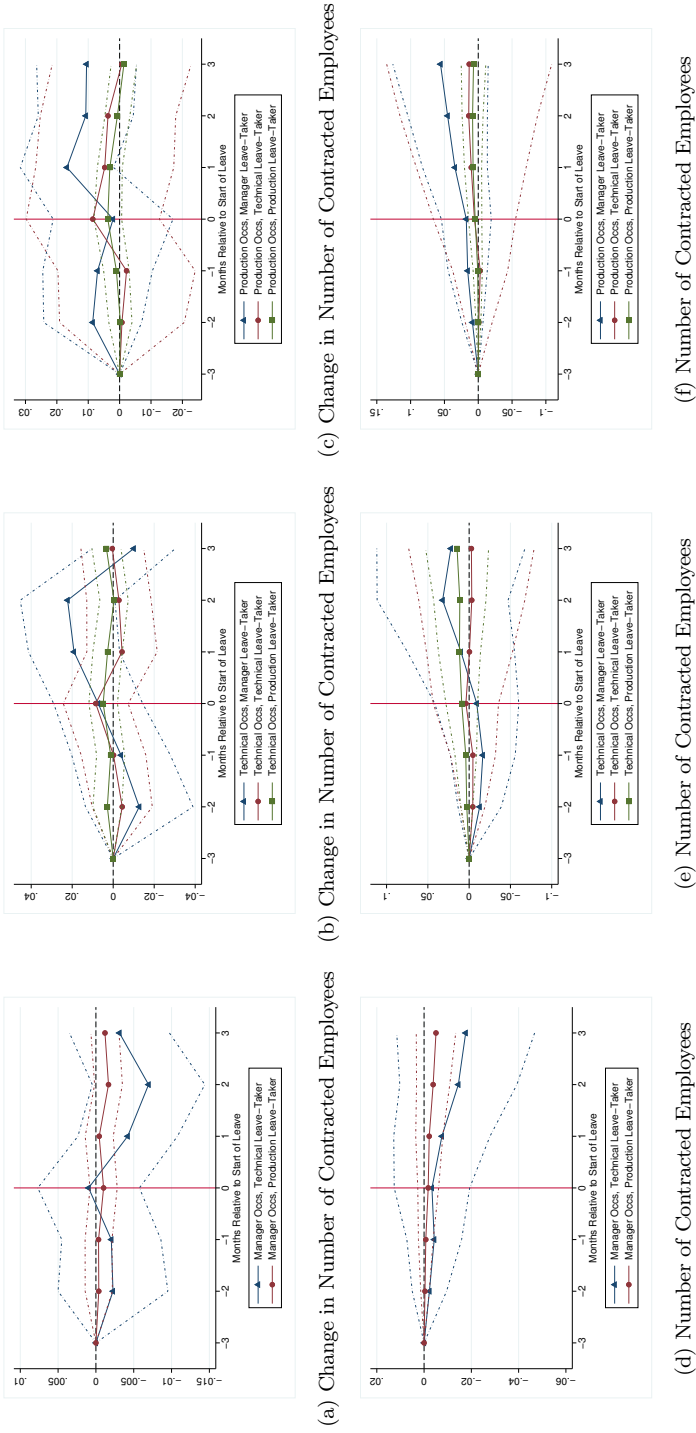
(a) Change in Number of Contracted Employees

(b) Change in Number of Contracted Employees

(c) Change in Number of Contracted Employees

(d) Number of Contracted Employees

(e) Number of Contracted Employees

(f) Number of Contracted Employees

Note: The panels display regression coefficients 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 the non-sickness-leave-taking plant-occupation groups during the event window. In panels (d)–(f), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panels (a)–(c).
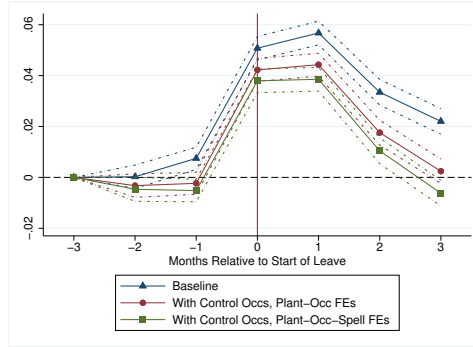
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Figure A5: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with Different Calendar Time Controls
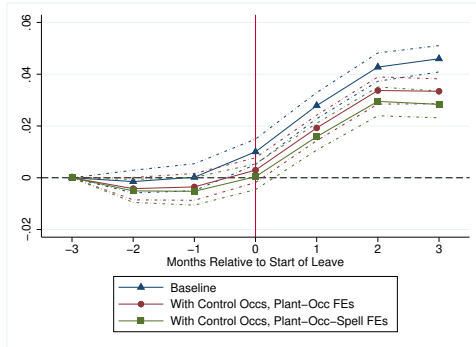

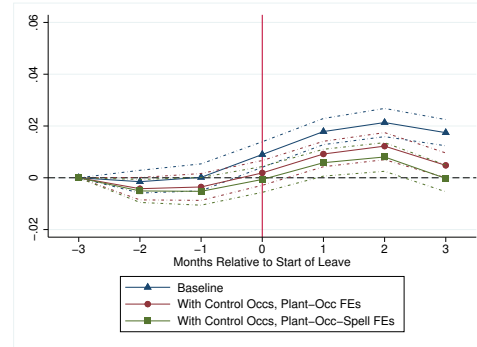(a) Change in Number of Contracted Employees
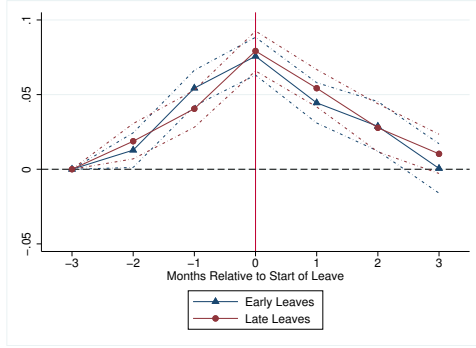

(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations

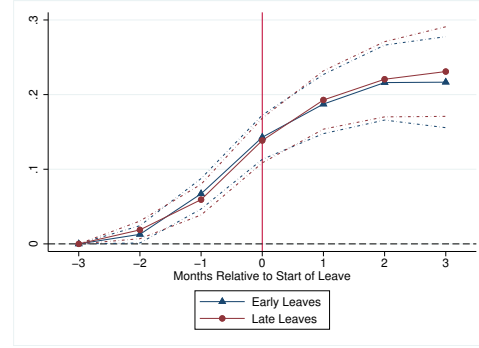(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from 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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

Figure A6: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with Different Calendar Time Controls

