Bucks for Bumps

How Paid Parental Leave Pays Off for Firms*

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Abstract

Firms use benefits such as paid parental leave (PPL) to attract and retain employees. In the U.S., which lacks broad public PPL benefits, firms increasingly provide this benefit. Systematic evidence of its benefits remains limited, due in part to the absence of comprehensive data on firm policies. Using a novel, hand-collected database of firm policy changes, this paper estimates the causal effect of PPL expansion on employee retention using a differences-in-differences approach. PPL expansions reduce departure rates for both men and women by 3.5%. Retention gains for women are concentrated among the lowest paid and those in the most junior roles, while retention gains for men are broad-based. At the firm level, PPL expansions reduce departures most in firms with moderately flexible internal labor markets. These expansions also have stronger effects when bundled with other family-friendly benefits. Firm PPL expansions do not appear to yield differential retention effects in jurisdictions with public PPL benefits. Combining fertility estimates from the American Community Survey (ACS) with worker and firm data, the paper calculates the expected costs of PPL provision and compares them to retention gains: at realistic utilization rates, PPL expansions generate positive net returns to firms.

1 Introduction

In the competition for human capital, firms offer many benefits to attract and retain employees. Benefits are expensive: as of 2023, they constitute 30% of the total compensation of private industry workers in the U.S. (Bureau of Labor Statistics, 2025a). Yet, unlike wages, relatively little is known about whether benefits help to recruit or retain employees, or to increase their productivity.

Paid parental leave (PPL) is an increasingly prominent component of the benefits landscape for U.S. firms. While the U.S. lags other developed countries in the provision of universal public PPL, the share of private-sector workers with access to employer-based PPL has increased from only 2% in 1997 to 27% in 2023 (Bureau of Labor Statistics, National Compensation Survey, various years). This growth reflects increased provision across the firm size distribution (Society for Human Resource Management, 2019-2025).

Some firms have reported large reductions in employee attrition following expansions of their PPL policies. For example, Google expanded PPL from 12 to 18 weeks in 2007 and reported a 50% reduction in the attrition of new mothers (Wojcicki, 2014). Similarly, Accenture reported a 40% decline in the attrition of new mothers following an expansion of PPL from eight to 16 weeks in 2015 (Vanderkam, 2016). Despite these prominent claims, the broader retention effects of private firm PPL have not been systematically established in academic research.

In this paper, I examine whether and when expansions of employer-provided PPL improve employee retention, how the strength of retention effects varies across workers, firms, policy design features, and institutional context, and whether retention gains offset the expected costs of benefit provision. I construct a novel, hand-collected database of 219 PPL policy changes implemented by 206 U.S. firms between 2005 and 2025. I link firm PPL policy changes to individual-level employment histories from Revelio, which constructs longitudinal worker-firm data from online

*This is a working paper. I welcome comments and suggestions.

professional profiles. The resulting data set comprises 5.3 million workers over 196 million employee-months. I utilize a differences-in-differences design that compares departures before versus after PPL expansions, and between workers who receive a genuine PPL expansion relative to their prior entitlement versus those who do not. This is estimated separately for men and women. The results show a 3.5% reduction in monthly departure rates relative to the mean in the period prior to the policy change. While the pooled sex differential is small and statistically insignificant on average, this masks substantial heterogeneity across workers, organizational environments, and policy designs.

Firm-provided PPL may affect retention through multiple and potentially offsetting mechanisms, which fall into two structurally distinct categories. The first operates broadly across the workforce, regardless of whether any individual worker ever takes leave. The second category is contingent on actual or anticipated leave-taking and generates heterogeneous, and in some cases opposing, retention effects across workers.

The broad retention mechanism through which PPL is expected to influence employee retention is signaling. PPL expansions may signal firm quality through the credible offering of an expensive benefit, as well as firm commitment to long-term employment relationships and employee well-being. This signaling has the potential to strengthen organizational attachment and reduce departures even among workers who do not anticipate using PPL. PPL may further convey a sex-differentiated signal that the focal firm offers a supportive work environment for women, strengthening attachment for women in particular. Through broad signaling, PPL can increase retention across the workforce, including among workers who ultimately never use the benefit.

Contingent mechanisms generate retention effects that vary in sign and magnitude across workers. The contingent mechanisms through which PPL is expected to impact employee retention are option value, income insurance, return-to-work facilitation, career disruption, and backlash.

PPL policies vary across firms, are difficult to observe prior to employment, and are typically contingent on tenure. This creates option value that can make employees "sticky" at the focal firm if they anticipate they may use PPL at some point in the future. At its core, PPL also provides income continuity during childbirth-related career interruptions. The income insurance value of PPL should reduce departures among lower-paid workers who face tighter financial constraints. This channel should also disproportionately affect lower-rank workers, who have less bargaining power to negotiate an ad hoc PPL arrangement with their employer (Hochschild, 1997). Finally, PPL facilitates return to work by providing paid time for postpartum medical recovery and early caregiving when work is often not feasible for mothers and other birthing parents, a period during which inadequate leave can precipitate labor force exit altogether.

These contingent-positive channels should load most heavily on low-paid and low-rank women, who are expected to value most PPL's insurance against childbirth-related employment disruption. Women are more likely to experience childbirth-related career disruptions (Klerman and Leibowitz, 1999; Bertrand et al., 2010; Almond et al., 2023), making the option value, income insurance, and return-to-work facilitation benefits of PPL especially salient. For lower-paid women, paid leave provides stable income during a period when unpaid time away from work may be financially infeasible. For lower-rank women, formal PPL may substitute for individualized arrangements that they are less able to negotiate informally.

Offsetting these retention-positive mechanisms, at longer leave lengths and in higher-responsibility roles, extended absences may disrupt workplace ties and advancement trajectories (Reid, 2015; Cristea and Leonardi, 2019; Gagliardi et al., 2024). The career disruption costs of PPL may attenuate or even reverse women's retention gains in more senior roles and as policy length increases. PPL may also generate backlash among employees who do not anticipate using the benefit and perceive inequity and the potential burden of workload redistribution to accommodate the leaves of colleagues. Finally, women who intend to exit the labor force entirely following childbirth derive little retention value from PPL, regardless of policy length. Depending on the size of the group of women who plan to exit the labor force, observed estimates, which reflect a blended population, may generate stronger average responses among men than women.

Analysis of subgroup retention effects reveals substantial heterogeneity. Women's differential retention gains are concentrated among the lowest-paid and lowest-rank workers and attenuate at higher salary and rank levels, where extended absences are more disruptive to workplace ties and career progression. This pattern is consistent with PPL providing greater insurance and return-to-work value where income insurance is most valuable and and absence costs are lowest. Firm context, policy design, and leave length further condition these effects, generating substantial heterogeneity that the pooled estimate obscures.

Finally, I quantify the costs to firms of PPL provision and compare them to retention gains. Under a conservative assumption of full policy utilization, and with replacement costs set to 50% of a worker's salary, retention benefits alone do not offset program costs. Yet full utilization likely overstates expected costs: Jack et al. (2025) estimate that U.S. mothers take 7.2 weeks of leave on average, while fathers take only 0.6 weeks, with actual leave-taking falling well short of available entitlements. Consistent with this evidence, sensitivity analyses vary take-up and turnover costs, showing that PPL generates positive returns under more moderate utilization assumptions and higher replacement-cost scenarios.

For managers and policymakers, these results indicate that firm PPL is more than just a family-friendly perk or a women's-only benefit: PPL can function as a broad and meaningful retention instrument. I find very little evidence of settings in which PPL increases worker departures, and it particularly boosts the retention of low-paid women, low-rank women, and men. The magnitude is substantial relative to alternative retention tools: PPL reduces departures by approximately 3.5%, an effect that would require a permanent wage increase of approximately 2.1% to achieve through compensation alone. [1] Additionally, there is no evidence that PPL is less effective in states with a public PPL program, environments in which the costs to firms are subsidized by state benefits. Lastly, the structure of PPL policies and offering them in combination with other family-friendly benefits appears to generate the largest retention response from workers. PPL costs accrue only when workers actually take leave, and under realistic take-up scenarios, the cost savings from avoided departures exceed the program's direct costs. The retention effect also does not depend on firms bearing the full cost: I find no evidence that PPL is less effective in states with a public PPL program, where firm costs are partially subsidized by state benefits. Finally, both the structure of PPL policies and their combination with other family-friendly benefits shape the magnitude of the retention response, suggesting that policy design matters beyond the mere existence of a program.

This paper contributes to strategic human capital research on how firms create competitive advantage by structuring employment systems that shape mobility, retention, and the protection of firm-specific human capital (Becker, 1964; Becker and Gerhart, 1996; Campbell et al., 2012; Kryscynski et al., 2020). Prior work shows that labor market frictions and firm-specific incentives can anchor employees to the focal firm (Mahoney and Qian, 2013; Starr et al., 2018), including evidence that firm-specific incentive bundles reduce dysfunctional turnover (Kryscynski, 2021). While much of the existing literature focuses on wages, training, and mobility constraints, this paper identifies the mechanisms through which a specific non-wage benefit can impact retention.

This study extends research on employer-specific benefits as strategic retention instruments. Prior work shows that non-transferable benefits such as health insurance reduce worker mobility (Madrian, 1994). Tsolmon and Ariely (2022) find that firm provision of health insurance is associated with lower turnover, higher productivity, and stronger financial performance. Using data on health insurance, retirement benefits, and paid time off (sick leave, holidays, and vacation), Ouimet and Tate (2023) find benefit generosity reduces departures, especially among low-wage workers. This paper extends this line of work by showing that a single benefit can operate through multiple concurrent channels and can generate opposing retention effects across workers, firms, and institutional environments. Uniform PPL provision generates a portfolio of retention effects whose net value depends on workforce composition and organizational context, which is a departure from the implicit assumption in much of the benefits literature that benefits operate through a dominant channel with heterogeneous intensity. This framework clarifies why the same policy expansion can substantially reduce departures among some workers and some environments while having no detectable effect on others, and why the net retention value of a benefit depends on the context in which it is deployed.

2 Theoretical Background

Firms succeed in large part by attracting and retaining a high-quality workforce, and compete with each other for top human capital assets. In this way, strategic human capital management practices can be a source of sustained and competitive advantage for firms (Becker and Gerhart, 1996; Hatch and Dyer, 2004). Non-wage benefits are an increasingly important dimension of this competition, yet relatively little is known about the mechanisms through which specific benefits generate retention. PPL is provided uniformly within firms, so the theoretical interest lies not in differential access to the policy, but in heterogeneous responses to a common benefit, shaped by the broad

[1] The wage equivalent applies a separation-to-wage elasticity of 1.7, which is the median reported in the meta-analysis of Sokolova and Sorensen (2021) and used as a benchmark by Bassier et al. (2022). A 3.5% reduction in departures corresponds to a permanent wage increase of approximately $3.5/1.7 \approx 2.1\%$.

mechanisms that activate through policy existence and the contingent mechanisms that activate through anticipated or actual leave-taking.

Voluntary turnover is persistent and costly for firms. The annual voluntary quit rate among U.S. private sector works is consistently between 25-30% (Bureau of Labor Statistics, 2000-2025), and the cost to replace a worker who departs is estimated to be 150-250% of their annual salary (Cascio, 2006). Work–family conflicts, particularly around childbirth, are a well-documented driver of labor force exit, especially for women (Klerman and Leibowitz, 1999; Bertrand et al., 2010; Almond et al., 2023). PPL is a natural instrument through which firms may seek to retain workers during transitions to parenthood. The high cost of turnover makes retention a central objective of strategic human capital management, and a natural benchmark to evaluate the impact of a benefit such as PPL.

While there is much variation in wage compensation across workers within a firm based on job function, department, seniority and other factors, firms tend to provide the same benefits to all workers. Ouimet and Tate (2023) ascribe the observed low level of within firm benefit variation to nondiscrimination regulations, fairness concerns, and the high administrative burden of managing multiple or complex benefit plans. Consistent with this, when firms introduce or expand PPL, they almost always offer the same benefit to all employees.

Prior work has explained heterogeneous retention effects from uniform benefits in two main ways. The benefits literature (Madrian, 1994; Ouimet and Tate, 2023; Tsolmon and Ariely, 2022) typically frames benefits as operating through a single dominant channel whose intensity varies across workers (e.g., stronger job-lock effects from health insurance among those who heavily utilize the benefit). A separate line of work emphasizes worker sorting on preferences: Bode et al. (2015) finds that CSR programs disproportionately retain employees who value social initiatives. PPL generates a more complex pattern, because it simultaneously activates multiple mechanisms that vary in sign and magnitude across the workforce and depend on which underlying mechanisms dominate for a given subpopulation.

2.1 PPL Mechanisms Shifting the Worker's Stay/Leave Threshold

PPL uniquely combines income replacement, job protection, non-transferability, and the potential for prolonged absence, and not all of these features are predictive of increased retention. The mechanisms through which PPL may affect retention fall into two structurally distinct categories. The first is broad signaling about firm quality and commitment that is activated by the existence of a PPL policy, regardless of whether any individual worker ever takes leave. The second set is contingent on leave-taking or anticipated leave-taking (including the anticipated leave-taking of colleagues) and generates heterogeneous, and in some cases opposing, retention effects across workers.

2.1.1 Broad: Signaling

The broad mechanism of signaling is expected to increase retention for both women and men. PPL expansions signal employer quality and commitment to employee well-being and long-term employment relationships. PPL expansions are costly for firms to implement, and thus constitute a credible signal of both employer commitment and financial health. Employees can reasonably infer that a firm willing and able to bear the costs of a PPL program is genuinely invested in their welfare and stable enough to support the program over the long-term. This can strengthen organizational attachment and reduce voluntary turnover even among employees not immediately (or ever) planning to have children. PPL expansions may also convey a sex-differentiated signal that the firm offers a more supportive work environment for women, strengthening attachment even beyond direct leave utilization. Through the signaling mechanism, PPL can increase attachment across the workforce, including among workers who ultimately never use the benefit.

2.1.2 Contingent: Option Value, Income Insurance, Return-to-Work, Career Disruption, and Backlash

The contingent mechanisms are activated by anticipated or actual leave-taking and generate both positive and negative retention effects depending on worker characteristics. On the retention-positive side, PPL creates benefit option value, income insurance and encourages return to work rather than labor force exit.

Firm-provided PPL is expected to generate option value that raises the cost of voluntary departure for some employees. PPL is non-transferable and typically earned only after 12 months of full-time employment, unlike other benefits that can be maintained between jobs (e.g., COBRA for health insurance, 401(k) rollovers) or that start immediately upon hire. This delayed eligibility creates option value: employees who anticipate future PPL use face meaningful forfeiture costs upon departure, making them "sticky" at the focal firm. Benefits earned at the focal firm cannot be carried to a new employer, and eligibility is typically based on tenure, so employees who anticipate childbirth and have earned PPL at the focal firm face meaningful forfeiture costs upon departure.

By providing income replacement during periods of medical recovery and intensive caregiving, PPL offers insurance against childbirth-related income loss. This is expected to lower the cost of continued employment and disproportionately increase post-birth workforce attachment for mothers and other birthing parents, for whom the postpartum period requires medical recovery during which work is not feasible. Firm PPL transforms a statutory entitlement to unpaid leave, which many workers cannot afford to use, into a paid benefit they can actually take. When unpaid Family Medical Leave Act (FMLA) leave is the only option, financially constrained workers may return prematurely because they cannot absorb the income loss. Or those workers may exit the workforce entirely when the joint burden of unpaid leave and childcare costs — which can consume a substantial share of low earners' wages — makes continued employment infeasible. Through the income insurance mechanism, PPL expands the set of workers for whom adequate recovery and workforce reintegration become feasible.

Beyond replacing income during the leave itself, PPL shapes whether the worker returns to the focal firm at the leave's end. PPL may increase the likelihood that a worker returns to the focal firm after having a child rather than exiting the workforce altogether. With adequate leave, the worker can recover, establish childcare, and return to work at a point when they are ready and able, preserving the employment relationship. The existence of a firm policy reshapes the return event itself, by signaling that returning to work at the end of parental leave is the firm's default expectation. When a firm explicitly carves out a PPL period, it may also shift norms toward parental leave usage as expected and legitimate. While leave length is a component of the return-to-work mechanism, the norms and signaling components operate independent of length and can be activated by even modest PPL policies. For example, a worker with eight weeks of firm PPL and four weeks of unpaid FMLA experiences a different employment relationship than a worker with 12 weeks of unpaid FMLA alone, even though the total time away from work is identical. The worker with firm PPL is operating within an explicit firm framework that contemplates and plans for both PPL and return to work after PPL. The worker with only FMLA, on the other hand, is exercising a statutory right that the firm has not endorsed and may even penalize. Evidence on work-family policy implementation supports this distinction: Briscoe and Kellogg (2011) show that the career consequences of using a reduced-hours program depend not on the formal policy but on whether local managers signal support. Unpaid parental leave is a lengthy, scheduled, highly visible absence that workers are statutorily entitled to take but for which firm endorsement and pay are discretionary, making it rare among employee benefits and giving explicit firm policy unusual signaling weight.