(a) Change in Number of Contracted Employees

(b) 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 from 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
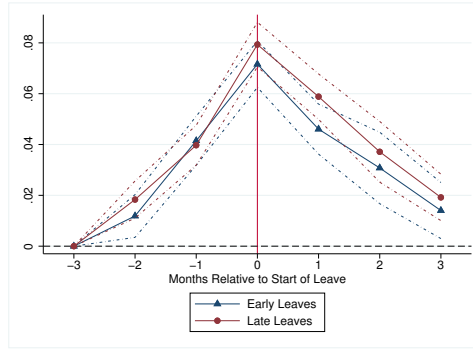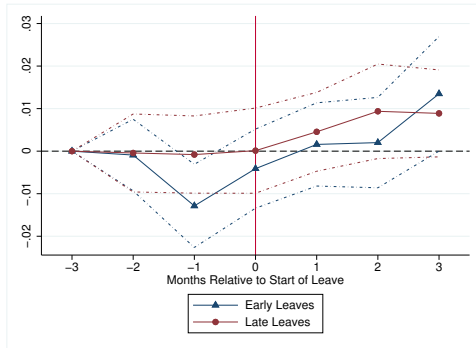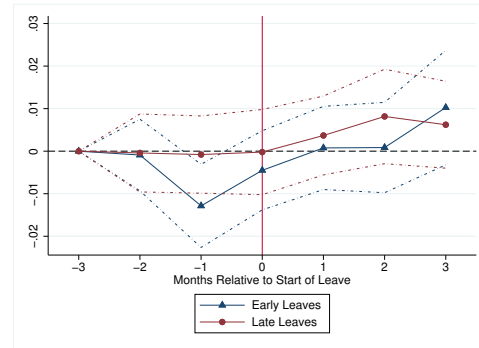
Figure A7: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker Including Control Groups

(a) Change in Number of Contracted Employees
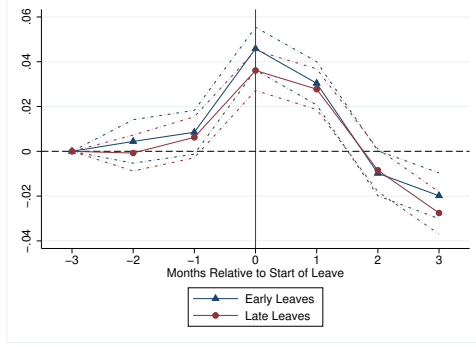
(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations

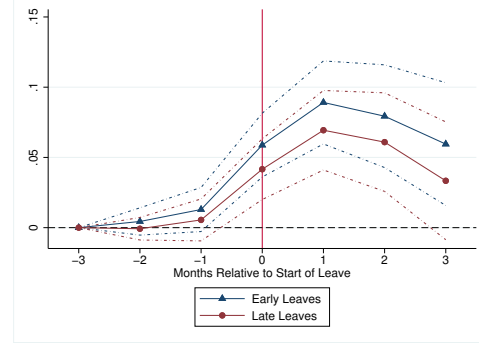(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients 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. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
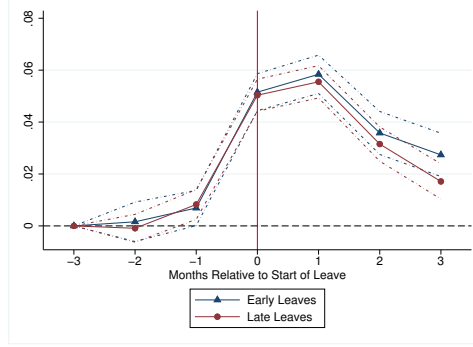
Figure A8: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Including Control Groups
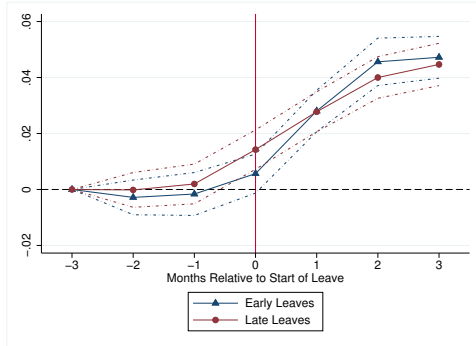

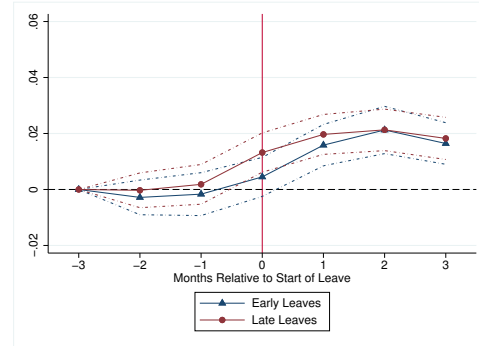(a) Change in Number of Contracted Employees

(b) 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 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. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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Figure A9: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker in Early and Later Years

(a) Change in Number of Contracted Employees

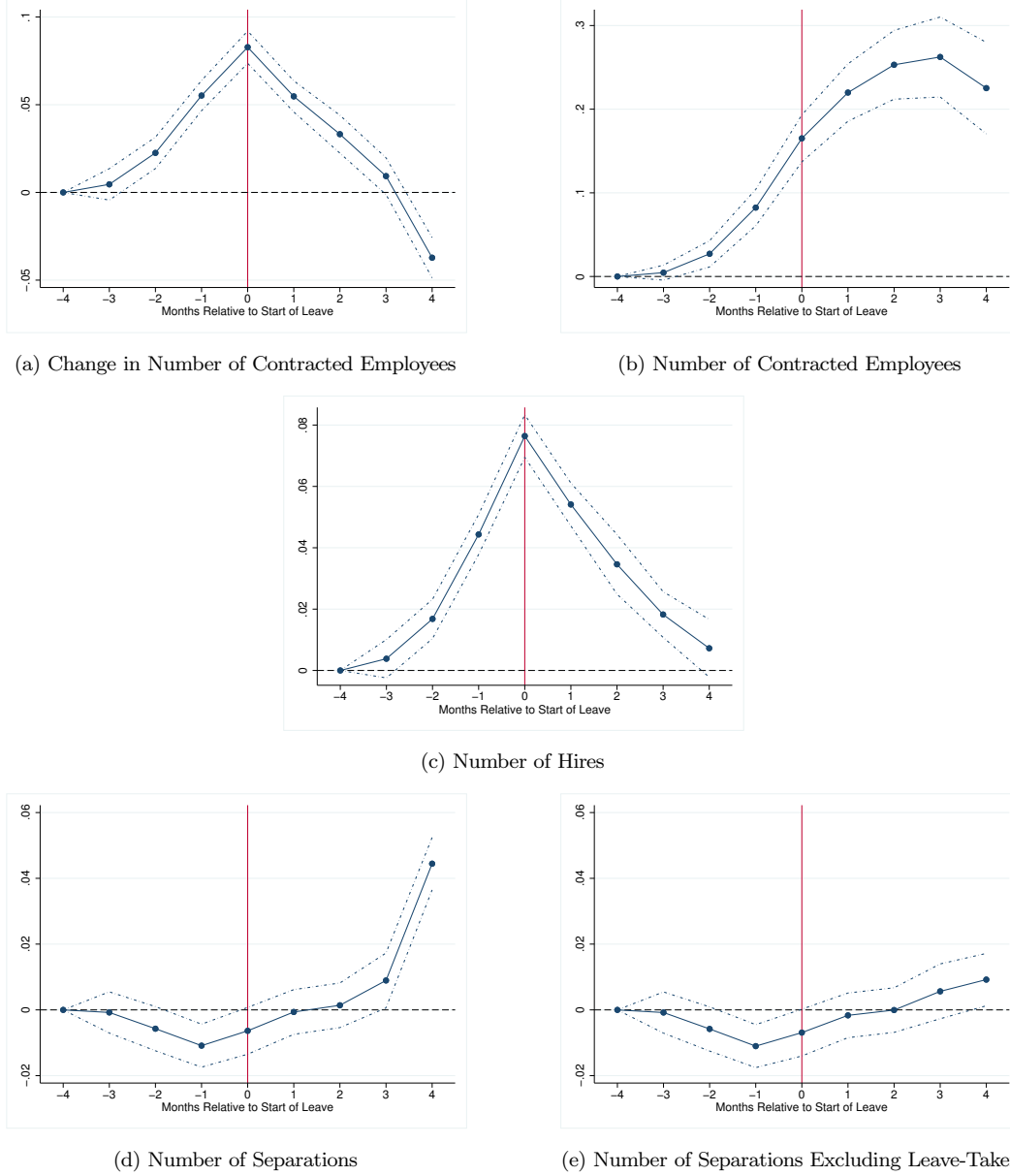(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations

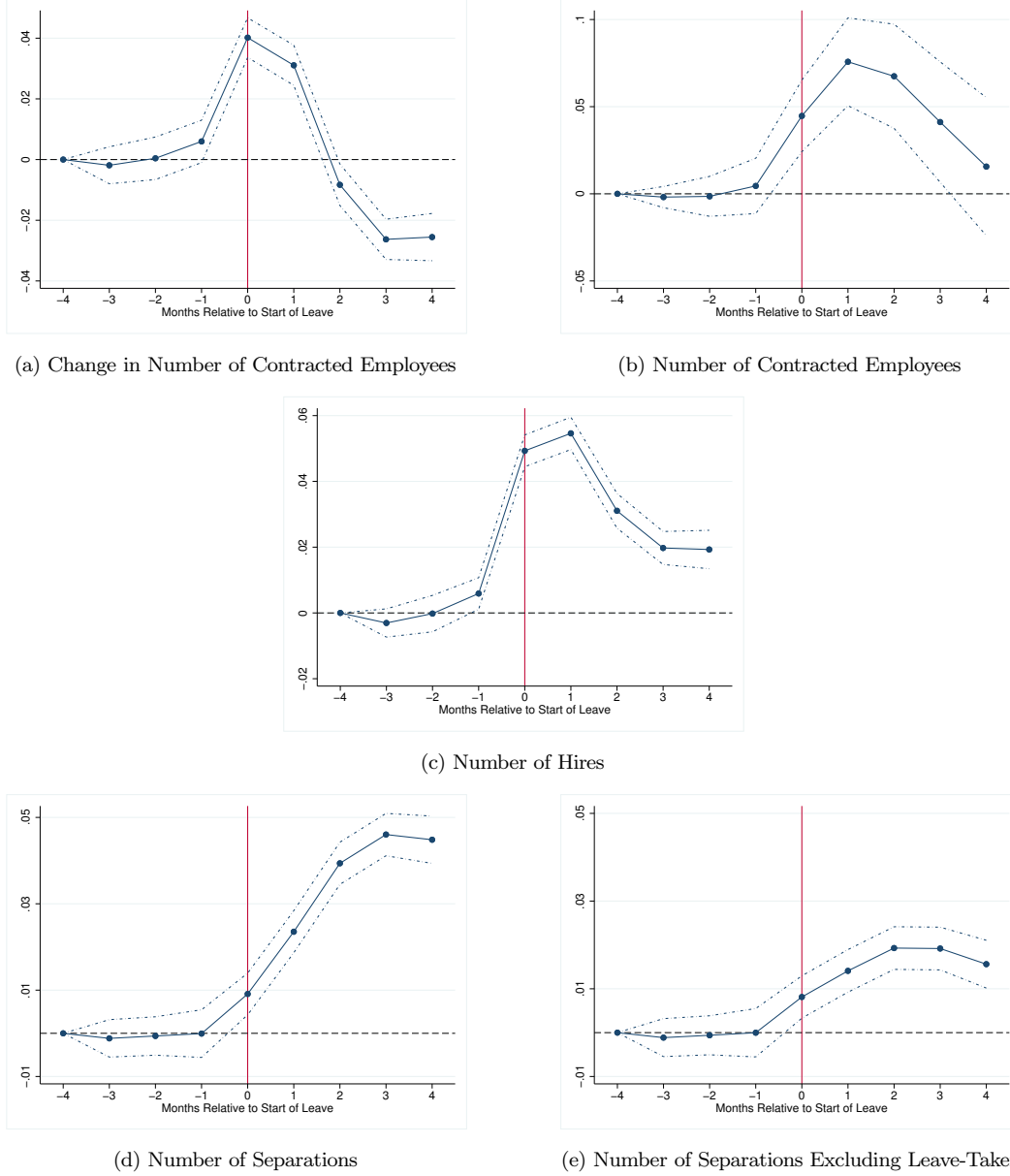(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from an augmented version of equation 1. We allow the employment dynamics (i.e., the β_k coefficients) to differ 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 the plant-occupation group of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

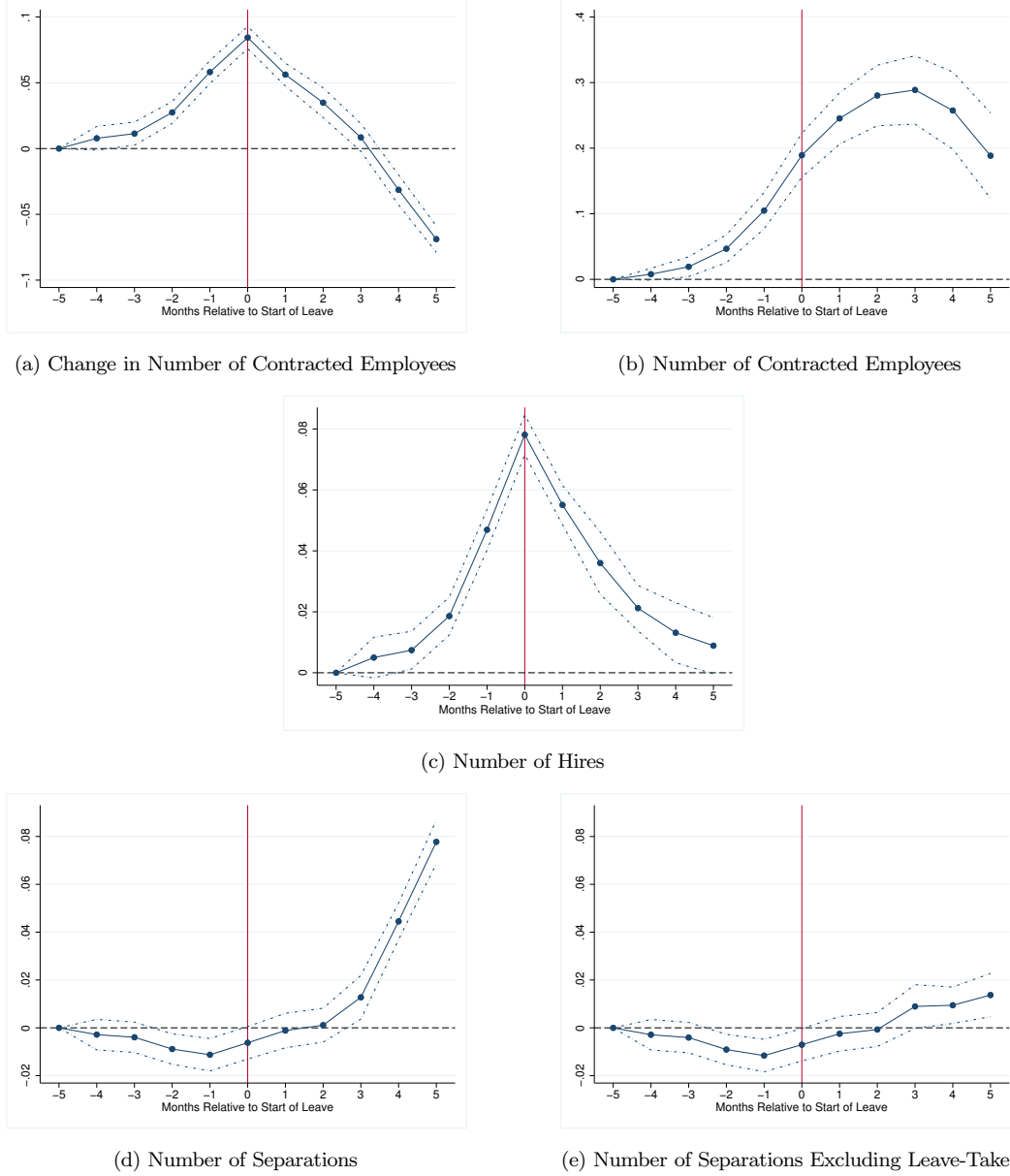Figure A10: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker in Early and Later Years