On the retention-negative side, extended absences may disrupt task and client continuity, weaken workplace ties, and increase perceived deviations from "ideal worker" norms that shape career advancement (Reid, 2015; Cristea and Leonardi, 2019; Gagliardi et al., 2024). Extended absences can also disrupt firm-specific human capital accumulation, reducing returns to continued employment (Becker, 1964). These career disruption costs scale with leave duration and organizational seniority, and load primarily on women because they take substantially longer leaves even when longer durations are available to men as well.

PPL may also generate backlash among employees who do not anticipate using the benefit and perceive inequity and the potential burden of workload redistribution to accommodate the leaves of colleagues (Perrigino et al., 2018). Visibility, cost, exclusivity, and intensity of benefits can engender resentment when benefits are clearly targeted and scarce. The probability that any individual coworker is on PPL at a given time is low: using demographic-specific fertility rates matched to workers in the sample (see Section 9.3 for details), I estimate that only 1.5% (median) to 1.8% (mean) of a firm's workforce would be on parental leave at any given time, even under full utilization of entitled weeks. Additionally, while the U.S. fertility rate has declined in recent decades, the composition of this decline suggests PPL utilization per worker is falling: a growing share of women are becoming mothers, but having fewer children on average (Livingston, 2018). This means more employees may take PPL at least once, but fewer will take it repeatedly, reducing the per-worker operational footprint of the benefit over time. The operational burden of PPL is diffuse, distributed across the firm and over time. However, the backlash literature suggests it is perceived inequity, not realized burden, that drives backlash, and PPL is a salient benefit. To the extent that backlash attenuates retention effects, the estimates in this paper reflect net effects inclusive of such a response.

Women differ in how PPL is mapped to the retention margin observed in the data. Some women expect to remain employed after having a child, while others plan to leave the labor force. In the case of the former, PPL can be expected to increase return-to-work and reduce exits. For the latter, PPL may still be valuable because it provides paid time off, but it may not translate into retention and instead may shift the timing of departure to after PPL ends. These expectations are unobserved and estimated effects reflect a mixture of workers for whom PPL meaningfully changes attachment and workers for whom it primarily changes the timing of exit. Men are less likely to experience permanent labor force exits around childbirth (Klerman and Leibowitz, 1999; Bertrand et al., 2010; Almond et al., 2023), and as a result, may exhibit stronger average responses to broad retention mechanisms such as option value and organizational signaling, even when childbirth-related channels operate primarily for women.

The broad and contingent mechanisms load differently across workers, firms, and policy designs, generating heterogeneous retention effects. The following sections develop specific predictions about when and for whom each set of mechanisms is expected to dominate.

2.2 Heterogenous Retention Effects of Firm PPL

2.2.1 Sex

The retention effects of firm PPL are expected to be stronger for women than for men, because the contingent-positive channels (option value, income insurance, and return-to-work facilitation) operate disproportionately through women. Women are more likely than men to use PPL on both the extensive and intensive margins: they are more likely to take leave in response to childbirth and to take longer leaves when they do so (Jack et al., 2025). This asymmetry in usage reflects the underlying biological asymmetry of childbirth and the prevailing social asymmetry of childcare. The postpartum period requires medical recovery during which work is often not feasible for mothers and other birthing parents, and this biological constraint does not apply to fathers and other non-birthing parents. Women also perform the majority of early childcare in dual-parent households (Yavorsky et al., 2015), extending the period during which PPL is expected to be more valuable to women.

Each contingent-positive channel inherits this asymmetry. The option value channel loads more heavily on women because higher expected utilization translates directly into a higher forfeiture cost on departure: a woman who anticipates taking PPL stands to lose more by leaving the focal firm before becoming eligible than a man with the same tenure but lower expected utilization. The income insurance channel loads more heavily on women because women face a higher probability of needing extended time away from work during a period when income would otherwise be lost. The return-to-work channel loads more heavily on women because childbirth is the moment at which women's labor force attachment is most at risk: childbirth produces sharp and persistent declines in earnings and labor force participation for women, and the same pattern does not appear for men (Almond et al., 2023).

2.2.2 Individual Worker Characteristics

The income insurance mechanism is expected to be most salient for workers who earn less and have lower financial security and stability, generating stronger retention effects in this group. Workers with tighter financial constraints derive greater insurance value from benefits that replace income during predictable non-work spells (Chetty, 2008) and empirical evidence on other non-wage benefits confirms that retention effects are concentrated among lower-wage workers (Ouimet and Tate, 2023). For workers with fewer financial buffers, forfeiting earned PPL eligibility can be particularly costly, increasing the relative attractiveness of remaining at the focal firm versus pursuing or taking a new job at a competitor firm. Consider a worker who anticipates becoming a parent and is offered an external role with a 10% raise but no immediate access to PPL. A 10% wage increase is equivalent to slightly more than five weeks of pay; any PPL policy providing more than five weeks of fully-paid leave therefore exceeds the raise in dollar terms. For policies of eight, twelve, or sixteen weeks, the foregone benefit represents 15%, 23%, and 31% of annual salary, respectively. For prospective mothers and other birthing parents, the stakes are particularly high: childbirth requires a period of medical recovery during which work is not feasible, and the absence of paid time off exposes families to substantial income loss at the same moment their expenses rise. Beyond the baseline retention effect for lower-paid workers, lower-paid women are expected to exhibit an incremental retention effect. For financially secure and highly-paid workers, on the other hand, the income insurance mechanism of PPL is expected to have limited to no influence on retention because they can absorb short-term income loss.

Seniority of role in the organization further mediates the expected retention effects of PPL. In the most junior positions, roles are relatively standardized and temporary absences are less disruptive to career progression. Consistent

Table 1: PPL Retention Mechanisms by Worker Type

Mechanism	Worker type				
	Men	Low-rank women	High-rank women	Low-paid women	High-paid women
Broad mechanism (activated by policy existence)					
Signaling	+	++	++	++	++
Contingent positive (activated by anticipated or actual leave-taking)					
Option value	+	++	+	++	+
Income insurance	∅	++	∅	++	∅
Return-to-work facilitation	∅	++	+	++	+
Contingent negative (activated by anticipated leave-taking — own or colleagues')					
Career disruption	∅	∅	−−	∅	−−
Backlash	−	∅	−	∅	−
Predicted net retention effect	+	++	−	++	−

Note: Predicted retention effects are denoted as ++ (strong positive effect), + (positive effect), ∅ (negligible or absent), − (negative effect), −− (strong negative effect). Net effects reflect the predicted balance of mechanisms for each worker type.

with this view, lower-level roles are more often filled through external hiring from similar job titles or horizontal transfers, whereas higher-level managerial and professional roles are more commonly filled through internal promotion, reflecting the greater importance of accumulated firm-specific knowledge at higher ranks (DeVaro et al., 2015; Pastorino, 2024). This makes use of PPL more viable, and thus more valuable, for low-rank workers. Further, lower-rank workers have less access to the informal flexibility, location control, and leave provisions that higher-status workers can often negotiate or assume (Briscoe, 2007; Kossek and Lautsch, 2018), so a formal employer PPL policy represents a larger marginal change in their work-life infrastructure. For workers in rigid hourly or shift-based schedules, a formal entitlement is often the only mechanism through which extended leave is feasible at all.

Conversely, at higher levels of seniority and responsibility, roles require greater continuity of presence and are less substitutable, making absences more disruptive to workplace ties and career progression (Lepak and Snell, 1999; Goldin, 2014). This can reduce the viability of utilizing a PPL policy for women at more senior roles in the firm, or penalize those who do utilize their formal entitlement. The mechanisms that support retention for lower-rank workers weaken or invert at this level. The income insurance value of PPL is not binding for senior women who have the financial means to weather an unpaid leave. Senior-rank women are also expected to have greater access to informal alternatives to PPL, such as partial work-from-home or other ad hoc accommodations, which may substitute for and reduce the marginal retention value of formal PPL entitlements. At the same time, these women may bear the career disruption costs of PPL usage (weakened ties, missed advancement, signaling of reduced commitment) and their departures may increase if their position or advancement trajectory has been undermined by the absence.

Backlash is expected to be negligible for low-paid and low-rank workers because the operational burden of leave-taking is bounded for lower-rank roles. Standardized work is more readily covered through temporary replacement than the specialized work of senior roles, limiting the workload spillover that drives coworker resentment. Further, the competitive promotion dynamics that may amplify backlash in senior-rank roles are less salient in lower-rank roles, where advancement is structured more by tenure or formal grade progression than by direct competition with leave-takers.

A sex asymmetry emerges in the retention mechanisms of PPL. For men, option value and signaling are expected to generate broadly positive retention effects, with backlash as the only offsetting channel. Women's responses reflect those same mechanisms, amplified by the income insurance and return-to-work mechanisms, and attenuated by the career disruption costs and the subpopulation of women who plan to leave the workforce after childbearing regardless of PPL policy. As a result, men may show stronger average retention responses to PPL expansions not because they value PPL more, but because there is no subpopulation of men for whom PPL is retention-negative. In the aggregate, the offsetting mechanisms for women are expected to attenuate women's overall retention response relative to men's.

This attenuation is expected to be most pronounced at higher levels of pay and seniority, where the insurance value of PPL is lowest and the career costs of leave-taking are highest. Table 1 summarizes the predicted direction and relative strength of each mechanism across these worker types.

2.2.3 Firm characteristics

Firm characteristics also shape the effectiveness of PPL as a retention tool. Retention effects are expected to be more pronounced at larger firms, which typically have greater organizational slack, more formalized staffing systems, and greater capacity to absorb temporary absences without disrupting operations (Bourgeois, 1981; Osterman, 1994; Kalleberg et al., 1996; Shaw et al., 2005). The bureaucratic structures of large firms can further serve to buffer workers from the career penalties associated with absences (Briscoe, 2007). In contrast, smaller firms often face tighter staffing constraints and higher marginal costs of worker absence, which may reduce the perceived credibility or usability of PPL policies and diminish their value to employees.

Similarly, the retention effects of PPL are expected to be strongest at firms with low to moderate growth. Such firms offer more stable organizational environments in which career advancement follows predictable trajectories and temporary absence is less likely to carry significant professional costs (Bidwell and Briscoe, 2010). In contrast, high-growth firms often feature intense workloads and rapid organizational change (Baron et al., 2001), and temporary absences risk displacing workers from evolving roles and advancement opportunities (Gagliardi et al., 2024). In these environments, even generous PPL policies may have limited retention value if taking leave is perceived to carry significant career risk.

Internal labor market structure further conditions the extent to which PPL generates broad retention effects for both men and women through option value and organizational signaling. In firms with high internal labor market flexibility, workers are less attached to a specific role or trajectory and less locked in by firm-specific human capital, weakening the binding force of any non-transferable benefit. Frequent internal moves already signal organizational accommodation, reducing the incremental signaling value of PPL expansions. At the other extreme, firms with very rigid internal labor markets may already generate strong lock-in through other mechanisms, such as firm-specific tenure, relationships, and position in high-stakes promotion queues (Doeringer and Piore, 1971; Lazear and Rosen, 1981; Bidwell and Keller, 2014). In these environments PPL adds limited additional non-transferable value at the margin. In moderately flexible firms, by contrast, baseline lock-in is meaningful but not overwhelming, and PPL expansions add a significant additional dimension of non-transferable value by raising the cost of departure and more credibly signaling firm commitment to long-term employment relationships. Retention effects are therefore expected to be largest in firms with moderate internal labor market flexibility, where the conditions for PPL's option value and signaling mechanisms to bind are most favorable.

Firms differ in the degree to which advancement depends on firm-specific human capital. Where progression follows rigid internal ladders that reward accumulated, non-transferable knowledge (Becker, 1964; Campbell et al., 2012), returns to continuous tenure are steeply nonlinear (Lazear and Rosen, 1981; Bertrand et al., 2010; Goldin, 2014), and even short absences can permanently displace a worker from the promotion queue. The contingent career-disruption channel binds tightly under these conditions: PPL does not compensate workers for forgone tournament position or for depreciation of firm-specific skills during leave, and workers anticipating these costs may exit rather than absorb them. The broad option-value and signaling channels operate more weakly as well, because formalized leave cannot reshape the rigid advancement structure that workers are already optimizing against. As I result, I expect that firms with high firm-specific human capital will have attenuated or null retention gains from PPL expansions: workers can take leave, but the career costs of doing so (forgone tournament position, depreciation of firm-specific skills) erode the benefit's value, so PPL does little to alter their stay-leave threshold.

2.3 Interaction of PPL and Other Firm Benefits

Firms expand PPL in different organizational contexts. Some firms expand PPL in isolation, while others expand PPL alongside concurrent changes to other benefits. Those other benefits can be family-friendly (e.g., adoption and surrogacy assistance, fertility benefits, ramp-back programs, childcare support, expanded caregiving leave) or unrelated to PPL (e.g., 401(k) matching, one-time bonuses, student loan repayment, vacation policy adjustments). The retention effects of a PPL expansion are expected to depend on what else changes at the same time, particularly for women.

PPL expansions accompanied by concurrent family-friendly benefit changes should generate stronger retention effects than PPL expansions made in isolation or accompanied by unrelated benefit expansions. Some family-friendly benefits are economically complementary to PPL: fertility and adoption assistance may increase the likelihood of leave-taking and the expected option value of PPL, while ramp-back programs and childcare support may reduce the career disruption costs that would otherwise attenuate women's retention gains. Concurrent family-friendly changes may also amplify the signaling channel by conveying broader firm commitment to working parents and mothers. Broad family-friendly benefit expansions may also reduce backlash by extending coverage to workers facing different paths to parenthood or with existing dependents, reducing the perceived inequity that could create backlash against a standalone PPL expansion. PPL expansions accompanied by unrelated benefit changes are not expected to generate these complementarity effects, since such changes do not reduce leave costs or raise expected utilization.

2.4 Interaction of Public and Private PPL Benefits

The interaction with public benefits further conditions the retention effects of firm PPL. In jurisdictions without a public PPL program, employer-provided PPL represents the sole source of PPL available to workers. In these settings, entitlement to PPL at the focal firm increases the cost of job change by raising the risk of having no paid leave if childbirth occurs shortly after a move or if the prospective firm does not provide PPL. This risk is amplified by the limited transparency of firm PPL policies[2] and the signaling costs associated with inquiring about PPL benefits during the hiring process. As a result, firm-level PPL expansions should generate stronger retention effects for both women and men in jurisdictions without statutory public PPL programs.

2.5 Retention and PPL Length

The effect of PPL on retention is expected to vary with the length of leave provided. While longer PPL increases the potential insurance value of the benefit for workers facing childbirth-related constraints, additional weeks may yield diminishing or no incremental retention gains beyond a point. As leave length increases, the insurance value of additional weeks attenuates while absence-related career disruption costs for women grow, eventually dominating the marginal retention effect.

At shorter policy lengths, PPL may generate broad retention effects for both women and men by increasing the option value of staying relative to uncertain PPL access elsewhere, and signaling organizational support and commitment to employees. Even limited durations of firm PPL can increase effective income protection, either by providing paid leave where none exists or by supplementing capped public benefits. Policy expansions may also affect retention by increasing the salience of PPL and reducing perceived stigma or uncertainty associated with leave-taking, independent of duration.

Among the contingent mechanisms, insurance value, return-to-work facilitation, and absence costs scale with leave duration, while option value and signaling are activated by the existence of a policy and depend less on its specific length. As PPL length increases, marginal retention gains may attenuate or reverse for women via absence costs. Extended absences can disrupt task and client continuity, weaken workplace ties, and increase perceived deviations from "ideal worker" availability norms that shape advancement (Reid, 2015; Cristea and Leonardi, 2019; Gagliardi et al., 2024). Additional weeks of leave may increase career disruption risks that offset the marginal retention benefits of longer policies. Because men take substantially shorter leaves even when longer durations are available, these absence-related mechanisms are expected to load disproportionately on women.

These mechanisms imply that the retention effects of PPL depend on how policy length interacts with option value, insurance value, signaling, and absence-related costs, motivating an empirical examination of how retention responses vary across leave durations and between women and men.

[2] This opacity is substantial in practice. In my hand-collected data of the PPL policies of the 500 largest U.S. public companies as of 2025, only 51% publicly confirm whether they offer PPL on their corporate websites, and only 32% disclose the length of the benefit. This limited transparency makes it difficult for prospective employees to assess PPL at potential employers without direct inquiry, which may carry signaling costs given documented penalties associated with anticipated parenthood, particularly for women.

2.6 The Cost of PPL

While wages and benefits like health insurance affect all employees continuously and enter the firm's cost structure mechanically, PPL is a contingent benefit whose costs are realized only when leave is taken. As a result, the overall cost of firm PPL programs depends not only on stated policy generosity but also on workforce composition, fertility timing, usage rates, and interactions with public programs that subsidize firm PPL.

Demographic trends and institutional features jointly shape firms' expected PPL costs by affecting both the likelihood that leave is taken and the cost of leave when it occurs. Long-run declines in U.S. fertility (The Pew Charitable Trusts, 2022) reduce the expected incidence of childbirth-related leave among employees, lowering expected PPL costs. At the same time, delayed childbearing concentrates births among older workers (Livingston, 2018), for whom time-to-pregnancy is longer and fertility rates decline with age (George and Kamath, 2010). This shift can reduce expected PPL utilization at the firm level while increasing the cost of PPL when it occurs, as births are more likely among more senior and higher-paid employees. Delayed fertility may also extend the period during which workers remain attached to the focal firm to preserve PPL eligibility. Men's access to and use of PPL has increased in recent years (U.S. Census Bureau, 2025), raising expected costs as the base of PPL-utilizing workers expands.

Public policy further conditions the cost of PPL provision. In states with public PPL programs, statutory benefits subsidize firm-provided leave, reducing the marginal cost of firm PPL expansions. Realized costs also depend on actual usage rather than eligibility: recent evidence shows that employees frequently take less leave than formally offered, implying that stated policy generosity overstates realized firm expenditures (Jack et al., 2025). Several factors increase the expected cost of PPL to firms: delayed childbearing, longer or more generous PPL policies, and rising utilization among men. However, these are countered by factors that reduce costs: declining fertility, public PPL subsidies, and usage that falls short of formal entitlements.

3 Institutional Background

Paid parental leave (PPL) specifically refers to leave intended to recover from childbirth and/or care for a newborn. For the purposes of this paper, firm PPL encompasses the total paid time off provided to full-time workers following the birth of a child. Firms use varied language in identifying their PPL policies. PPL in this paper includes policies that might be called pregnancy disability, birthing, maternity, parental and/or bonding leave by the firm providing it.

The 1993 passage of the Family and Medical Leave Act (FMLA) in the U.S. required establishments with 50 or more employees to provide 12 unpaid workweeks of leave during any 12-month period for various family matters including but not exclusive to parental leave. The carve-out for firms with less than 50 employees allows them to deny FMLA benefits if granting such a leave would jeopardize the long-term viability of the business. While FMLA created access to unpaid parental leave for most working Americans, paid leave access has remained relatively low.

PPL comes in the form of state-sponsored (public) and firm-sponsored (private). State-sponsored PPL is relatively new, and California was the first state to enact it in 2004. As of 2025, 9 states and D.C. have a statutory entitlement to PPL for state residents. State or public PPL is typically financed through payroll taxes and administered as a state-run social insurance program, often integrated with state disability or unemployment insurance systems. For example, California's PPL program is fully funded through contributions to the State Disability Insurance program. Unlike FMLA, state-sponsored PPL does not depend on employer size and establishes a statutory entitlement to paid leave for workers who meet state-specific eligibility requirements. Public PPL overage is typically conditional on modest work or earnings thresholds, and some states include exceptions for self-employed persons, but otherwise, it tends to cover nearly all workers in the state.

State PPL programs are structured as income replacement defined as a maximum percentage of earnings up to a statutory cap and paid for a fixed duration. Table 2 summarize the key features of U.S. public PPL programs. In states with public PPL, benefits provided by the state are paid first, and firm-provided PPL is structured as a supplement to bridge the potential gap between the public benefit and as much as 100% income replacement. Firm PPL is not a substitute for public PPL, but rather, the two systems work in tandem. Residents in states without public PPL rely solely on firm-provided PPL, if any.

Firms vary widely in both the baseline provision of PPL and, among firms that do provide it, in the specific terms and design of their policies, such as the length of PPL provided, eligibility tenure requirements, and whether leave

Table 2: Statutory Public PPL Programs in the U.S.

State	Effective Date	Birthing Parent Paid Weeks	Non-Birthing Parent Paid Weeks	Max Wage Replacement	Weekly Benefit Cap ($)
California	Jan 7, 2004	14	8	70%	1,620
Colorado	Jan 1, 2024	12	12	90%	1,100
Connecticut	Jan 1, 2022	12	12	95%	942
Delaware	Jan 1, 2026	12	12	80%	900
Maine	May 1, 2026	12	12	90%	1,103
Maryland	Jan 1, 2026	12	12	90%	1,000
Massachusetts	Jan 1, 2021	18	12	80%	1,150
Minnesota	Jan 1, 2026	18	12	90%	1,103
New Jersey	Jan 7, 2009	18	12	85%	1,055
New York	Jan 1, 2018	18	12	67%	1,151
Oregon	Mar 1, 2023	18	12	100%	1,524
Rhode Island	Jan 1, 2014	11	6	60%	1,070
Washington	Jan 1, 2020	16	12	90%	1,456
Washington, D.C.	Jul 1, 2020	14	12	90%	1,118

Note: This table summarizes statutory public PPL programs in the U.S. Included programs establish a legal entitlement to PPL for workers who meet state-specific work or earnings requirements and provide benefits through state-run social insurance systems. Voluntary or optional programs, including those that rely exclusively on private insurance arrangements without a statutory guarantee of coverage, are excluded. The table includes all enacted public PPL programs announced as of the time of writing. Leave durations reflect statutory entitlements; maximum wage replacement rates and weekly benefit caps are reported at statutory maxima.

entitlements are more generous for mothers and other birthing parents. Private firm PPL ranges from as little as one week to as much as a full year of leave. Eligibility requirements vary and there is no standard set of terms among firms that offer PPL. Notably, most firms delay the onset of PPL benefits and restrict them to employees who have worked at the focal firm on a full-time basis for a minimum specified term (often 12 months) and/or worked a minimum number of hours in the preceding 12 months (often 1,250). A minority of firms do offer full PPL benefits to employees as of the first day of their employment, meaning an employee could theoretically start their job on leave.

4 Data

This work relies on a combination of firm-level PPL policy change information as well as individual-level work history. Individual-level work history data was sourced from Revelio, which constructs longitudinal worker–firm histories from publicly available online professional profiles (primarily LinkedIn). This was combined with my hand-collected dataset of 219 PPL policy changes implemented by 206 U.S. firms.

I created the firm PPL policy change database by searching news articles, press releases, and public company filings for PPL policy changes. All policy changes observed were expansions of PPL benefits; there were no examples found of a reduction in PPL benefits. In a few cases, I observe a firm expand its PPL policy and then expand it further in subsequent years. Some instances of PPL policy changes were the initiation of a PPL policy (e.g., increasing from zero some amount of paid weeks), while others were expansions making existing policies longer.

Firms almost always offer the same PPL benefit to all employees, with one common distinction: policies often vary by birthing status, providing one entitlement for mothers and other birthing parents and another for fathers and other non-birthing parents. In the database, 58% of policies vary PPL length by birthing status, offering an average of 8.4 additional weeks to mothers and other birthing parents. It is very rare for PPL benefits to differ based on other employee characteristics, and only two of the policy changes in the sample include such a distinction.[3] In these instances, the different PPL entitlements are based on clear employment distinctions, such as hourly versus salaried employees or corporate versus frontline divisions. When firms specify leave entitlements in terms of a maximum allowable duration, the maximum stated length is used in my analysis. If a firm refers to a supplemental paid leave

[3]This is consistent with a broader survey of PPL policies. I collected the PPL policies of the 500 largest U.S. public companies in 2024 and 2025, and fewer than 5% reported a PPL entitlement that differs on a distinction other than birthing status.

allowance for pregnancy-related disability without specifying duration, I assume a six-week period unless otherwise noted. Any incremental leave explicitly tied to cesarean delivery is excluded from the weeks of PPL used in the analysis.

The firm PPL policy changes recorded in the database occur between 2005 and 2025, with a pronounced clustering after 2015, as shown in Figure 1. This aligns with BLS data on the expansion of PPL benefits to U.S. private sector workers over time, as illustrated in Figure 2.

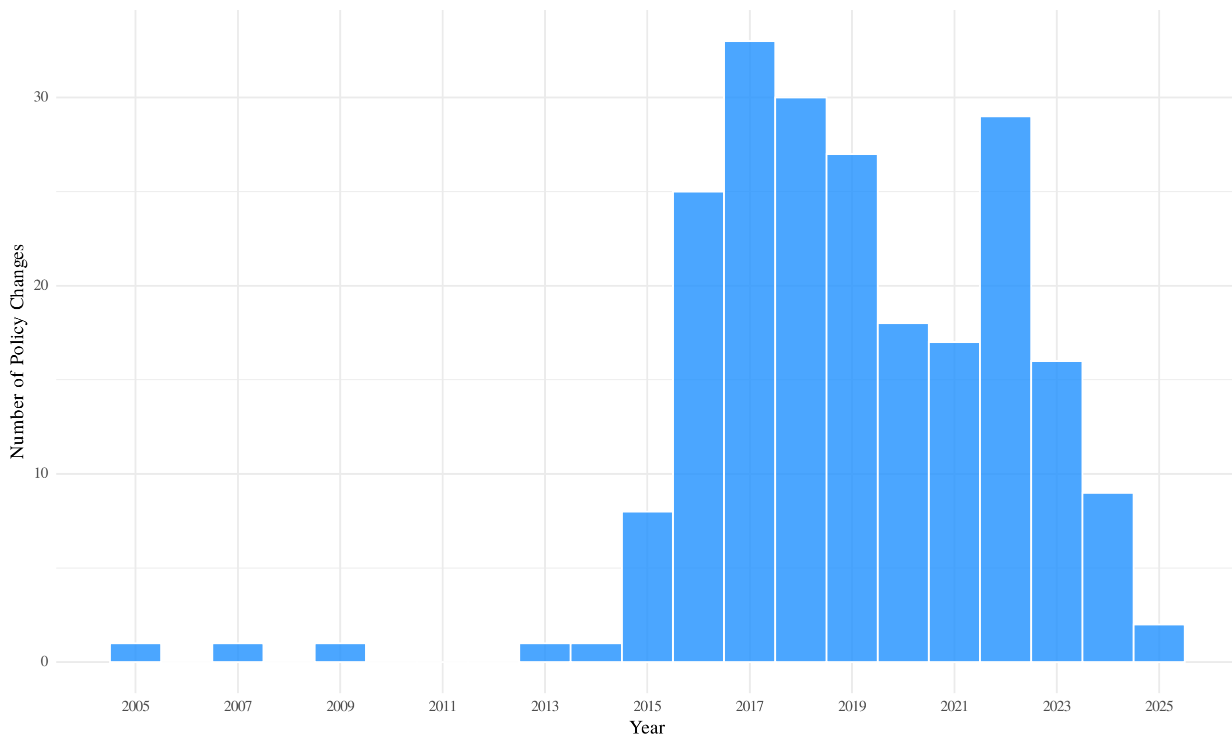

Figure 1: Distribution of Firm PPL Policy Changes by Year

Note: This figure shows the distribution of observed firm PPL policy changes by year, comprising 219 policy changes at 206 firms. All observed changes were policy expansions; no reductions were observed.

The individual-level data used for this analysis consist of employment histories from Revelio, which I link to the firm-level PPL policy change database described above. The resulting dataset includes all workers employed at a firm that implemented a PPL policy change within a ± 36 month window around the firm's policy change date. The full dataset contains 5.9 million unique workers observed over 222 million employee-months.

I restrict the sample to firm–state event windows unaffected by changes in public PPL policy, excluding worker observations where a public PPL enactment occurs in the worker's state of employment within ± 36 months of the employing firm's PPL policy change. The avoids confounding the effects of public and private PPL expansions. I also exclude the policy change month itself because policy effective dates vary within the month, creating ambiguity about whether observations in that month should be classified as pre- or post-treatment. The analysis is further restricted to include only U.S.-based workers and positions with valid employment start dates. The resulting analytical sample comprises 5.3 million unique workers observed over 196 million employee-months. Compared to the broader U.S. working population, the sample is younger, more educated and more highly paid (see Table 3). As is discussed in greater detail in Section 5, the empirical strategy uses within-firm variation in PPL exposure for identification, so estimated effects reflect changes for workers within these firms rather than averages over the broader workforce. Further, the results heterogeneity analysis explicitly examines how treatment effects vary across worker characteristics such as age, education and compensation.

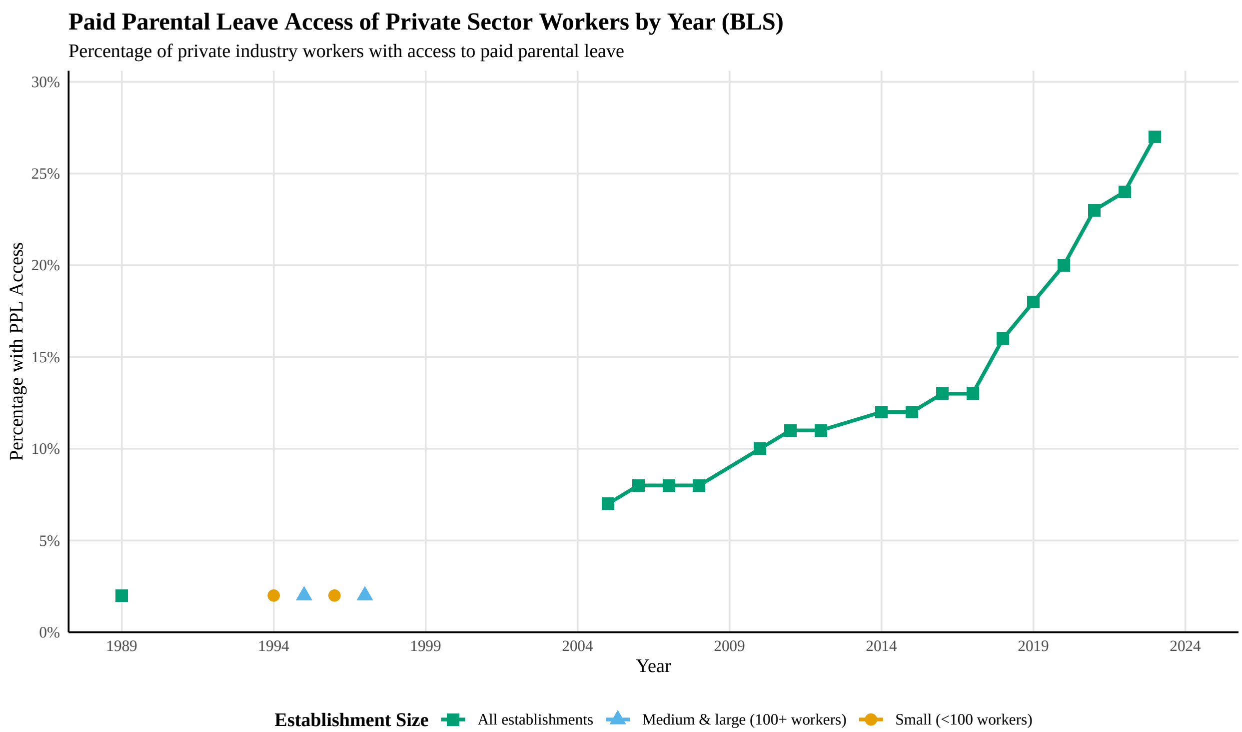

Figure 2: Access to Paid Family Leave Among U.S. Private Industry Workers, 1989–2023

Note: This figure shows the percentage of private industry workers with access to employer-funded paid family leave, by establishment size. Estimates are derived from the Bureau of Labor Statistics National Compensation Survey (NCS) and include only family leave plans for which the employer incurs a cost. BLS estimates on family leave were not published between 1999 and 2004.

Table 3: Summary Statistics: Worker Characteristics

	Sample			*U.S. Benchmark*	
	Mean	Median	SD	U.S. Workforce	U.S. College-Educated Workforce
Female (%)	44.4	—	—	47.0	51.6
Age (years)	30.7	27.7	10.6	42.0	44.5
Annual salary ($)	77,558	59,927	56,598	56,000	89,000
Seniority (1–7 scale)	2.36	2	1.44	—	—
Highest Education (% among those with observed degree)					
High school	4.5			25.6	—
Associate	7.4			10.5	—
Bachelor	54.0			27.0	59.8
Master	18.1			9.3	28.6
MBA	11.0			—	—
Doctorate	5.1			2.1	11.7

Note: N = 5,277,047 unique workers. Sex and highest education are time-invariant per worker; salary, seniority, and age are first averaged within worker across their months in the sample, then summarized across workers. Age is observed for the subset of workers for whom educational attainment and graduation year permit imputation (N = 3,035,171); the younger mean age relative to U.S. benchmarks reflects selection into the age subsample, since workers with recently observed graduation dates are more likely to be young. Education is observed for 73% of workers. *U.S. Workforce* reports benchmarks for the employed civilian labor force age 25+ from the BLS Current Population Survey and Census Bureau ASEC (2024). *U.S. College-Educated Workforce* reports benchmarks for employed workers age 25+ holding a bachelor's degree or higher, computed from 2024 ACS 1-year PUMS (N = 625,135 unweighted, 62.5 million weighted). The salary row in this column reflects median annual wages for full-time year-round workers; in the sample column, salary covers all workers regardless of work intensity. ACS does not separately code MBAs; master's includes MBAs. ACS doctorate (5.2%) refers to academic doctorates only; ACS also reports a professional degree category (6.5%, e.g., law and medicine) that does not have a separately reported Revelio counterpart.