(a) Change in Number of Contracted Employees

(b) 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 from an augmented version of equation 1. We allow the employment dynamics (i.e., the β_k coefficients) to differ 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 the plant-occupation group of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta_k}$, using the β_k estimates from panel (a).

Figure A11: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 4-Month Event Window

(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations
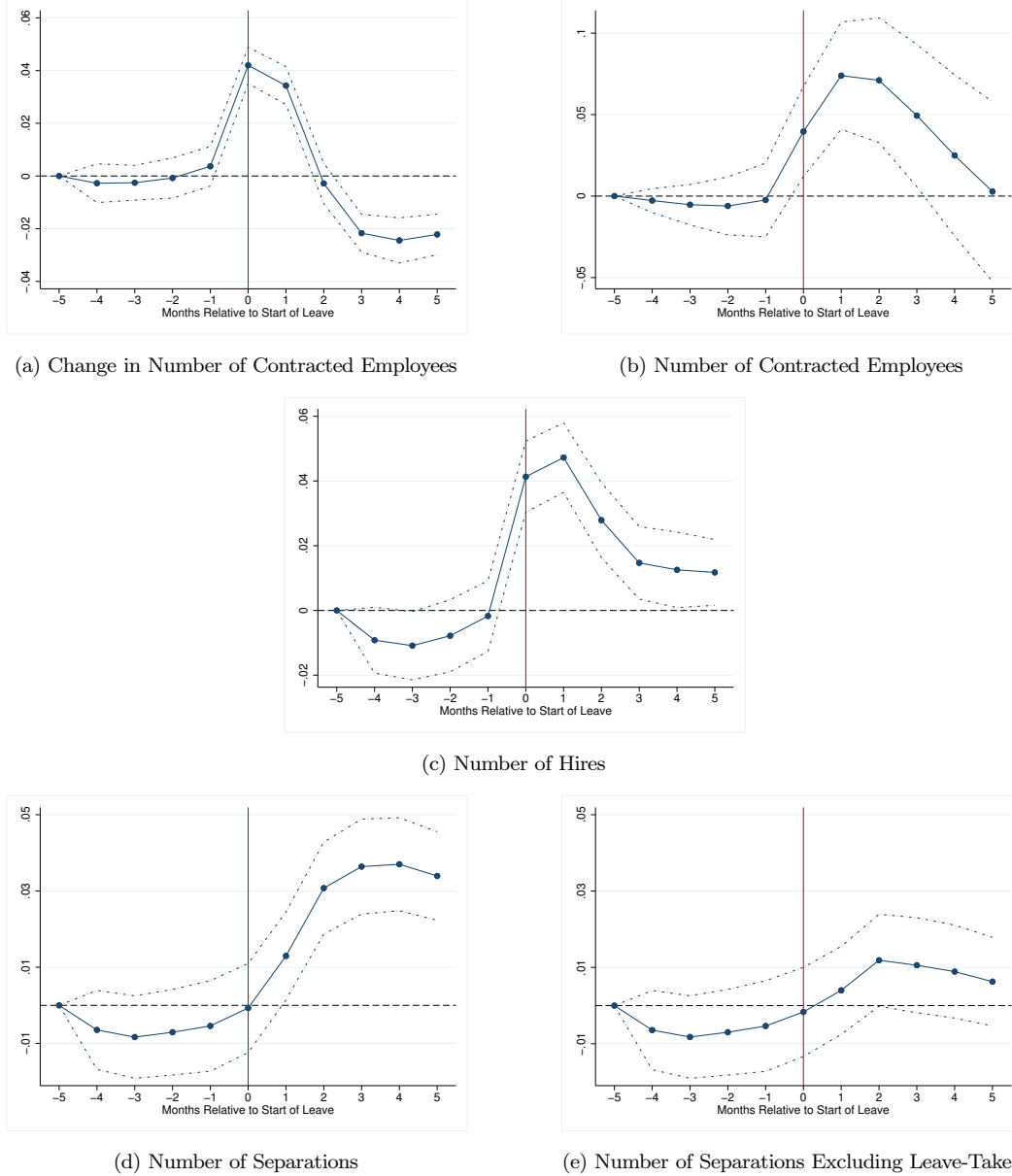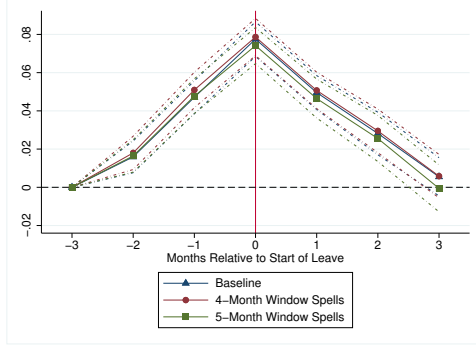
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients 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 the plant-occupation group of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-3, -2, -1, 0, 1, 2, 3, 4\}$ relative to four months prior to leave onset, obtained by computing $\sum_{k=-4}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

Figure A12: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 4-Month Event Window
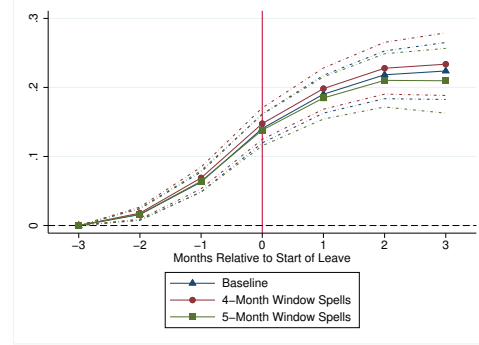