Table 4: Summary Statistics: Firm & PPL Policy Characteristics

	Mean	Median	SD	Min	Max
Total firm employees (CapIQ)	82,758	26,300	202,841	1	2,100,000
Firm employees in sample	27,772	9,048	49,268	1	330,858
States with firm employees	39	44	11.7	1	50
PPL weeks (mothers/birthing parents)					
Before expansion	7.2	6	5.1	0	18
After expansion	14.0	14	6.8	2	52
Change	6.3	6	4.0	0	24
PPL weeks (fathers/non-birthing parents)					
Before expansion	2.5	0	3.4	0	14
After expansion	9.1	8	6.8	0	52
Change	5.8	6	4.3	0	21

Note: Sample includes 215 PPL policy expansions across 202 firms; 12 firms made more than one expansion. Four firms in the policy database have no remaining observations after restricting to U.S.-based workers whose states did not enact a public PPL mandate within the ±36-month event window. *Total firm employees (CapIQ)* is the firm's total worldwide employee count from S&P Capital IQ (N = 206 of 215 expansions; missing for firms not covered by Capital IQ). *Employees in sample* reflects the number of unique workers observed in the Revelio data within the ±36-month event window (196,407,437 worker-months in total; mean 913,523 per expansion, median 281,261). *States with employees* counts U.S. jurisdictions in which the firm has at least one observed worker. *PPL weeks* reflect policy entitlements coded in the underlying policy database. "Before" and "Change" rows are restricted to the N = 116 expansions for which the prior policy is known; "After expansion" rows include all expansions with a known post-policy entitlement (N = 214 for birthing parents; N = 209 for non-birthing parents).

Table 4 provides summary statistics of the firms in the sample. The firms in the sample are predominantly large, national employers. While the size of firms in the sample ranges from fewer than 50 to more than 100,000 employees, approximately two-thirds have 10,000 or more employees. The firms in the sample also tend to have a large geographic footprint: more than half of firms have employees in 40 or more states. This broad geographic footprint supports the identification strategy, which exploits within-firm variation in PPL exposure across states with different public PPL environments.

Employment spells without an observed end date are treated as ongoing. Employee age is not directly observed in the employment histories and is instead imputed using educational attainment and graduation year information; individuals without sufficient information to impute age are excluded only from analyses that require age. Additional details on sample construction and age imputation are provided in Appendix 9.1. To mitigate measurement error arising from delayed updates to LinkedIn profiles, which underlie Revelio employment histories, the data are right-censored in June 2025, after which point observed departures begin to decline mechanically.

5 Research Design & Empirical Strategy

I estimate the causal effect of firm PPL expansions on employee retention using a differences-in-differences design. The outcome variable is employee departure from the focal firm. The first difference compares monthly departure rates before versus after a firm's PPL expansion. The second difference compares monthly departure rates between workers who receive a genuine increase in their PPL entitlement relative to the prior level versus those who do not. The specification further interacts a dummy variable for worker sex to generate estimates of the effects separately for men and women. lastly, fixed effects are included to account for sex-specific baseline departure rates and time trends.

My independent variable is *EffectiveTreatment*, a binary indicator of whether the focal firm's PPL expansion provides workers with weeks of leave strictly exceeding the maximum previously available to that worker from either statutory public PPL programs or the firm's previous policy. This definition isolates genuine benefit expansions and accounts for heterogeneity in both state policies and firms' prior PPL offerings. Firms typically offer a single PPL policy that applies uniformly across their U.S. workforce[4], often with different entitlements for birthing and non-birthing parents. As a result, a PPL policy change can be viewed as a firm-wide treatment defined separately by sex. Women are effectively treated when the firm's maternity leave weeks (i.e., leave for mothers and other birthing parents) exceed both any applicable public maternity weeks and the firm's pre-policy-change maternity weeks. Similarly, men are considered effectively treated when the firm's paternity leave weeks (i.e., leave for fathers and other non-birthing parents) exceed both any applicable public paternity weeks and the firm's pre-policy-change paternity weeks. This approach allows both sexes to be treated, but at potentially different times or not at all, depending on whether the firm's policy expansion provides genuine additional coverage for each sex-state cell.

For each sex, a worker's effective PPL entitlement in a given month is:

$$\text{EffectiveWeeks}_{g,f,s,t} = \max\{\text{FirmWeeks}_{g,f,t},\, \text{StateWeeks}_{g,s,t}\} \tag{1}$$

where $\text{EffectiveWeeks}_{g,f,s,t}$ is the maximum PPL weeks available to sex g at firm f in state s in month t, $\text{FirmWeeks}_{g,f,t}$ refers to the weeks offered by firm f to sex g in month t, and $\text{StateWeeks}_{g,s,t}$ denotes the weeks provided by state s's public PPL program to sex g in month t, with $g \in \{\text{women}, \text{men}\}$.

The sex-specific treatment start is defined as the first post-policy month in which effective weeks ($\text{EffectiveWeeks}_{g,f,s,t}$) exceed both the current state offering and the firm's historical maximum for that sex:

$$T^{\text{start}}_{g,f,s} = \min\left\{t:\ \text{post}_{f,t} = 1 \text{ and } \text{EffectiveWeeks}_{g,f,s,t} > \max\{\text{StateWeeks}_{g,s,t}, \text{PreMaxWeeks}_{g,f,s}\}\right\} \tag{2}$$

where $\text{PreMaxWeeks}_{g,f,s}$ is the maximum weeks firm f offered to sex g in state s before the policy change. For firms with no observed pre-policy-change PPL offering, $\text{PreMaxWeeks}_{g,f,s}$ is set to zero. Policy announcements confirm that the change represents an increase (e.g., "XYZ Company increased maternity leave to 12 weeks") even when they do not specify the prior level, so this assumption preserves the correct treatment classification. To the extent that

[4] While firm policies often vary by country, I have not encountered a single instance of a firm PPL policy that varies by jurisdiction within the U.S.

some of these firms had pre-existing PPL, however, the assumption overstates the magnitude of the policy change and attenuates estimated treatment effects toward zero. If a firm's policy change does not increase effective weeks above this threshold, the sex-firm-state unit remains untreated (EffectiveTreatment$_{i,f,s,t}$ $= 0$ for all t).

The construction of EffectiveTreatment$_{i,f,s,t}$ isolates firm PPL expansions that provide workers with additional PPL weeks beyond their prior PPL entitlement from either public PPL benefits or the firm's previous PPL policy. A worker is treated if and only if the firm's new PPL policy offers strictly more weeks than the maximum available from these alternative sources. This binary treatment indicator equals 1 from the sex-specific treatment start month onward, and 0 otherwise:

$$\text{EffectiveTreatment}_{i,f,s,t} = \mathbf{1}\{t \geq T^{\text{start}}_{g,f,s}\} \tag{3}$$

where $g \in \{\text{women}, \text{men}\}$ denotes the sex of worker i, and $T^{\text{start}}_{g,f,s}$ is the first month in which firm f in state s provides sex g with strictly more PPL weeks than the maximum available from alternative sources.

The unit of observation is an employee-month, and the primary outcome is an indicator of employee departure from the focal firm in a given month. To avoid confounding public and firm PPL expansions, I exclude any observations where a state PPL policy change occurs in a worker's state within the $\pm$36-month observation window relative to the focal firm's policy change. This ensures that *EffectiveTreatment* captures only firm-driven expansions in leave generosity.

Formally, I estimate the following specification:

$$\begin{aligned}\text{Departure}_{i,f,s,t} = \beta_0 &+ \beta_1\,\text{Woman}_i + \beta_2\,\text{EffectiveTreatment}_{i,f,s,t} \\ &+ \beta_3\big(\text{Woman}_i \times \text{EffectiveTreatment}_{i,f,s,t}\big) + \alpha^g_{f,s} + \lambda^g_t + \varepsilon_{i,f,s,t}\end{aligned} \tag{4}$$

where Departure$_{i,f,s,t}$ equals 1 if individual i in firm f and state s departs in month t, otherwise 0. Woman$_i$ equals 1 if individual i is a woman, otherwise 0. The term $\alpha^g_{f,s}$ denotes firm-state-sex fixed effects, which absorb time-invariant differences in departure rates across firm-state-sex cells. This allows women's baseline departure rate at a given firm-state to differ from men's at the same firm-state. The term λ^g_t denotes sex-specific month-year fixed effects, which allow aggregate time trends and shocks to differ between men and women. The coefficient β_1 is absorbed by the sex-specific fixed effects, but the coefficients of interest, β_2 and β_3, remain identified. β_2 estimates the response in men's departure rates to effective treatment, while β_3 captures the additional effect for women, with $\beta_2 + \beta_3$ giving the total treatment effect on women's departures. Standard errors are clustered at the firm-state-sex level.

This identification strategy relies on three sources of identifying variation. The first, cross-firm timing variation, arises because different firms expand PPL at different times, so not-yet-treated firm-state-sex cells provide the baseline against which the gender-specific time trends are estimated. The second, within-firm, across-state variation, arises because *EffectiveTreatment* is defined relative to statutory public PPL entitlements: the same firm policy change creates effective treatment in states with no or little statutory public PPL but not in states whose public PPL already meets or exceeds the new firm offering. The third, within-firm-state, across-sex variation, arises because *EffectiveTreatment* is constructed separately by sex, so a single-sex expansion (e.g., a firm raising maternity weeks beyond the state benchmark while leaving paternity weeks unchanged) generates comparisons across sexes within the same firm-state. As a result, a sex-firm-state cell where the firm expands PPL but not in excess of the employee's existing entitlement is never treated. These never-treated cells, together with not-yet-treated cells, form the control group.

6 Results

6.1 Main Effects of PPL Expansions on Employee Departures

The results of the effective treatment specification outlined in Equation 4 are shown in Table 5. Column (1) estimates the average retention effect of PPL expansions; Column (2) adds a *Woman* $\times$ *EffectiveTreatment* interaction to test whether effects differ by sex. Because the dependent variable is a binary indicator for whether an employee departs in a given month, all reported coefficients can be interpreted as percentage-point changes in monthly departure rates. The *EffectiveTreatment* coefficient is negative, statistically significant, and consistent across the two specifications: PPL expansions reduce monthly departure rates by 0.06 percentage points (a reduction of 3.5% relative to the pre-treatment mean monthly departure rate of 1.81%). The interaction term *Woman* $\times$ *EffectiveTreatment* is small and insignificant, suggesting retention effects do not differ meaningfully by sex in the aggregate.

The absence of a significant sex differential in the pooled specification may reflect either genuinely similar retention responses across sexes or heterogeneity across workers and organizational contexts that averages out in the aggregate. To distinguish between these possibilities, I examine how retention effects vary over time and across worker characteristics, firm attributes, and institutional environments by estimating Equation 4 separately for relevant subgroups. As I will show in subsequent sections, the null pooled interaction masks substantial heterogeneity across workers and organizational settings.

Table 5: Monthly Departure Regressed on Effective Treatment × Woman

	(1)	(2)
EffectiveTreatment	-0.0625^{***}	-0.0602^{***}
	(0.0105)	(0.0158)
Woman × EffectiveTreatment		-0.0049
		(0.0208)
Firm × State × Sex FEs	Yes	Yes
Year × Month × Sex FEs	Yes	Yes
Mean (departure)	1.7815	1.7815
SD (departure)	13.2278	13.2278
Num. obs.	194,045,615	194,045,615
Num. clusters	14,299	14,299

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and firm's previous policy, calculated separately by sex. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

As a robustness check, I assess whether the staggered structure of the design induces negative-weighting bias (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). The firm-state-sex fixed effects absorb all unit-level heterogeneity, limiting cross-firm comparisons, but the sex-specific time fixed effects are estimated across all firms, leaving open the possibility that already-treated firms influence the counterfactual trend for later-treated firms. The de Chaisemartin and D'Haultfœuille (2020) decomposition shows this concern is empirically negligible: negative weights account for just 0.5% and 2.8% of total weight mass for women and men respectively — well below any threshold that could materially distort the estimates (Appendix 9.4.2).

I also redefine treatment in units of full-wage-equivalent weeks to address the concern that a week of firm PPL and a week of state PPL are not equivalent in their effective wage replacement value. State programs typically replace less than 100% of pre-leave wages and impose binding weekly caps for higher earners, while firm PPL typically replaces 100% by topping up state benefits during weeks of overlap. Under this Effective Compensated Weeks (ECW) construction, a firm expansion is coded as treatment when it raises the worker's total compensated weeks. The pooled retention effect is similar in sign and magnitude to the main specification, as are the results by worker salary quartile. Full details are reported in Appendix 9.4.1.

Additionally, I aggregate observations to the annual level to ensure that results are not driven by seasonal patterns in employment transitions or by Revelio's imputation of January as the default start/end month for employment spells with unknown dates. The annual specifications yield coefficients that are directionally consistent with and somewhat larger than the monthly estimates presented here. The interaction term *Woman* × *EffectiveTreatment* is negative but statistically insignificant in the annual analysis, as in the monthly specification. Full details are reported in Appendix 9.4.3.

6.2 Dynamic Effects and Event-Study Evidence

To examine the dynamic response of employee departures to firm PPL expansions and to assess the validity of the identifying assumptions underlying the difference-in-differences estimates, I estimate an event-study specification that traces departures relative to the timing of effective treatment. The event-study framework replaces the effective treatment indicator in Equation 4 with a series of indicators for event time, defined as the number of months relative to the sex-specific effective treatment start date.

To address statistical power concerns arising from estimating 72 separate monthly coefficients, I aggregate event time into 12-month bins: -36 to -24, -24 to -12, -12 to -1 (reference), 1 to 12, 13 to 24, and 25 to 36 months. (Period 0 is excluded because PPL policy change dates vary across firms, making it ambiguous whether observations in the policy change month should be classified as pre- or post-treatment). This matches the sample restriction used in the difference-in-differences specification. The coefficients capture the evolution of departures in each 12-month window relative to the omitted bin immediately preceding treatment. I estimate the model separately for men and women, which allows baseline departure rates and time trends to differ by sex. Each specification includes firm-state and month-year fixed effects, with standard errors clustered at the firm-state level within each sex subsample.

Formally, I estimate the following:

$$\text{Departure}^g_{i,t} \;=\; \sum_{b\in B} \beta^g_b \,\text{Treated}^g_{f,s}\, \mathbf{1}\{k_{i,t}\in b\} \;+\; \alpha^g_{f,s} \;+\; \lambda^g_t \;+\; \varepsilon^g_{i,f,s,t} \tag{5}$$

where $g \in \{\text{women}, \text{men}\}$ and the model is estimated separately by sex. The indicator $\text{Treated}^g_{f,s}$ equals one for sex-firm-state cells that are ever effectively treated (i.e., those for which the minimization set in Equation 2 is non-empty), so that $T^{\text{start}}_{g,f,s}$ is well defined, and zero otherwise. For ever-treated cells, event time $k_{i,t} = t - T^{\text{start}}_{g,f,s}$ is the number of months relative to the sex-specific effective treatment start date, and the policy-change month ($k_{i,t} = 0$) is excluded from the sample. Event time is aggregated into the non-overlapping 12-month bins $B = \{[-36,-25], [-24,-13], [1,12], [13,24], [25,36]\}$, with the reference bin $[-12,-1]$ omitted; the coefficients β^g_b capture deviations in departure rates from the reference period for sex g. For never-effectively-treated cells, $\text{Treated}^g_{f,s} = 0$, so every term $\text{Treated}^g_{f,s}\,\mathbf{1}\{k_{i,t} \in b\}$ in the regression is identically zero and $k_{i,t}$ need not be defined; these cells enter the regression only through the firm-state fixed effects $\alpha^g_{f,s}$ and month-year fixed effects λ^g_t. Standard errors are clustered at the firm-state level within each sex subsample.

Figure 3 presents event-study estimates that trace the dynamic effects of effective PPL treatment on monthly employee departures, estimated separately for men and women. The coefficients plot estimates for 12-month bins relative to the reference period (-12 to -1 months), which is normalized to zero. Horizontal dashed lines indicate pre-treatment averages (periods -36 to -12) and post-treatment averages (periods 1 to 36) for each sex.

The event study reveals distinct pre-treatment patterns by sex. Men's departure rates are flat and statistically indistinguishable from zero prior to effective treatment (point estimates of $+0.012$ and $+0.034$ percentage points at 25–36 and 13–24 months pre-treatment, both with $p > 0.10$), demonstrating parallel pre-trends. Given the sample of 107,627,981 men-months, this is a well-powered null rather than an artifact of limited statistical power. Women, in contrast, exhibit elevated and statistically significant departure rates in the pre-treatment periods ($+0.067$ and $+0.050$ percentage points; both $p < 0.001$). This asymmetric pattern is consistent with strategic policy adoption in response to elevated female turnover: firms expanding PPL following a period of elevated departures among the targeted group. However, I cannot rule out that other unobserved factors drive both higher pre-treatment departures among women and subsequent firm-level policy expansions.

Following effective treatment, both men and women show significant reductions in monthly departures of similar magnitude. Men's departure rates fall by 0.043, 0.061, and 0.055 percentage points at 1–12, 13–24, and 25–36 months post-treatment (all $p < 0.05$), representing approximately a 3.4% decline relative to men's pre-period departure mean. Because men's pre-trend is flat and well-powered, this decline cannot be attributed to a pre-existing trajectory. Women's post-treatment departure rates also decline (to roughly -0.04 percentage points across the three post bins, approximately a 3.5% decline relative to women's pre-period mean), a reversal from their elevated pre-treatment levels. While the women estimate is potentially confounded by the pre-existing trend, the men's estimate—in a group that is neither the primary user of PPL nor a plausible channel for selection into firm policy adoption—provides the cleaner causal benchmark.

The event study also clarifies the null sex interaction in the pooled specification: the underlying retention response is essentially symmetric in aggregate, so the pooled interaction is small not because of compositional differences across event-time bins or features of the reference period. The mechanisms underlying these aggregate effects, and how they vary across workers and environments, are examined in the heterogeneity analysis that follows.

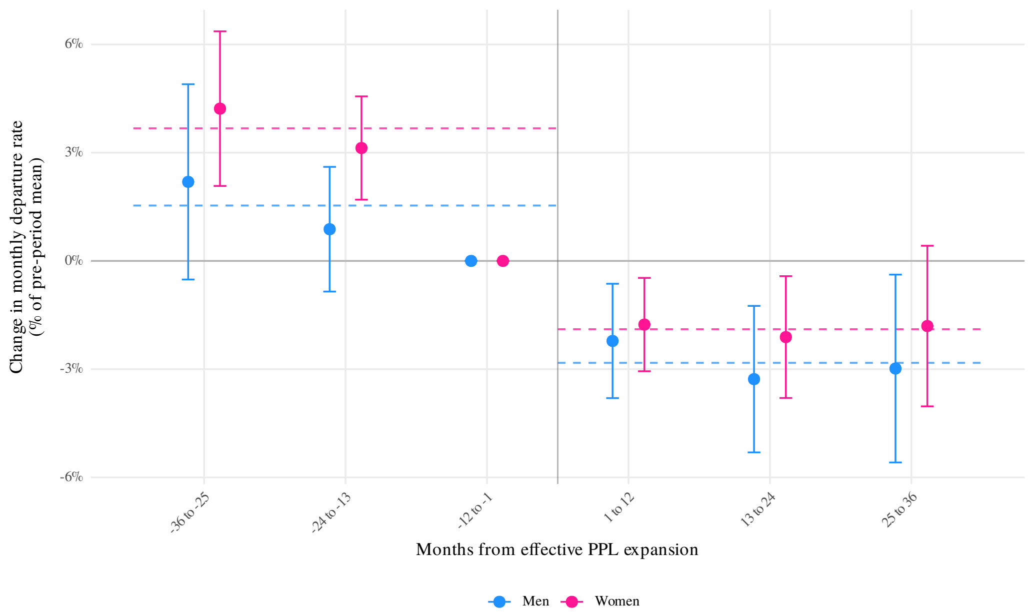

Figure 3: Event-Study Estimates of Departure Rates by Sex Around Effective PPL Expansion

Notes: This figure displays coefficients from binned event-study regressions estimated separately for men (blue) and women (pink), as specified in Equation 5. Event time is defined relative to the sex-specific effective treatment start date and aggregated into non-overlapping 12-month bins; the month of effective treatment ($k = 0$) is excluded. Coefficients are expressed as a percentage of each sex's pre-period departure mean (i.e., the raw percentage-point coefficient divided by the mean monthly departure rate among observations with $\text{EffectiveTreatment} = 0$ in that sex's estimation sample). The omitted reference bin is $[-12, -1]$ months. Each specification includes firm-state and month-year fixed effects; never-effectively-treated and not-yet-treated sex-firm-state cells enter as comparison observations through the fixed effects. Horizontal dashed lines show the mean of the two estimated pre-treatment bin coefficients ($[-36, -25]$ and $[-24, -13]$) and the mean of the three post-treatment bin coefficients ($[1, 12]$, $[13, 24]$, and $[25, 36]$), separately for each sex. Vertical bars represent 95% confidence intervals with standard errors clustered at the firm-state level within each sex subsample.

6.3 Heterogeneity by Worker Characteristics

This section evaluates the worker-level heterogeneity in retention effects by estimating 4 separately across worker characteristics capturing economic position and organizational role, including salary, education, seniority, and age. This analysis is done separately for each subgroup as opposed to in a triple-differences estimation to allow for potential nonlinearities in retention effects by subgroup.

6.3.1 Salary

Table 6 examines retention heterogeneity by salary quartile, capturing workers' economic position. The effect for men is negative and statistically significant across all salary quartiles, with no strong evidence of a systematic gradient. In contrast, women's differential retention response varies across the salary distribution. In the lowest salary quartile, the total treatment-induced reduction in departures for women ($-0.092 + (-0.141) = -0.233$ percentage points) represents an 8.0% reduction relative to the mean departure rate for that quartile. At the highest salary quartile, the positive and significant interaction term ($+0.101$) nearly offsets the main retention effect (-0.120), yielding a negligible total effect for women ($-0.120 + 0.101 = -0.019$ percentage points), compared to a 9.0% reduction for men.

Table 6: Monthly Departure Regressed on Effective Treatment × Woman, Results by Salary Quartile

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
EffectiveTreatment	-0.092^{**}	-0.077^{**}	-0.061^{*}	-0.120^{***}
	(0.031)	(0.026)	(0.027)	(0.024)
Woman × EffectiveTreatment	-0.141^{**}	0.055	0.014	0.101^{**}
	(0.048)	(0.035)	(0.034)	(0.034)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	2.918	2.059	1.971	1.326
SD (departure)	16.832	14.199	13.901	11.441
Num. obs.	32,776,338	32,776,572	32,776,133	32,775,996
Num. clusters	10,845	11,481	11,151	9,147

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and firm's previous policy, calculated separately by sex. Salary quartiles computed within-sample. Standard errors clustered at firm-state-sex level.

6.3.2 Seniority

Table 7 shows a pattern of results by seniority that mirrors those by salary quartile. *EffectiveTreatment* coefficients are negative across all seniority levels and statistically significant at most (Entry, Associate, Manager, Director, and Executive). Only Junior and Senior Executive fail to reach conventional significance, the latter likely reflecting lower statistical power given the small number of Senior Executive observations. The *Woman* × *EffectiveTreatment* interaction reveals a clear gradient: women experience an additional, statistically significant retention benefit at the most junior level (-0.094^{**}), which attenuates and becomes indistinguishable from zero through the manager level, then reverses and becomes significantly positive at the Director ($+0.098^{**}$) and Executive ($+0.114^{*}$) levels. This pattern is consistent with the mechanism framework: at junior levels, the insurance and return-to-work value of PPL amplifies women's retention response, while at senior levels, career disruption costs and the attenuation predicted for high-pay, high-seniority women offset the broad retention mechanisms, reducing women's net retention response relative to men's. The consistency of this gradient across both salary and seniority—two correlated but distinct worker characteristics—strengthens the mechanism interpretation.

6.3.3 Education

Results by highest level of educational attainment are shown in Table 8. The bachelor's degree subsample, by far the largest at over 70 million observations, yields the most precisely estimated effect: PPL expansions reduce monthly departures by 0.081 percentage points ($p < 0.001$). The MBA subsample also yields a significant negative estimate

Table 7: Monthly Departure Regressed on Effective Treatment × Woman, Results by Seniority Level (1 = most junior, 7 = most senior)

	1	2	3	4	5	6	7
EffectiveTreatment	-0.058^{**}	-0.029	-0.075^{***}	-0.054^{*}	-0.123^{***}	-0.096^{*}	-0.120
	(0.022)	(0.018)	(0.019)	(0.023)	(0.025)	(0.038)	(0.085)
Woman × EffectiveTreatment	-0.094^{**}	0.005	0.040	0.060	0.098^{**}	0.114^{*}	0.061
	(0.031)	(0.024)	(0.027)	(0.032)	(0.036)	(0.058)	(0.153)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean (departure)	2.303	1.673	1.616	1.434	1.375	1.407	1.529
SD (departure)	14.998	12.826	12.609	11.888	11.646	11.777	12.269
Num. obs.	59,933,763	52,532,285	24,449,405	24,461,723	24,679,266	7,611,229	377,363
Num. clusters	11,908	12,042	10,834	10,417	9,825	6,122	1,937

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and firm's previous policy, calculated separately by sex. Standard errors clustered at firm-state-sex level. Seniority levels: Entry (1), Junior (2), Associate (3), Manager (4), Director (5), Executive (6), Senior Executive (7).

(-0.052, $p < 0.05$), and the master's point estimate (-0.042) is similar in magnitude though it does not cross conventional significance. The high school, associate, and doctoral categories produce statistically insignificant point estimates, but their confidence intervals are wide enough to include the bachelor's effect, so the data are consistent with similar-magnitude retention effects across education levels rather than sharply discriminating among them.

Table 8: Monthly Departure Regressed on Effective Treatment × Woman, Results by Level of Highest Educational Attainment

	High School	Associate	Bachelor	Master	MBA	Doctor
EffectiveTreatment	-0.027	-0.002	-0.081^{***}	-0.042	-0.052^{*}	-0.018
	(0.034)	(0.024)	(0.019)	(0.029)	(0.026)	(0.038)
Woman × EffectiveTreatment	-0.063	-0.051	0.019	-0.021	0.024	-0.034
	(0.048)	(0.033)	(0.026)	(0.038)	(0.036)	(0.057)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mean (departure)	2.444	1.809	2.068	2.206	1.935	2.006
SD (departure)	15.441	13.329	14.230	14.687	13.775	14.020
Num. obs.	4,999,320	9,864,804	70,408,343	23,405,128	16,441,070	6,687,891
Num. clusters	6,946	8,140	12,672	10,201	9,233	6,923

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and firm's previous policy, calculated separately by sex. Standard errors clustered at firm-state-sex level.

The absence of significant *Woman* × *EffectiveTreatment* interactions across all education levels is consistent with the prediction that sex asymmetry in retention responses is most pronounced along the pay and seniority dimensions rather than education per se. Education and salary are correlated but not identical, and education may not provide a sufficiently sharp partition to detect the attenuation mechanism predicted for high-earning women.

The retention of high-skilled women who would otherwise reduce labor supply or exit the workforce after childbearing is a common motivation for firm-level PPL expansions. This pattern is documented for Chicago Booth MBA cohorts by Bertrand et al. (2010), who show that mothers in their sample exhibit substantial post-birth labor supply declines a decade after graduation, with 17% out of the labor force entirely and another 22% employed part-time. Under that policy logic, one would expect PPL retention effects to be (a) concentrated in the credential categories where this opt-out is most prevalent, and (b) larger for women than for men within those categories. The first prediction is consistent with the table: the precisely-estimated retention effects fall in the bachelor's and MBA subsamples, but the second is not.

A sharper interpretation emerges when this null sex interaction is paired with evidence on which MBA mothers opt out.

Bertrand et al. (2010) show that opt-out is concentrated among MBA mothers with high-earning spouses, while mothers with lower-earning spouses retain near-typical labor supply post-birth. Attachment is most economically determined for mothers with low-earning spouses, which corresponds to strong retention effects observed among low-paid and low-rank women. [5] PPL appears to reinforce the attachment of women whose retention is on the financial margin, while leaving the opt-out group largely unmoved. PPL works as a marginal retention tool for the economically-attached worker rather than as an intervention that reverses the preference-driven exit decisions of women in higher-resource households. If the objective of PPL is broadly construed as retaining workers around the childbearing transition, the expansions deliver. If the objective is specifically to retain highly educated, highly paid women whose post-birth labor supply declines drive much of the gender gap in elite professional careers, these results here suggests PPL alone is unlikely to be the lever.

6.3.4 Age

Table 9 examines results by age group, and the pattern is U-shaped. The largest treatment effects appear at the youngest end of the age distribution: both men (-0.146^{*}) and women (-0.267^{*}) under age 20 show economically and statistically meaningful reductions in monthly departures.[6] Effects attenuate to zero across the prime childbearing ages 20–39, where point estimates range from $+0.001$ to -0.019 and confidence intervals exclude moderate-to-large negative effects, then return at older ages: men aged 40 and above show significant reductions of -0.070 to -0.095 percentage points, while women's estimates in this range are negative but not individually significant. *Woman* $\times$ *EffectiveTreatment* interactions are statistically insignificant across every age group.

The magnitude at ≤ 19 deserves emphasis. Standard errors for this group are the widest in the table, reflecting the smallest subsample, but the point estimates are large enough that both men and women cross conventional significance thresholds. Because the mean monthly departure rate for workers ≤ 19 is also the highest in the sample at 4.0%, the percentage-point reductions translate to a 3.7% decline in baseline departures for men and 6.7% for women.

The U-shaped pattern is difficult to reconcile with a single mechanism. Option value and income insurance should both bind most strongly through the prime childbearing years, where forward-looking fertility risk is highest and the realized income shock from childbirth would be most binding. Yet retention effects across ages 20–39 are tightly estimated near zero. At the same time, the option-value story is partially consistent with the ≤ 19 result: these workers likely have not yet used PPL, face a long forward-looking horizon over which they could utilize PPL, so the forfeiture cost of a non-transferable benefit is meaningful. What is hardest to explain through utilization-based channels is the $40+$ result: these workers are largely past childbearing, so option value and income insurance should be approaching zero, yet men in this range exhibit precisely-estimated retention effects.

A more plausible interpretation is among workers ≤ 19, both option value and signaling contribute to the retention effect: this group has not yet used the benefit and may respond strongly to a firm-quality signal. Among workers $40+$, option value, income insurance, and return-to-work facilitation are largely irrelevant and signaling appears to dominate. High accumulated tenure (mean monthly departures fall from 4.0% at ≤ 19 to 1.0% at $50+$) and lower baseline turnover make this group particularly responsive to firm-level signals about workforce treatment even when they will not personally consume the benefit. The null estimates in 20–39 may reflect the partial offset of heterogeneous channels within these cells: some workers retained by PPL's contingent-positive channels and others for whom career-disruption costs dominate. Whether the 20–39 null reflects true zero effects at peak-childbearing ages or compositional dilution across the pay and rank distribution is not separately identifiable from this cut. The pay and seniority results in Tables 6 and 7 provide the sharper tests of these mechanisms by isolating the subgroups within which the retention response concentrates.

[5] While the data do not include direct measures of spousal income, the low-paid and low-rank women in our sample plausibly correspond to the lower-spousal-income group through positive assortative mating in the labor market (Greenwood et al., 2014).

[6] The women's effect at ≤ 19 combines the EffectiveTreatment main effect and the Woman $\times$ EffectiveTreatment interaction. The interaction itself is not statistically distinguishable from zero, so men and women cannot be separated at this age, but the net women's effect is significant and roughly double the men's estimate in magnitude.

Table 9: Monthly Departure Regressed on Effective Treatment $\times$ Woman, Results by Worker Age

	$\leq$19	20–24	25–29	30–34	35–39	40–44	45–49	50+
EffectiveTreatment	-0.146^{*}	0.039	0.001	-0.010	-0.019	-0.070^{**}	-0.095^{***}	-0.083^{***}
	(0.072)	(0.037)	(0.029)	(0.024)	(0.022)	(0.023)	(0.024)	(0.023)
Woman $\times$ EffectiveTreatment	-0.121	-0.063	-0.001	0.018	0.028	0.060	0.038	0.050
	(0.102)	(0.048)	(0.039)	(0.032)	(0.030)	(0.032)	(0.036)	(0.030)
Firm $\times$ State $\times$ Sex FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year $\times$ Month $\times$ Sex FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean (departure)	4.002	3.826	2.280	1.774	1.512	1.348	1.232	1.019
SD (departure)	19.601	19.182	14.926	13.200	12.203	11.531	11.033	10.044
Num. obs.	3,798,268	17,841,406	20,699,450	17,381,602	12,988,441	9,731,648	7,337,551	11,582,439
Num. clusters	6,896	10,414	10,764	10,444	9,783	9,191	8,582	8,844

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and firm's previous policy, calculated separately by sex. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

6.3.5 Fertility

To assess whether retention responses scale with anticipated fertility, I estimate Equation 4 separately by predicted fertility quartile (see Appendix 9.3 for estimation details). If retention operates primarily through anticipated PPL utilization, workers with higher expected fertility should show stronger responses; if it operates through signaling or a broader form of option value, the effect should be diffuse across fertility levels.

Tables 10–12 report results across three stratifications: current-period fertility for workers under 40, five-year forward-looking fertility for the full sample, and five-year forward-looking fertility for workers under 40. None of the three shows a monotonic fertility gradient. Significant retention effects concentrate instead in the *lowest* fertility quartiles. In Table 10, the Woman × EffectiveTreatment interaction is -0.100^{*} in Q1 and null elsewhere, indicating women's response concentrates in the lowest current-fertility quartile. In Table 11, both men and women show significant retention effects in Q1 (-0.090^{***} for men, -0.084^{*} for women's total effect), with null effects across the other three quartiles. In Table 12, women's total effect is -0.167^{**} in Q1 (interaction -0.164^{**}), and effects are null across the other quartiles for both sexes.

This is the opposite of the utilization-driven prediction. Workers with the lowest expected five-year fertility (i.e, the group least likely to need PPL in the near term) show retention responses for both sexes in Table 11, and significant differential women's responses in Tables 10 and 12. The pattern is consistent with a broader form of option value: workers who have not yet used PPL retain the greatest forward-looking option value from a non-transferable benefit, since the benefit remains unclaimed and forfeiture risk upon departure is most binding. It is also consistent with signaling, which operates regardless of near-term fertility risk and would predict diffuse effects across the workforce. Taken together, these results weigh against the variant of option value in which retention scales with the probability of using PPL, and instead point to signaling and broad option value operating across the workforce rather than concentrating among likely users.