(a) Change in Number of Contracted Employees

(b) 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 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 the plant-occupation group of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-3, -2, -1, 0, 1, 2, 3, 4\}$ relative to four months prior to leave onset, obtained by computing $\sum_{k=-4}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
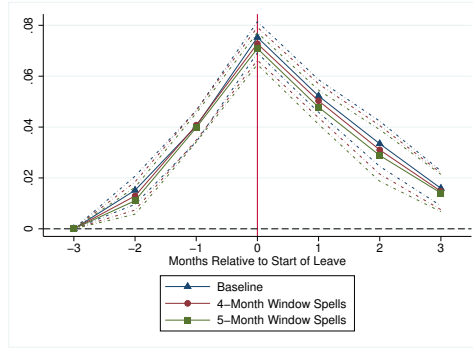
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Figure A13: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window
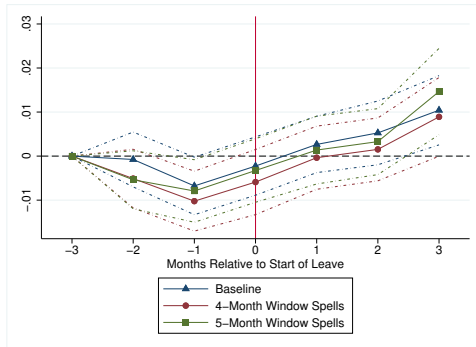

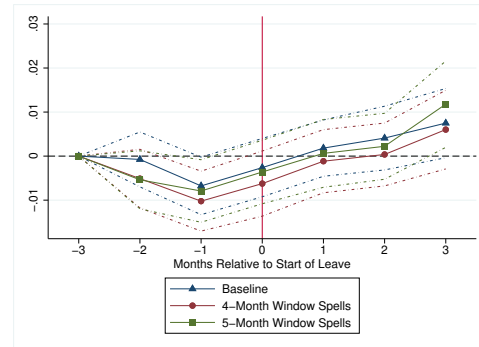(a) Change in Number of Contracted Employees

(b) 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 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 the plant-occupation group of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-4, -3, -2, -1, 0, 1, 2, 3, 4, 5\}$ relative to five months prior to leave onset, obtained by computing $\sum_{k=-5}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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Figure A14: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker with 5-Month Event Window

(a) Change in Number of Contracted Employees
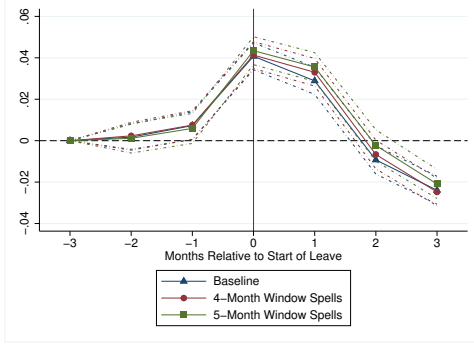
(b) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations

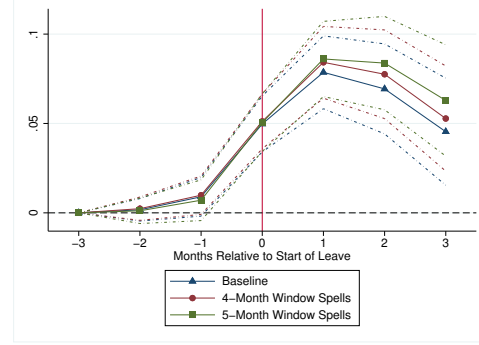(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients 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 the plant-occupation group of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-4, -3, -2, -1, 0, 1, 2, 3, 4, 5\}$ relative to five months prior to leave onset, obtained by computing $\sum_{k=-5}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
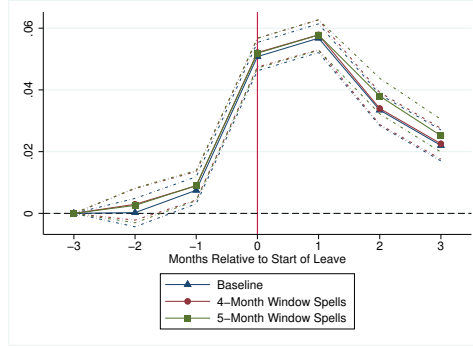
Figure A15: Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker with 3-Month Event Window Using 4-Month and 5-Month Window Spells
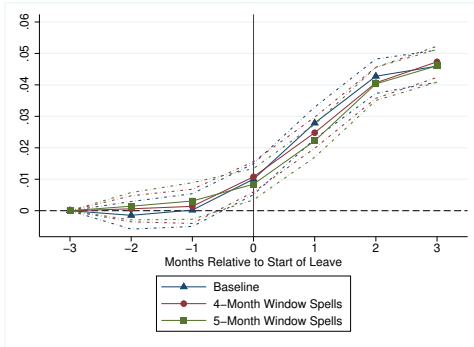

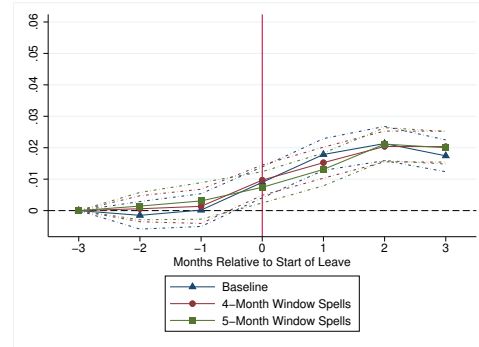(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees
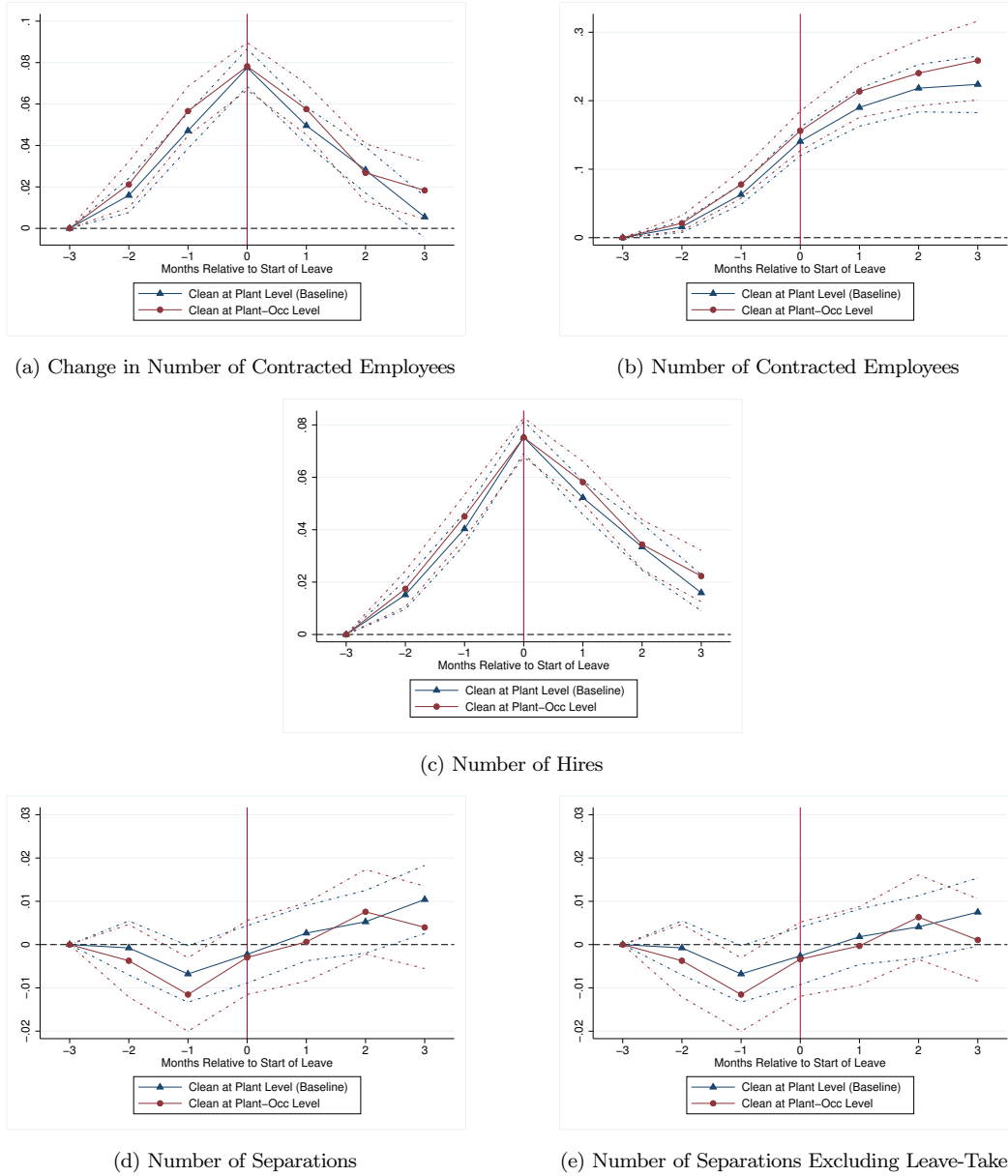
(c) Number of Hires