Table 10: Monthly Departure Regressed on Effective Treatment × Woman, Results by Current Fertility Quartile (Workers Under 40)

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
EffectiveTreatment	0.026	−0.012	−0.000	−0.024
	(0.035)	(0.025)	(0.024)	(0.023)
Woman × EffectiveTreatment	-0.100^{*}	0.013	−0.012	0.031
	(0.052)	(0.041)	(0.035)	(0.031)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	3.773	2.537	1.831	1.686
SD (departure)	19.054	15.724	13.407	12.875
Num. obs.	18,838,025	18,630,136	18,750,376	18,659,376
Num. clusters	10,483	10,659	10,608	10,233

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Sample restricted to workers under age 40. Coefficients and mean departure rate are expressed in percentage points. Fertility quartiles constructed based on predicted fertility risk. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

One caveat to this reading is that the fertility estimates capture demographic averages rather than individual subjective beliefs about future childbearing. If workers respond to private beliefs that are only weakly correlated with the demographic average, or have low awareness of their own fertility prospects more generally, then the demographic-predicted gradient is a noisy proxy for the subjective gradient that actually drives retention decisions. To the extent this is the case, the flat estimates across fertility quartiles weaken less the existence of utilization-driven retention than the existence of utilization-driven retention that workers can accurately forecast and act on. Either reading points away from a fine-grained, fertility-scaled mechanism and toward broader channels (signaling, forfeiture-based option value) that do not require accurate forward-looking forecasting.

Table 11: Monthly Departure Regressed on Effective Treatment × Woman, Results by Future Fertility Quartile (5-Year)

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
EffectiveTreatment	-0.090^{***}	-0.023	-0.000	-0.002
	(0.022)	(0.024)	(0.024)	(0.025)
Woman × EffectiveTreatment	0.006	-0.052	-0.014	0.008
	(0.031)	(0.043)	(0.034)	(0.034)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	1.508	2.892	2.180	1.906
SD (departure)	12.189	16.759	14.602	13.675
Num. obs.	25,340,545	25,377,195	25,336,361	25,307,179
Num. clusters	10,906	11,272	11,335	10,918

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. Future fertility quartiles constructed using predicted five-year cumulative fertility risk from PUMS, assigned to all workers regardless of age. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

Table 12: Monthly Departure Regressed on Effective Treatment × Woman, Results by Future Fertility Quartile (5-Year), Workers Under 40

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
EffectiveTreatment	-0.003	0.007	-0.008	-0.005
	(0.034)	(0.025)	(0.024)	(0.028)
Woman × EffectiveTreatment	-0.164^{**}	-0.018	-0.007	0.024
	(0.051)	(0.034)	(0.034)	(0.036)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	3.678	2.394	1.979	1.913
SD (departure)	18.822	15.285	13.927	13.698
Num. obs.	18,192,720	18,171,181	18,177,101	18,168,310
Num. clusters	10,724	10,964	10,965	10,352

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Sample restricted to workers under age 40. Coefficients and mean departure rate are expressed in percentage points. Future fertility quartiles constructed using predicted five-year cumulative fertility risk. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

6.4 Heterogeneity by Firm Environment

6.4.1 Firm Size and Growth Rate

Next I examine heterogeneity in the retention effects of PPL by firm environment. I focus on dimensions that proxy for organizational capacity, growth, and internal labor market conditions that are expected to condition both the magnitude of PPL's option value and signaling effects and the feasibility of return-to-work following leave.

Table 13 examines heterogeneity in retention effects by firm size, as measured by headcount from Capital IQ. The main effect of *EffectiveTreatment* is negative and statistically significant for medium (-0.313, $p < 0.05$) and very large firms (-0.061, $p < 0.001$), while estimates for small and large firms are imprecise and statistically indistinguishable from zero. The interaction term *Woman* $\times$ *EffectiveTreatment* is statistically insignificant across all size categories, implying no differential retention effect for women by firm size. These results suggest that the retention benefits of PPL expansions are present among both medium-sized and very large firms, consistent with greater organizational capacity to absorb and institutionalize expanded leave policies.

Table 13: Monthly Departure Regressed on Effective Treatment $\times$ Woman, Results by Firm Size (Headcount)

	Small (1–500)	Medium (501–5,000)	Large (5,001–10,000)	Very Large (10,000+)
EffectiveTreatment	0.141	-0.313^{*}	-0.148	-0.061^{***}
	(0.179)	(0.159)	(0.121)	(0.016)
Woman $\times$ EffectiveTreatment	-0.159	0.092	-0.004	-0.001
	(0.217)	(0.209)	(0.162)	(0.021)
Firm $\times$ State $\times$ Sex FEs	Yes	Yes	Yes	Yes
Year $\times$ Month $\times$ Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	1.870	1.645	1.890	1.783
SD (departure)	13.546	12.719	13.617	13.232
Num. obs.	381,362	3,657,903	2,236,260	187,770,003
Num. clusters	582	1,334	1,051	11,398

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Firm size categories are drawn from hand-collected policy data and reflect reported employee headcount from Capital IQ at the time of the policy change. The Small category comprises 18 firms and 381K observations and should be interpreted with caution. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

In an analysis of subgroup results by firm revenue growth quartile in the three years before and after the PPL policy change (Appendix 9.5.1), no clear patterns of heterogeneity were observed. The overall retention effect for men is negative and significant in most pre-growth and post-growth quartiles, but does not follow a discernible pattern. The differential effect for women is not statistically significant in any pre- or post-growth quartiles, indicating that firm growth environment does not appear to moderate the sex gap in retention responses to PPL expansions. These results do not support the prediction that retention effects are concentrated at low-to-moderate-growth firms.

6.4.2 Internal Labor Market Flexibility

To examine how internal labor market flexibility moderates the retention effects of PPL, I classify firms into quartiles based on the percentage of workers holding multiple positions in a given year, averaged over the three years prior to policy change, and estimate Equation 4 separately for each quartile.

The results in Table 14 show heterogeneity in retention effects by firm internal labor market flexibility, with patterns that differ by sex. For men, effective treatment significantly reduces departures in firms with moderate flexibility (Q2: -0.165^{***}; Q3: -0.090^{**}) and has null effects in the most flexible (Q1) and least flexible (Q4) quartiles. For women, total retention responses are significant in the moderately flexible Q2 (-0.172^{**}) and most prominently in the least flexible Q4 (-0.188^{***}), with null effects in Q1 and Q3. The men's pattern is broadly consistent with the theoretical prediction that option value and signaling mechanisms bind most strongly where baseline lock-in is meaningful but not overwhelming: retention effects are strongest in moderately flexible firms, where firm-specific human capital is

Table 14: Monthly Departure Regressed on Effective Treatment × Woman, Results by Internal Labor Market Flexibility Quartile

	Q1 (Most Flexible)	Q2	Q3	Q4 (Least Flexible)
EffectiveTreatment	-0.016	-0.165^{***}	-0.090^{**}	-0.049
	(0.035)	(0.037)	(0.032)	(0.026)
Woman × EffectiveTreatment	0.026	-0.007	0.049	-0.139^{***}
	(0.053)	(0.050)	(0.046)	(0.037)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	1.777	1.577	1.774	2.135
SD (departure)	13.211	12.458	13.202	14.454
Num. obs.	35,403,766	61,723,866	57,971,360	38,029,113
Num. clusters	3,758	3,981	3,688	3,273

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Internal labor market flexibility quartiles are based on the percentage of workers holding multiple titles in the three-year period preceding the policy change. The total effect for women equals the sum of the main effect and the interaction term. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

meaningful enough to make forfeiture of non-transferable benefits costly, but internal mobility is not so constrained that the signal is already fully priced in.

6.4.3 Firm-Specific Human Capital

Table 15 examines results by firm-specific human capital (FSHC) quartile. FSHC is measured as the share of workers with at least five years of tenure at the firm in the calendar year preceding the policy change, adjusted for the firm's hire rate to net out the mechanical dilution of long-tenured share at fast-growing firms. The theoretical prediction developed earlier is that firms with high FSHC will exhibit attenuated or null retention gains from PPL expansions, because the career-disruption costs of taking leave under rigid internal ladders erode the benefit's net value and leave the stay-leave threshold unchanged.

Table 15: Monthly Departure Regressed on Effective Treatment × Woman, Results by Firm-Specific Human Capital Quartile

	Q1 (Lowest FSHC)	Q2	Q3	Q4 (Highest FSHC)
EffectiveTreatment	-0.033	-0.014	-0.043	0.091^{**}
	(0.041)	(0.023)	(0.033)	(0.031)
Woman × EffectiveTreatment	0.117^{*}	-0.058	0.005	-0.063
	(0.053)	(0.030)	(0.053)	(0.049)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	1.957	1.860	1.699	1.436
SD (departure)	13.852	13.510	12.925	11.895
Num. obs.	39,943,794	68,366,259	42,228,946	31,855,109
Num. clusters	3,486	3,844	4,063	3,446

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Firm-specific human capital quartiles are based on the share of workers with at least five years of tenure at the firm in the calendar year preceding the policy change, adjusted for the firm's hire rate to net out the mechanical dilution of long-tenured share at fast-growing firms. The total effect for women equals the sum of the main effect and the interaction term. Standard errors clustered at firm-state-sex level.

The Q4 result is the only departure-positive coefficient in the paper. Within the framework developed in Section 2, high-FSHC firms have structural features that could plausibly amplify the backlash channel for men and the career-disruption channel for women: specialized roles, rigid tenure-based promotion ladders, and heavy weight on accumulated firm-specific capital. Q4 firms also have a somewhat lower female share than firms in other quartiles (39.9% vs 44.6% at pre-policy baseline, $t = 2.11$, $p = 0.038$).

For men in Q4 firms, the option value channel is attenuated because they are already locked in by accumulated firm-specific capital, so the marginal forfeiture cost of a non-transferable PPL benefit is small relative to the substantial forfeiture costs they would already absorb by leaving. Whatever signaling benefit PPL provides is evidently insufficient to offset whatever additional cost men face in this environment. The result is a small positive departure response.

For women at Q4, the women's total effect is $+0.028$, statistically indistinguishable from zero. Women show null total responses across all FSHC quartiles, consistent with FSHC being a firm-level variable that does not isolate the individual worker characteristics (e.g., low pay and low rank) where women's retention gains concentrate elsewhere in the paper.

At lower FSHC quartiles, both men's and women's effects are statistically indistinguishable from zero. The Q1 sex interaction is positive and significant ($+0.117^*$), though neither sex's absolute effect reaches significance there; this may reflect weakened lock-in at low-FSHC firms where portable skills make non-transferable benefits less binding, or selection effects in low-FSHC firms expanding PPL in response to elevated women's turnover.

6.5 PPL Policy Length

Retention effects may also depend on the length and incremental generosity of PPL policies. To assess this, I first estimate Equation 4 with results stratified by the number of PPL weeks provided under the new policy, measured separately by sex using weeks offered to mothers and other birthing parents and weeks offered to fathers and other non-birthing parents. This reflects that most firms specify PPL entitlements separately by birthing status.

Table 16: Monthly Departure Regressed on Effective Treatment × Woman, Results by Sex-Specific New Policy Weeks

	1–4	5–8	9–12	13+
EffectiveTreatment	-0.062^*	0.023	-0.070^*	-0.141^{**}
	(0.025)	(0.036)	(0.034)	(0.044)
Woman × EffectiveTreatment	0.060	-0.102	0.101^*	0.105^*
	(0.069)	(0.057)	(0.044)	(0.049)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	1.717	1.846	1.763	1.785
SD (departure)	12.989	13.460	13.160	13.242
Num. obs.	38,739,662	39,308,710	44,584,321	65,668,248
Num. clusters	2,564	3,544	3,363	5,397

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. New policy weeks are sex-specific: weeks offered to women for female workers and weeks offered to men for male workers. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

Table 16 shows that the estimated retention effect for men is negative and statistically significant at 1–4 weeks (-0.062^*), 9–12 weeks (-0.070^*), and 13+ weeks (-0.141^{**}), with no significant effect at 5–8 weeks. The pattern is non-monotonic but the largest effect concentrates at the longest policy lengths. The total retention effect for women, combining the main effect and the *Woman* × *EffectiveTreatment* interaction, is essentially zero at every policy length: -0.002, -0.079, $+0.031$, and -0.036 across the four bins. The significantly positive interactions at 9–12 and 13+ weeks ($+0.101^*$ and $+0.105^*$) reflect this: women's departure reductions are sharply attenuated relative to men's at the most generous policy lengths, leaving the net women's effect near zero.

This appears to show that policy-length-keyed retention response accrues almost entirely to men. The divergence at the longest policy lengths is consistent with the theoretical prediction that absence-related costs scale with leave duration: because men take substantially shorter leaves even when longer durations are available (Jack et al., 2025), the career-disruption costs that offset PPL's retention-positive channels load disproportionately on women, leaving men to benefit from the signaling and option value of generous policies without incurring the absence costs that women bear when they use the leaves the policies entitle them to take. The duration-scaling argument does not fully explain the null women's effect at 1–4 weeks, where realized absence costs should be modest.

Appendix Section 9.5.2 examines retention effects by the magnitude of the PPL increase. The results are null across increase magnitudes, suggesting the level of the new policy matters more for retention than the size of the increase relative to prior coverage.

6.6 Retention Effects of Firm-Provided PPL vs. Public PPL Entitlements

The theoretical framework predicts that firm-provided and public PPL expansions should generate different retention responses. Firm-provided PPL is not portable: workers forfeit these benefits upon departure, creating an option value that ties them to the firm. Public PPL, in contrast, is portable across employers within the same state and therefore does not penalize most job mobility, since intrastate moves account for the large majority of job-to-job transitions (Azzopardi et al., 2020). If the retention effects documented in the preceding sections operate through option value and organizational signaling rather than through the general welfare effects of having access to PPL, firm-provided PPL should reduce departures while public PPL should not. The structure of the U.S. institutional environment provides a natural setting to test this prediction, as both firm-level policy expansions and state-level program introductions occur during the sample period, and the staggered timing of each allows both to be estimated within the same difference-in-differences framework.

Unlike earlier specifications that excluded observations where state policy changes occurred within ± 36 months of firm policy changes, these specifications retain all observations and allow workers to be treated by whichever source (firm or state) first increases their PPL entitlement beyond their prior maximum.

$$\begin{aligned}\text{Departure}_{i,f,s,t} = \beta_0 &+ \beta_1 \text{Woman}_i + \beta_2 \text{EffectiveTreatmentFirm}_{g,f,s,t} \\ &+ \beta_3 (\text{Woman}_i \times \text{EffectiveTreatmentFirm}_{g,f,s,t}) \\ &+ \beta_4 \text{EffectiveTreatmentState}_{g,s,t} \\ &+ \beta_5 (\text{Woman}_i \times \text{EffectiveTreatmentState}_{g,s,t}) \\ &+ \alpha^g_{f,s} + \lambda^g_t + \varepsilon_{i,f,s,t}\end{aligned} \tag{6}$$

$\text{EffectiveTreatmentFirm}_{g,f,s,t}$ equals 1 when firm f's new PPL policy for sex g exceeds the worker's prior entitlement from either the firm's previous policy or current state benefits, defined as $\text{PriorEntitlement}_{g,f,s,t-1} = \max\{\text{FirmWeeks}_{g,f,t-1},\ \text{StateWeeks}_{g,s,t-1}\}$. $\text{EffectiveTreatmentState}_{g,s,t}$ equals 1 when state s's PPL benefits for sex g first exceed the worker's prior entitlement from either source. Both indicators switch on permanently once activated and are not mutually exclusive: workers who experience both a state and a firm expansion contribute to both estimates. In practice, the overlap is limited: 11.3% of firm-treated observations are also state-treated. The specification includes firm-state-sex fixed effects ($\alpha^g_{f,s}$) and month-year-by-sex fixed effects (λ^g_t), which absorb β_1. Standard errors are clustered at the firm-state-sex level. By estimating both treatment indicators jointly, β_2 captures the firm PPL retention effect conditional on state treatment status, and β_4 captures the state PPL retention effect conditional on firm treatment status, enabling a formal comparison of retention responses to firm versus public PPL.

Table 17 presents the results. Columns (1) and (2) estimate firm and state treatment effects separately, while column (3) estimates both jointly on the same sample. Firm PPL expansions significantly reduce monthly departures by 0.053 percentage points in the joint specification, consistent with the estimates in Table 5 despite the expanded sample. The sex interaction is small and insignificant. State PPL expansions, by contrast, show no significant retention effect. The point estimates for state treatment are positive (0.041 for men, an additional 0.079 for women), suggesting if anything a tendency toward increased departures, but neither reaches conventional significance levels. The state treatment coefficients are identified from only 20 state-by-sex cells, so the large standard errors reflect limited statistical power rather than necessarily a precise zero.

This divergence in retention effects likely reflects fundamental differences in how firm and public PPL benefits differentially affect worker mobility. Firm PPL benefits are tied to the focal employer and create incentives for employees to remain. Public benefits, on the other hand, are portable across employers within the state. Rather than tying workers to their current firm, state PPL may reduce barriers to job mobility by ensuring that workers retain PPL benefits even after changing employers. For workers who were considering departure but concerned about losing benefits, state

Table 17: Monthly Departure: Firm vs. State PPL Expansions

	Firm Only	State Only	Joint
EffectiveTreatmentFirm	-0.074^{***}		-0.073^{***}
	(0.016)		(0.016)
Woman:EffectiveTreatmentFirm	0.020		0.020
	(0.024)		(0.023)
EffectiveTreatmentState		0.042	0.034
		(0.037)	(0.037)
Woman:EffectiveTreatmentState		0.069	0.074
		(0.062)	(0.063)
Mean (departure)	1.805	1.805	1.805
SD (departure)	13.312	13.312	13.312
Num. obs.	219,145,249	219,145,249	219,145,249
Num. groups (firm–state–sex)	15,796	15,796	15,796
Num. groups (year–month–sex)	546	546	546
R^2 (full model)	0.003214	0.003212	0.003214
R^2 (proj model)	0.000002	0.000001	0.000003
Adj. R^2 (full model)	0.003139	0.003138	0.003140
Adj. R^2 (proj model)	0.000002	0.000001	0.000003
Fixed Effects		$\alpha^g_{f,s} + \lambda^g_t$	
Clustering		Firm–state–sex	

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. Columns (1) and (2) estimate firm and state treatment effects separately; column (3) estimates both jointly. Firm PPL treatment = 1 when firm provides PPL weeks exceeding worker's prior entitlement from either firm or state. State PPL treatment = 1 when state provides PPL weeks exceeding worker's prior entitlement from either source. State treatment is identified from 20 state-by-sex cells. Excludes observations at each treatment start date ($k = 0$). All specifications estimated on the same sample.

PPL may enable transitions they previously would have avoided. The null state PPL result is therefore consistent with multiple channels, including benefit portability, lower replacement rates, or both.

6.7 Firm PPL Retention Effects in the Presence of Public PPL

Next I examine whether the retention effects of firm-provided PPL differ across jurisdictions with and without public PPL programs. If firm PPL retains workers primarily through option value, the effect may be weaker in states with public programs, where workers already hold a portable baseline entitlement that reduces the marginal value of firm-specific benefits. Alternatively, public programs may strengthen firm PPL retention effects by normalizing leave-taking and reducing stigma costs associated with benefit usage.

Estimating Equation 4 separately for states with and without public programs would mechanically confound firm PPL policy length with public PPL coverage, since effective treatment is only positive when firm-provided leave exceeds the state entitlement. Instead, I estimate a pooled specification that interacts the treatment variables with an indicator for public PPL presence, allowing the data to identify whether firm PPL effectiveness varies across state policy environments while holding the treatment definition constant:

$$\begin{aligned} \text{Departure}_{i,f,s,t} = \beta_0 &+ \beta_1 \, \text{Woman}_i + \beta_2 \, \text{EffectiveTreatment}_{g,f,s,t} \\ &+ \beta_3 \, (\text{Woman}_i \times \text{EffectiveTreatment}_{g,f,s,t}) \\ &+ \beta_4 \, (\text{EffectiveTreatment}_{g,f,s,t} \times \text{StatePPL}_{s,t}) \\ &+ \beta_5 \, (\text{Woman}_i \times \text{EffectiveTreatment}_{g,f,s,t} \times \text{StatePPL}_{s,t}) \\ &+ \alpha^g_{f,s} + \lambda^g_t + \varepsilon_{i,f,s,t} \end{aligned} \tag{7}$$

$\text{EffectiveTreatment}_{g,f,s,t}$ is defined as in Equation 3. $\text{StatePPL}_{s,t}$ equals 1 if state s has an active public PPL program in month t, and 0 otherwise. The specification includes firm-state-sex fixed effects ($\alpha^g_{f,s}$) and month-year-by-sex fixed effects (λ_t^g), which absorb β_1 (the StatePPL main effect) and β_3 (the Woman $\times$ StatePPL interaction), since these vary only at the state-time or sex level. Standard errors are clustered at the firm-state-sex level.

The coefficient β_2 measures the firm PPL retention effect of effective treatment for men in states without public PPL, while $\beta_2 + \beta_4$ gives the corresponding effect in states with public programs. Similarly, β_3 captures the additional retention effect for women relative to men in states without public PPL, and $\beta_3 + \beta_5$ gives this differential in states with public programs. The key coefficients are β_4 and β_5: β_4 tests whether firm PPL has different overall retention effects in the presence of public programs, while β_5 tests whether the sex differential varies by public program presence.

Table 18: Firm PPL Retention Effects by Public Program Presence

	No State PPL	Has State PPL	Pooled
	(1)	(2)	(3)
EffectiveTreatment	-0.053^{***}	-0.143	-0.051^{***}
	(0.015)	(0.075)	(0.015)
Woman × EffectiveTreatment	-0.021	0.094	-0.014
	(0.020)	(0.100)	(0.021)
EffectiveTreatment × StatePPL			-0.081
			(0.067)
Woman × EffectiveTreatment × StatePPL			0.079
			(0.092)
Mean (departure)	1.744	1.922	1.782
SD (departure)	13.089	13.729	13.228
Num. obs.	152,774,488	41,271,127	194,045,615
Num. groups (firm–state–sex)	13,216	1,145	14,299
Num. groups (year–month–sex)	546	468	546
Fixed Effects		$\alpha_{f,s}^g + \lambda_t^g$	
Clustering		Firm–state–sex	

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. Columns (1) and (2) estimate Equation 4 separately for states without and with active public PPL programs. Column (3) pools all observations and interacts treatment variables with a StatePPL indicator; the StatePPL main effect and Woman × StatePPL interaction are absorbed by sex-specific fixed effects. Public PPL states contribute 1,145 of 14,299 firm-state-sex groups. Excludes observations at treatment start date ($k = 0$).

Table 18 reports the results. Columns (1) and (2) estimate the baseline specification separately for states without and with public PPL programs, while column (3) pools all observations and tests whether treatment effects differ through interaction terms. Firm PPL significantly reduces departures in states without public programs (-0.053^{***}). The point estimate in public PPL states is larger in magnitude (-0.143, $p \approx 0.06$) and marginally significant, suggestive of stronger retention effects for men where public coverage exists.

However, the larger estimate in column (2) likely reflects differences in policy generosity rather than a moderating role of public programs. Because *EffectiveTreatment* is only activated when firm-provided leave exceeds the state entitlement, treated firms in public PPL states offer systematically longer leave by construction. This generosity-selection mirrors the heterogeneity documented in Table 16, where longer policies produce strong retention effects for men but attenuated effects for women. The positive *Woman* × *EffectiveTreatment* interaction in column (2) ($+0.094$) is consistent with that attenuation: women's total retention effect in public PPL states is $-0.143 + 0.094 = -0.049$, similar in magnitude to the baseline men's effect in column (1).

6.8 The Interaction of PPL with Other Benefits

Some firms introduce PPL policy changes in isolation, while others implement PPL expansions as part of a broader set of benefit updates. During the data collection process, any firm benefit changes concurrent with the PPL change were noted. Firms were divided into the following categories based on concurrent benefit changes: (1) no other benefit changes, (2) other, family-friendly benefit changes, and (3) unrelated benefit changes. Examples of accompanying benefit changes observed in the sample considered family friendly include adoption and surrogacy assistance, fertility benefits, flexible ramp-back programs, childcare support, expanded caregiving leave, and breast milk shipping. Some of these benefits are economically complementary to PPL: fertility and adoption assistance increase the likelihood of leave-taking, raising the expected option value of PPL, while ramp-back programs and childcare support reduce the career disruption and financial costs associated with extended absences, making PPL more viable to use. Beyond direct complementarity, the bundling of family-friendly benefits may amplify the organizational signaling channel by more credibly conveying firm commitment to supporting working parents. Examples of unrelated benefit changes observed in the data are 401(k) contribution matching, special one-time bonuses, student loan repayment programs, adjustments to vacation policies, and expansions to military leave. The specification in Equation 4 was estimated for each of these subgroups and the results are shown in Table 19.

Table 19: Monthly Departure Regressed on Effective Treatment × Woman, by Type of Concurrent Benefit Changes

	No Other Benefit Changes (1)	Family-Friendly Benefit Changes (2)	Unrelated Benefit Changes (3)
EffectiveTreatment	-0.061^{**}	-0.090^{**}	-0.021
	(0.019)	(0.028)	(0.043)
Woman × EffectiveTreatment	0.017	-0.117^{**}	0.020
	(0.024)	(0.037)	(0.067)
Firm × State × Sex FEs	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes
Mean (departure)	1.795	1.748	1.802
SD (departure)	13.278	13.107	13.303
Num. obs.	138,937,002	56,269,472	16,913,390
Num. clusters	10,561	2,567	1,725

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Column (1) includes firms with PPL changes only; column (2) includes firms that also enhanced family-friendly benefits (e.g., adoption assistance, fertility coverage, caregiver leave); column (3) includes firms with unrelated benefit changes (e.g., 401(k) matching, vacation policy, student loan assistance). Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

The results show that retention effects of PPL policy expansions exist when PPL expansions are made in isolation, and are most pronounced when accompanied by other family-friendly benefits. The coefficient on *EffectiveTreatment* in column (1) indicates that isolated PPL expansions reduce monthly departures by 0.045 percentage points. This effect nearly doubles to 0.091 percentage points when PPL is implemented alongside complementary family-friendly benefits (column 2). The effect is smallest and statistically insignificant for firms with unrelated benefit changes (column 3), though the smaller sample size (17 million observations) limits statistical power.

Notably, the interaction term *Woman* × *EffectiveTreatment* is negative and statistically significant only in column (2), suggesting that women experience differential retention effects when PPL is part of a broader family-benefits strategy. The total effect for women in this specification is -0.207 percentage points (combining the main effect and interaction term), representing an 11.9% reduction relative to the baseline departure rate.

Several of the family-friendly benefits in column (2) are economically complementary to PPL rather than merely reinforcing a common signal. Fertility and adoption assistance increase the likelihood of leave-taking, raising the expected option value of PPL. Flexible ramp-back programs and childcare support reduce the career disruption and financial costs associated with extended absences, making PPL more viable to use. This direct complementarity may explain why the *Woman* × *EffectiveTreatment* interaction is significantly negative only in column (2): benefits that mitigate the absence costs that otherwise attenuate women's retention gains allow the return-to-work and insurance channels to operate more fully. Beyond direct complementarity, the bundling of family-friendly benefits may amplify the organizational signaling channel by more credibly conveying firm commitment to supporting working parents.

7 The Cost-Benefit Tradeoff of Firm PPL Expansions

While the preceding sections have shown when and how PPL expansions reduce departures, it is yet unclear whether these retention gains justify or exceed the cost of providing the benefit. PPL expansions generate direct expenditures through benefit payments to leave-takers, but they also produce savings by reducing costs associated with employee turnover. Whether the net effect is positive depends on the magnitude of retention gains, worker replacement costs, and PPL usage.

7.1 Eligibility

Worker eligibility for PPL scales program costs but not its benefits. On the cost side, eligibility determines which workers at the focal firm can utilize PPL, and therefore impacts estimated PPL program expenditures for firms. On the benefit side, the retention effects documented earlier operate across the entire workforce, including employees who are not themselves eligible for leave. This asymmetry implies that firms with lower eligibility rates may achieve more favorable cost-benefit ratios because they capture broad-based retention gains while incurring costs only for the subset of workers who qualify for and take leave.

Firms commonly impose tenure requirements for PPL eligibility; when no requirement is explicitly stated, I assume a default threshold of 12 months of employment. Figure 7 presents the distribution of employee eligibility rates across firms in the sample at the time of policy implementation. Variation in eligibility implies that PPL policies of the same length can generate substantially different cost profiles across firms depending on how much of the workforce qualifies for the benefit.

7.2 Total Annual PPL Program Cost

I use imputed worker fertility to estimate the expected annual total cost of PPL under the expanded firm policies. Total cost is calculated assuming full take-up of the expanded firm policy among eligible workers who have a child in expectation, with public PPL programs (when and where active) offsetting the firm's cost. Only employees who satisfy the firm's tenure-based eligibility requirements contribute to the expected cost; ineligible employees are assigned zero cost. Each worker in the sample is assigned an expected fertility rate each year. Fertility rate is expressed as expected births per 1,000 individuals, following standard demographic practice. These rates are imputed from demographic-specific fertility estimates in the U.S. Census Bureau's Public Use Microdata Sample (PUMS) and matched to workers in the sample based on sex, age group, education, and year of observation (see Appendix 9.3 for details).

I assume all eligible workers take the full leave amount available to them under the firm's PPL policy. Employees who do not satisfy tenure requirements for PPL eligibility as of period 1 are assigned zero cost, as they would not be able to access the benefit.

For each eligible employee i at firm f in state s, let $\tilde{w}_i \equiv w_i/52$ denote the worker's weekly wage and $b_{i,s} \equiv \min(r_s\tilde{w}_i, \bar{b}_s)$ the state's weekly benefit, where r_s is the state wage replacement rate and $\bar{b}_s$ the state weekly benefit cap. The firm's cost per leave episode under the post-expansion policy decomposes into two regions:

$$\text{Cost}^{\text{new}}_{i,f} = \underbrace{\left[\text{FirmWeeks}^{\text{new}}_{g,f} - \text{StateWeeks}_{g,s}\right]_+ \cdot \tilde{w}_i}_{\text{firm pays full wage}} + \underbrace{\min\!\left(\text{FirmWeeks}^{\text{new}}_{g,f},\, \text{StateWeeks}_{g,s}\right) \cdot \left[\tilde{w}_i - b_{i,s}\right]_+}_{\text{firm tops up state benefit}}, \tag{8}$$

where $g \in \{\text{women}, \text{men}\}$ is the worker's sex and $[x]_+ \equiv \max(x, 0)$. The first term covers weeks the firm's policy extends beyond state coverage, during which the firm pays the entire weekly wage; the second covers weeks of overlap with the state program, during which the firm tops the state benefit up to the worker's full wage (or pays zero if state benefits already meet or exceed the worker's wage). FirmWeeks and StateWeeks are defined as in Equation 1.

Each employee's expected annual PPL cost weights the cost per episode by expected births (fertility divided by 1,000) and eligibility:

$$\text{Expected Annual Cost}_{i,f} = e_i \cdot \frac{\text{Fertility Rate}_i}{1{,}000} \cdot \text{Cost per Leave Episode}^{\text{new policy}}_{i,f}, \tag{9}$$

where $e_i \in \{0, 1\}$ indicates whether employee i meets the firm's tenure-based eligibility threshold.

Firm-level annual PPL cost per employee is the simple mean across all workers:

$$\text{Cost per Employee}_f = \frac{1}{N_f}\sum_{i=1}^{N_f} \text{Expected Annual Cost}_{i,f}, \tag{10}$$

where N_f is the total number of employees at firm f at period 1, the first month following the PPL policy change.

Table 20 reports key drivers of the estimated mean annual cost of PPL per employee, with firms grouped into quartiles by that cost. Firms in higher-cost quartiles tend to offer longer PPL durations and employ higher-wage workers with higher expected fertility.

Table 20: PPL Cost Drivers by Firm Cost Quartile (Cost per Employee)

Cost Quartile	N Firms	Mean Annual Cost per Employee ($)	Mean Annual Employee Salary ($)	Mean Fertility (per 1,000)	Mean PPL Weeks (Primary)	Mean PPL Weeks (Secondary)	% Women	% Eligible
Q1 (Lowest)	53	299	69,278	68.2	7.6	3.8	45.3	70.0
Q2	53	720	88,632	80.8	11.9	5.7	42.3	77.8
Q3	52	1,251	99,084	83.7	16.7	10.1	41.3	75.4
Q4 (Highest)	53	2,365	117,506	91.0	20.1	16.8	38.7	73.8

Note: Quartiles are defined by firm-level mean annual PPL cost per employee. All means are calculated across all employees as of the first month after the PPL policy change is implemented. Mean Annual Cost per Employee is the post-expansion firm-level cost net of state-provided benefits, and includes non-eligible employees (assigned $0). Mean Annual Employee Salary is the firm-level average across all employees. Mean Fertility is the firm-level mean of imputed annual fertility rates (births per 1,000 workers), based on PUMS estimates matched on sex, age group, education, and year. Mean PPL Weeks (Primary) and Mean PPL Weeks (Secondary) report firm-level policy lengths for primary and secondary parents, averaged across firms in the quartile. % Women and % Eligible report the share of employees in each category.

7.3 Incremental Cost of PPL Expansions

I next estimate the incremental cost of PPL expansions, defined as the additional expected cost incurred by the firm moving from the pre-expansion policy to the post-expansion policy. Incremental cost depends on the magnitude of the policy change and on the share of employees eligible to utilize the expanded benefit.

I calculate incremental costs using the workforce composition immediately following policy implementation (the first month following policy adoption). For each employee, I evaluate both the old policy cost (what the firm would have paid under the pre-expansion policy) and the new policy cost (what the firm pays under the post-expansion policy) using the employee's characteristics (i.e., salary, tenure, and fertility) as of the first month following policy adoption. This approach ensures consistent comparison: the same employee evaluated under two different policy regimes.

As with the total cost calculation, I assume all eligible workers take the full leave amount available to them under the expanded firm PPL policy. Employees who do not satisfy tenure requirements for PPL eligibility as of period 1 are assigned zero incremental cost under both the old and new policies, as they would not have been able to access either benefit. When the length of the firm's pre-expansion PPL policy is unobserved, I conservatively assume that the firm offered zero weeks of PPL prior to the expansion, which overstates the incremental cost.