(d) Number of Separations

(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from equation 1 estimated separately on the 3-month, 4-month, and 5-month window samples. 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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

Figure A16: 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) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations
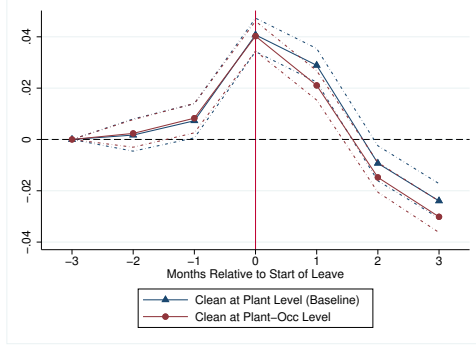

(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients from equation 1 estimated separately on the 3-month, 4-month, and 5-month window samples. 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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Figure A17: 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) Number of Contracted Employees

(c) Number of Hires

(d) Number of Separations
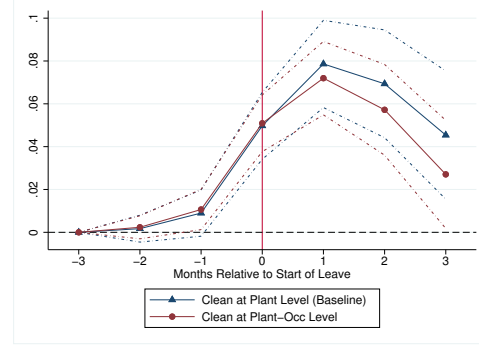
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients 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 the plant-occupation of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
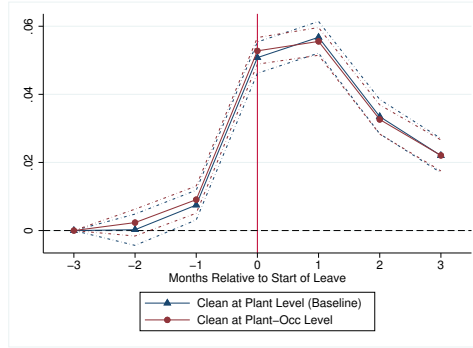
Figure A18: Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker Using a Relaxed Clean-Spell Definition
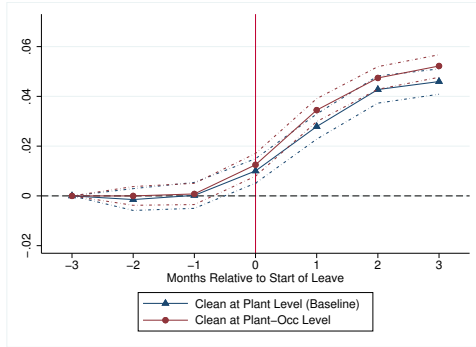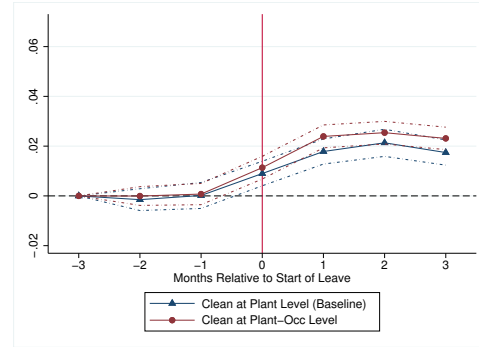

(a) Change in Number of Contracted Employees

(b) Number of Contracted Employees
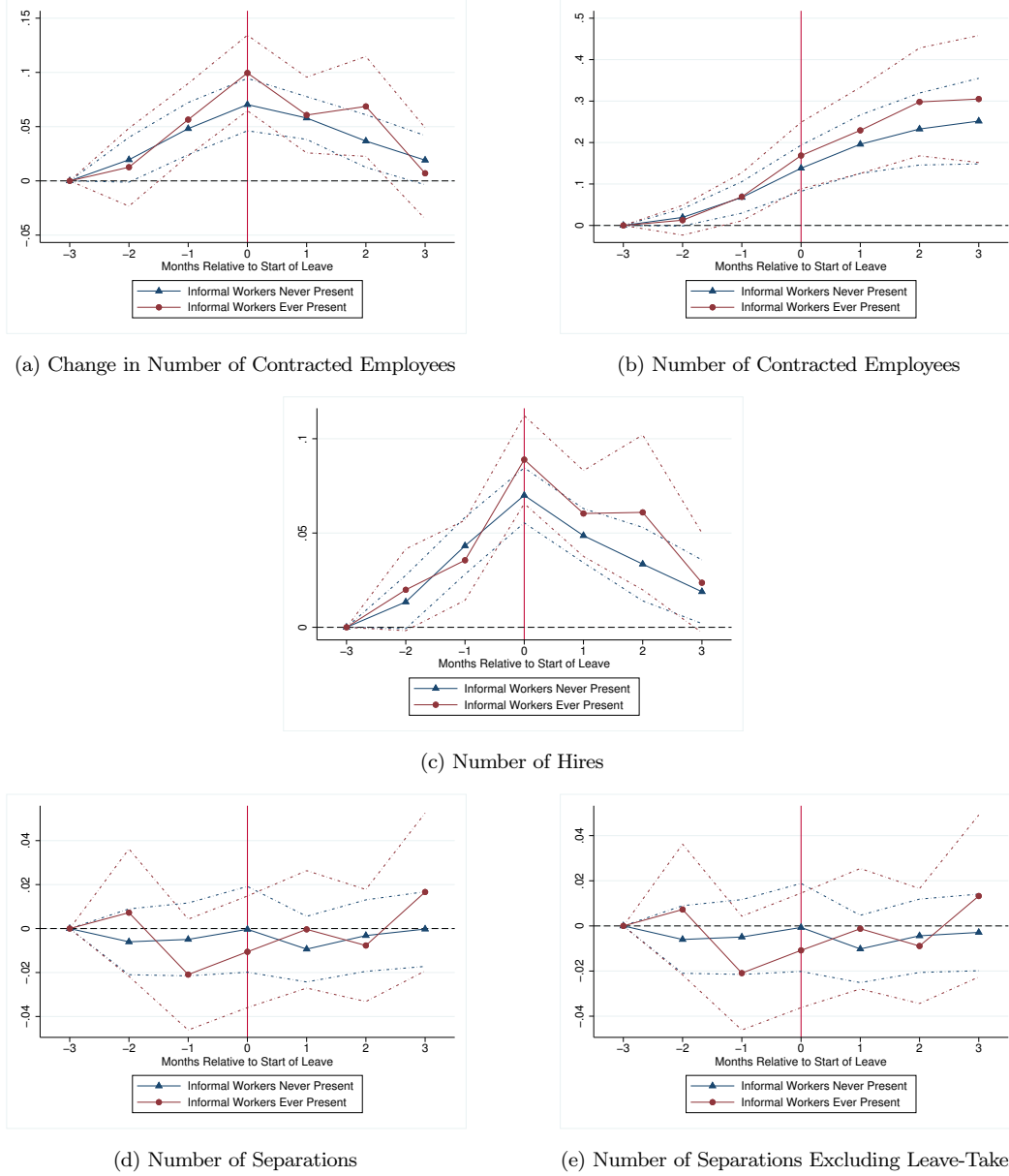
(c) Number of Hires

(d) Number of Separations
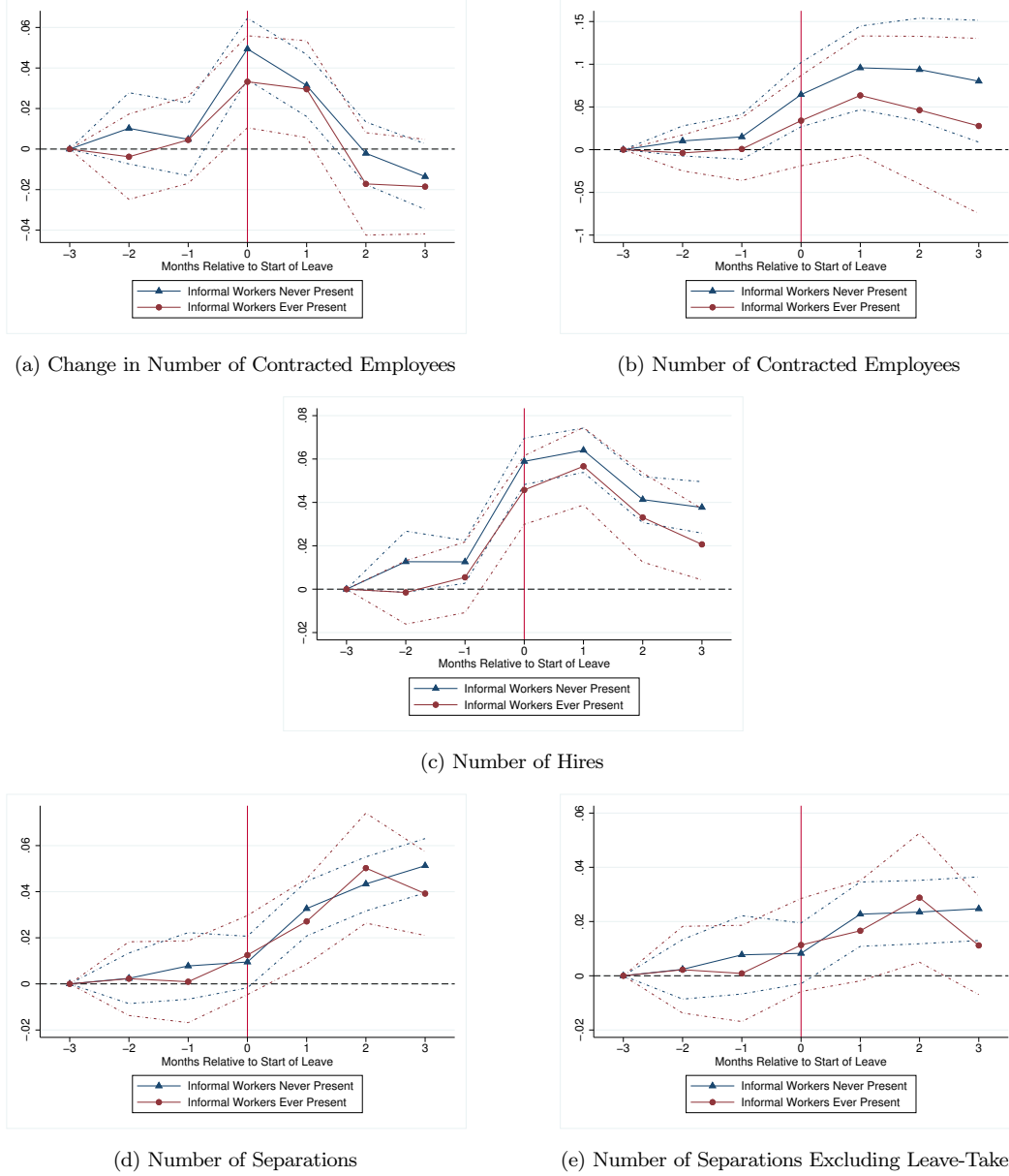
(e) Number of Separations Excluding Leave-Taker

Note: The panels display regression coefficients 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 the plant-occupation of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the $\hat{\beta}_k$ estimates from panel (a).

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Figure A19: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Inspection Status from 2003–2011
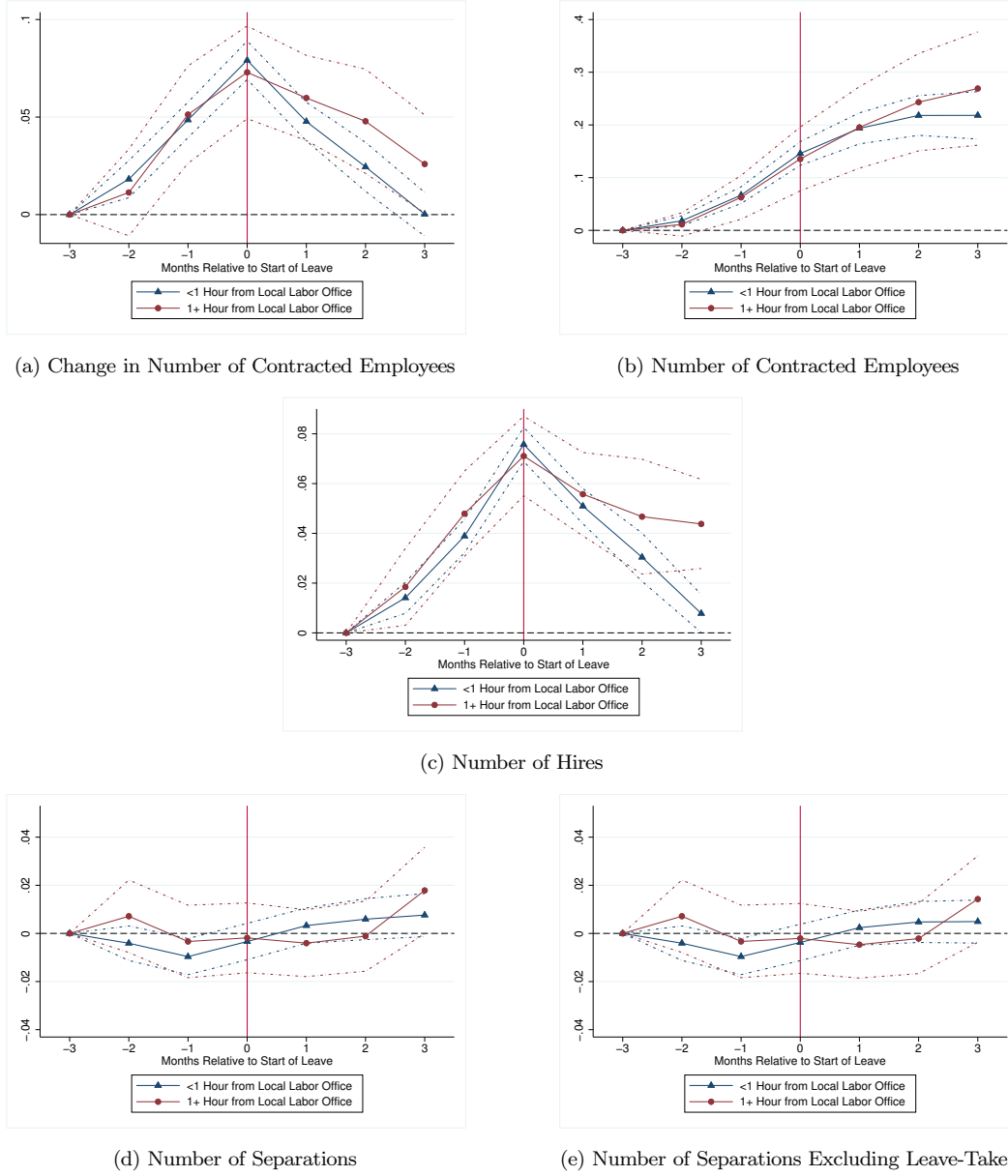

(a) Change in Number of Contracted Employees

(b) 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 from an augmented version of equation 1, where we allow the employment dynamics (i.e., the β_k coefficients) to differ with the plant's 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 the plant-occupation group of the maternity leave-taker during the event window and only plants that ever had their employee registration inspected between 2003–2011. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

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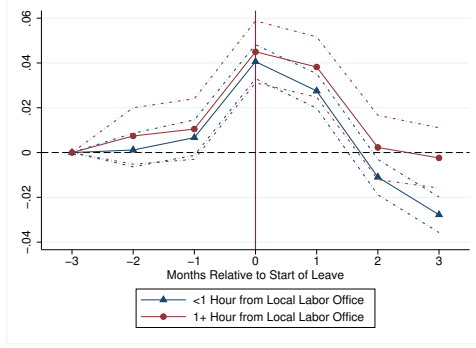

(a) Change in Number of Contracted Employees

(b) 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 from an augmented version of equation 1, where we allow the employment dynamics (i.e., the β_k coefficients) to differ with the plant's 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 the plant-occupation group of the sickness leave-taker during the event window and only plants that ever had their employee registration inspected between 2003–2011. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).

Figure A21: Heterogeneity in Employment Dynamics around Maternity Leave Initiation in Occupation of Leave-Taker by Driving Distance to Nearest Labor Enforcement Office
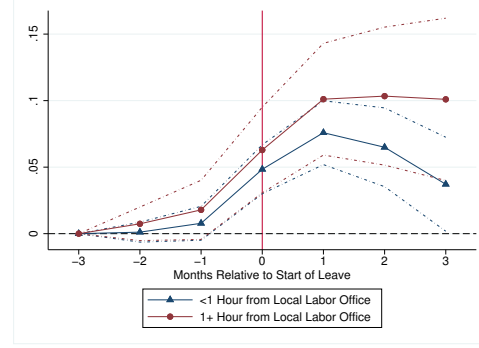

(a) Change in Number of Contracted Employees

(b) 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 from an augmented version of equation 1. We allow the employment dynamics (i.e., the β_k coefficients) to differ by whether the plant is located within one hour of the nearest local labor enforcement office. 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 the plant-occupation group of the maternity leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).
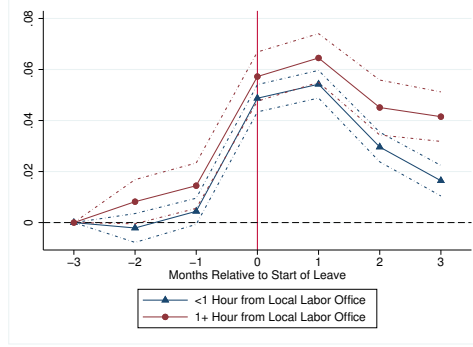
Figure A22: Heterogeneity in Employment Dynamics around Sickness Leave Initiation in Occupation of Leave-Taker by Driving Distance to Nearest Labor Enforcement Office
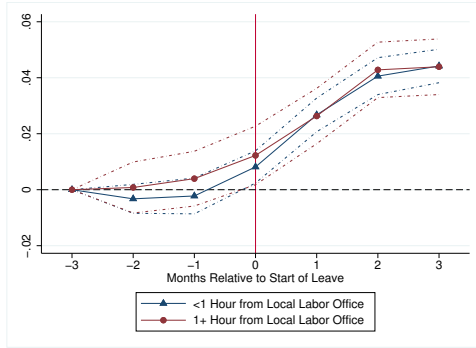

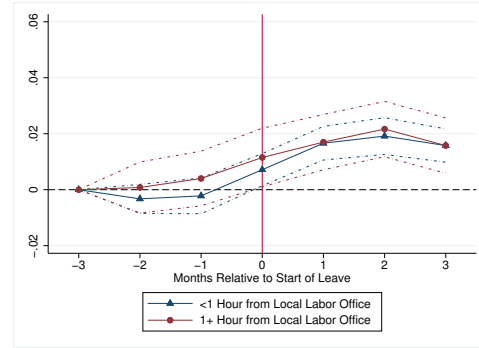(a) Change in Number of Contracted Employees

(b) 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 from an augmented version of equation 1. We allow the employment dynamics (i.e., the β_k coefficients) to differ by whether the plant is located within one hour of the nearest local labor enforcement office. 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 the plant-occupation group of the sickness leave-taker during the event window. In panel (b), we show the number of contracted employees in event month $i \in \{-2, -1, 0, 1, 2, 3\}$ relative to three months prior to leave onset, obtained by computing $\sum_{k=-3}^{i} \hat{\beta}_k$, using the β_k estimates from panel (a).