For each eligible employee i at firm f, the expected annual incremental cost is:

$$\text{Expected Annual Incremental Cost}_{i,f} = \frac{\text{Fertility Rate}_i}{1000} \times \Delta\text{Cost per Leave Episode}_{i,f}, \tag{11}$$

where fertility rates are demographic-specific birth rates per 1,000 population, assigned based on the employee's age, sex, education level, and year of observation.

The change in cost per leave episode is defined as:

$$\Delta\text{Cost per Leave Episode}_{i,f} = \text{Cost per Episode}_{i,f}^{\text{new policy}} - \text{Cost per Episode}_{i,f}^{\text{old policy}}, \tag{12}$$

where both costs are evaluated for employee i at period 1 using their salary and eligibility status at that time.

Firm-level incremental cost per employee is calculated as:

$$\text{Incremental Cost}_f = \frac{1}{N_f} \sum_{i=1}^{N_f} \text{Expected Annual Incremental Cost}_{i,f}, \tag{13}$$

where N_f is the total number of employees at firm f immediately following the policy implementation (period = 1). By construction, this measure reflects both the size of the policy expansion and the share of the workforce eligible to access it, as ineligible employees contribute zero to the numerator.

7.4 Cost-Benefit Estimation

Next I estimate the total cost-benefit of the firm PPL expansions observed in the data by comparing incremental program costs to retention gains (separations avoided). For each firm, I apply the sex- and seniority-specific treatment effects from Table 7 to the number of employees in each sex-seniority group, then aggregate the resulting avoided departures across firms to generate total incremental costs across all firms in the sample. I use seniority because there is substantial heterogeneity in the retention effects of PPL expansions by worker seniority. Additionally, as opposed to a relative measure such as salary quartile, seniority is more attuned to the specific workforce structure of a given firm.

Total incremental costs are obtained by restricting the summation in Equation 13 to employees at each seniority level and aggregating across firms:

$$\text{Total Incremental Cost} = \sum_{f=1}^{F}\sum_{s=1}^{7}\sum_{i\in S_{s,f}} \text{Expected Annual Incremental Cost}_{i,f,t=1}, \tag{14}$$

where $S_{s,f}$ denotes the subset of employees at seniority level s at firm f. Incremental costs reflect the sex-specific PPL entitlements at each firm and assume each worker takes the full PPL to which they are formally entitled.

I translate the sex- and seniority specific retention coefficients into retention benefits using replacement costs. Table 7 reports estimates of β_2 and β_3 from Equation 4 separately by seniority level $s \in \{1, \ldots, 7\}$, which for these purposes I will denote as $\hat{\beta}_{2,s}$ and $\hat{\beta}_{3,s}$. For each seniority level, $\hat{\beta}_{2,s}$ is the monthly departure effect for men, and $\hat{\beta}_{2,s} + \hat{\beta}_{3,s}$ is the total effect for women. Let

$$\hat{\beta}_{g,s} \equiv \begin{cases} \hat{\beta}_{2,s} & \text{if } g = m, \\ \hat{\beta}_{2,s} + \hat{\beta}_{3,s} & \text{if } g = w, \end{cases}$$

denote the sex-specific monthly departure effect. Annual avoided separations for each sex and seniority level are then:

$$\text{Avoided Separations}_{g,s} = |\hat{\beta}_{g,s}| \times N_{g,s} \times 12, \tag{15}$$

where $N_{g,s}$ is the total number of employees of sex $g \in \{m, w\}$ at seniority level s at period 1.

Each avoided departure is valued using a replacement cost equal to 50% of annual salary. Total retention benefits are:

$$\text{Total Retention Benefit} = \sum_{s=1}^{7}\sum_{g\in\{m,w\}} |\hat{\beta}_{g,s}| \times N_{g,s} \times 12 \times \bar{S}_{g,s} \times 0.50, \tag{16}$$

where $\bar{S}_{g,s}$ is the mean annual salary among employees of sex g at seniority level s at period 1.

Table 21 presents the aggregate cost-benefit analysis across all firms in the sample. The analysis is conducted on the 1,366,596 employees observed at period 1 for whom age data is available, which is necessary to assign demographic-specific fertility rates from the ACS used to calculate expected PPL utilization and costs.

The total annual incremental cost of PPL expansions is $1,058.4 million. The policy expansions are estimated to have prevented 10,885 annual departures (4,738 among women and 6,147 among men), yielding total retention benefits of $467.9 million. The resulting benefit-cost ratio of 0.44 indicates that retention benefits do not fully offset program costs under the assumptions of full PPL policy utilization and replacement costs set to 50% of salary. Men account for the majority of retention benefits (73%), reflecting their substantially larger treatment effects at senior levels (seniority levels 5 and 6) where replacement costs are highest, while women show stronger effects at entry level.

The disproportionate share of retention savings attributed to men warrants careful interpretation. The cost-benefit calculation takes net retention coefficients as inputs and does not decompose them into their constituent channels. For men, the broad signaling channel and contingent-positive option value operate in the retention-positive direction across most environments, while the backlash channel is generally bounded but can become salient in specific firm

Table 21: Cost-Benefit Analysis of PPL Expansions by Worker Seniority Level

Seniority Level	Employees		Avoided Departures		Retention Benefit ($M)	
	Women	Men	Women	Men	Women	Men
1 (Entry)	204,720	189,801	3,734	1,321	72.8	27.9
2 (Junior)	150,707	239,559	434	834	18.2	40.7
3 (Associate)	69,288	115,633	291	1,041	12.7	49.3
4 (Manager)	65,700	112,258	47	727	2.5	40.3
5 (Director)	62,322	118,580	187	1,750	14.3	140.2
6 (Executive)	17,748	38,680	38	446	3.5	41.3
7 (Senior Exec.)	850	1,989	6	29	0.7	3.4
Total	571,335	816,500	4,738	6,147	124.7	343.1
Total Annual Retention Benefit						$467.9M
Total Annual Incremental Cost						$1,058.4M
Net Benefit						-$590.5M
Benefit-Cost Ratio						0.44

Note: Avoided departures calculated by applying the sex- and seniority-specific coefficients from Table 7 to the number of employees at each seniority level in period 1 (first month following policy implementation). Each avoided departure is valued at 50% of mean annual salary for that sex-seniority group. Retention benefits represent a lower bound on total benefits, excluding productivity gains, improved morale, and recruitment advantages. Cost estimates assume all workers expected to have a child take the full leave to which they are entitled, with firm costs net of any state benefits.

contexts (Table 15 shows men's coefficient flips positive in the highest-FSHC quartile, the only departure-positive estimate in the paper). For women, observed net effects reflect contingent-positive channels (option value, income insurance, return-to-work facilitation) partially offset by career disruption costs that scale with leave duration and seniority. Women's significant retention effects in the heterogeneity analysis concentrate among low-pay and low-rank workers, where the insurance and return-to-work channels bind most tightly; effects attenuate at higher salary and seniority levels, consistent with career disruption costs offsetting the contingent-positive channels in these subgroups even where they do not produce net departures. The cost-benefit framework attributes retention savings where net effects are largest, which under the seniority-by-sex pattern documented in Table 7 concentrates savings on men at senior levels and on women at entry levels, where replacement costs and treatment effects differ substantially.

These retention-based benefit estimates should be interpreted as a lower bound on the total benefits of PPL expansions. The analysis focuses exclusively on turnover reduction and does not capture other potential organizational benefits such as productivity gains, improved morale, or enhanced recruitment. The 50% replacement cost assumption is also conservative relative to estimates that place turnover costs as high as 150–250% of annual salary (Cascio, 2006). In the next section I estimate the costs and benefits to firms of PPL under different replacement cost and utilization scenarios.

7.4.1 Sensitivity Analysis

The baseline cost-benefit analysis in the preceding section assumes full utilization of PPL benefits and an employee replacement cost of 50% of annual salary. Figure 4 presents a sensitivity analysis varying both leave utilization rates (25%, 50%, 75%, and 100%) and replacement cost (50%, 75%, 100%, 125%, and 150% of salary). The heatmap displays benefit-cost ratios across these parameter combinations, with values above 1.0 indicating that retention benefits exceed program costs. Costs scale linearly with utilization rates, as lower actual leave-taking directly reduces firm expenditures. Benefits, by contrast, remain constant across utilization scenarios, as the estimated retention effects capture the value of policy availability rather than realized leave-taking.

Research from Jack et al. (2025) documents that actual PPL usage in the U.S. falls substantially short of formal entitlements. Mothers take an average of 7.2 weeks of leave, while fathers take only 0.6 weeks. Even in states with generous public PPL benefits, such as California, mothers average just 9.7 weeks despite eligibility for roughly 18–20 weeks. They find that access to public PPL increases utilization, but take-up remains incomplete: mothers in states with public PPL benefits take about 2.5 additional weeks on average, while fathers gain only 0.32 weeks. This

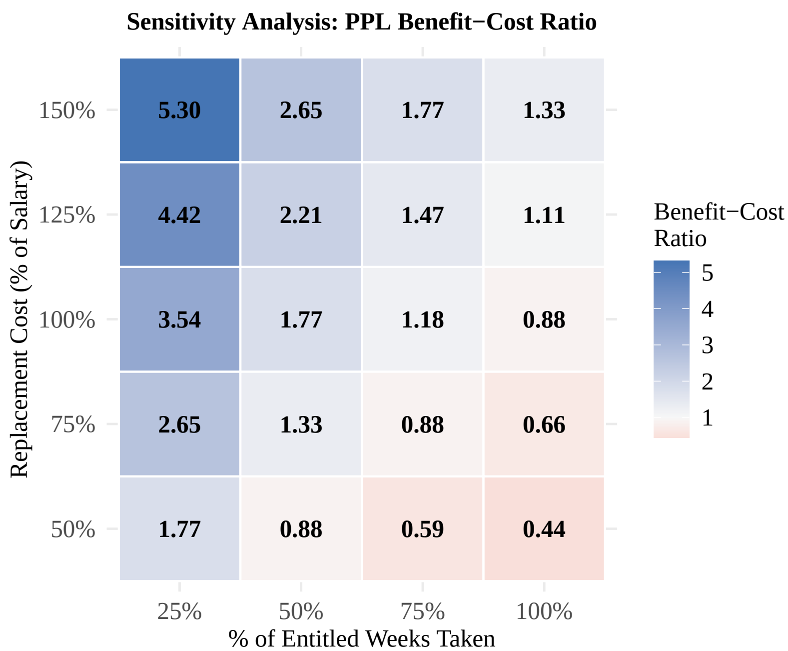

Figure 4: Sensitivity Analysis: PPL Benefit-Cost Ratio

Notes: Benefit-cost ratios are calculated varying leave utilization rates and replacement cost assumptions. Costs scale linearly with utilization; benefits are held constant, as estimated retention effects capture the value of policy availability rather than realized leave-taking. Values above 1.0 indicate retention benefits exceed program costs.

highlights both large sex gaps and substantial under-utilization of available benefits.

Translating these estimates to my sample, I assign each worker a sex- and state-matched take-up benchmark from Jack et al. (2025): 6.9 weeks for women and 0.60 weeks for men in states without public PPL, rising to 9.4 and 0.92 weeks respectively in states with public PPL. For each worker, implied utilization equals the ratio of estimated take-up weeks relative to firm entitlement, capped at one. This yields utilization rates of approximately 57% for women and 11% for men, and a weighted effective rate of roughly 29% across my full sample. At this utilization level and the assumed 50% replacement cost, the benefit-cost ratio is 1.54, more than triple the conservative full-utilization estimate of 0.44. At a 150% replacement cost, the benefit-cost ratio is 4.62. Under these realistic utilization assumptions, PPL expansions deliver positive net returns across the range of plausible replacement cost assumptions.

8 Discussion

In this paper, I study how firm PPL affects employee retention by examining changes in formal PPL entitlements and the subsequent effects on employment outcomes. This distinction is important: PPL entitlements alter expectations about future income security, job continuity, and career risk around childbirth, even when leave is not ultimately taken. As a result, the estimated effects reflect the impact of policy availability and eligibility rather than leave usage itself.

Consistent with this interpretation, the results confirm that PPL operates through multiple mechanisms rather than a single channel with varying intensity. Signaling generates positive retention effects across the workforce. Contingent-positive mechanisms (option value, insurance, return-to-work facilitation) and contingent-negative mechanisms (career disruption, backlash) load differentially by sex and organizational position. These mechanisms also load differently based on organization context, firm characteristics, and policy design features. This produces the varied retention effects observed across subgroups: a PPL policy expansion can reduce departures for some workers in some environments and increases them in others. The pattern of results is difficult to reconcile with a model in which PPL operates through a dominant channel with heterogeneous intensity, and instead supports the mechanism multiplicity framework.

For men, the signaling and option value appear to be the most impactful mechanisms. Men's significant retention effects span much of the seniority distribution and appear at both age extremes: among workers ≤ 19 who have not yet used PPL and have a long forward-looking horizon, and among workers $40+$ who are largely past childbearing and for whom option value is exhausted. The pattern at the younger end is consistent with option value (unclaimed benefit, long forfeiture horizon), though the pattern at the older end is not, and points most directly to signaling. The encome insurance and return-to-work facilitation of PPL apply less to men, who use PPL the least. The contingent-negative backlash channel appears to be generally bounded, becoming salient only in the highest- FSHC quartile (Table 15), which produces the only departure-positive coefficient in the paper.

For women, the contingent channels dominate, and the heterogeneity analysis identifies where they bind most tightly. Women's retention responses are concentrated among low-pay and low-rank workers. These are precisely the conditions where income insurance is most valuable (tight financial constraints make paid leave most binding), where informal alternatives to formal leave are least available, and where supported reintegration matters most. Women's responses attenuate to null rather than reversing at higher pay and seniority levels, consistent with career disruption costs offsetting the contingent-positive channels in these subgroups without driving net departures.

The fertility analysis is consistent with option value and signaling operating in tandem. In the under-40 stratifications (Tables 10 and 12), significant effects concentrate in the lowest fertility quartiles, among workers who have not yet had children and therefore retain the greatest forward-looking option value on a non-transferable benefit. In the full-sample stratification (Table 11), the Q1 result mixes these younger workers with older workers past childbearing, for whom option value is largely exhausted; the significant Q1 effect there is consistent with signaling carrying retention among older workers and option value among younger ones. What the fertility analysis rules out is the variant of utilization-driven option value in which retention scales positively with imminent leave-taking. That would predict the highest fertility quartiles to respond most to PPL expansions, and they do not.

The results speak to broader theories of labor mobility and firm-specific human capital. By increasing the cost of job change around childbirth and reducing uncertainty associated with temporary absence, PPL anchors workers to the focal firm, but the strength of this anchor depends on whether the mechanisms associated with leave-taking reinforce or offset retention.

While the analysis does not directly observe leave-taking, recent evidence indicates that take-up of parental leave in the United States remains far from complete, even among workers with access to paid benefits (Jack et al., 2025). The outcomes observed here are for all employees at treated firms rather than only those who utilized PPL, and future work could marry this analysis with data on actual leave-taking. The inability to observe leave-taking also limits the interpretation of sex differences. The attenuation of women's retention gains at higher seniority levels and higher income is consistent with career disruption costs, but it is also consistent with differential take-up patterns: these women may take less leave than entitled, may face greater informal penalties when they do, or may select out of firms where leave-taking is stigmatized. Disentangling whether the attenuation reflects the career costs of absence itself or the organizational response to absence requires data on realized leave usage, which this analysis cannot provide.

A strategic implication of this work relates to the durability of PPL-induced retention. Research on firm-specific human capital suggests that firms invest more in developing and retaining workers when labor mobility is restricted (Starr et al., 2018). PPL contributes to such retention infrastructure and may enhance the ability of firms to accumulate firm-specific human capital. Notably, PPL is a positive retention inducement, as opposed to a mobility restriction such as a non-compete or training repayment provision. PPL operates through a different logic: rather than constraining departure, it raises the value of staying. This distinction may make a difference in terms of the legitimacy of the retention tool (PPL is less likely to provoke legal or cultural backlash), as well as its durability (workers stay because they want to, not because they're constrained).

From a managerial perspective, these findings suggest that PPL can generate meaningful retention benefits, but that these benefits are unlikely to be uniform across firms or workers. The effectiveness of PPL depends critically on workforce composition, firm characteristics, and policy design. Firms operating in moderately flexible internal labor markets are most likely to realize retention gains, as PPL expansions increase the option value of remaining at the focal firm where career advancement occurs through meaningful but not overwhelming role transitions. Conversely, in highly flexible environments characterized by frequent internal mobility and stronger outside options, PPL expansions are associated with higher departures, as the incremental retention value of a non-transferable benefit is weakest where workers are least attached to specific roles or trajectories. Notably, the moderating effect of internal labor market flexibility appears to operate through broad retention mechanisms rather than female-specific channels, as the interaction between PPL and sex is not statistically significant across any flexibility quartile.

For firms operating in states with public PPL programs, the results carry an encouraging implication: public benefits subsidize the cost of firm-provided PPL without eroding its retention value. Firms in states with active public programs face lower incremental costs of PPL provision, as state benefits offset a portion of salary replacement, while the retention effects of firm expansions remain intact. This suggests that public PPL expansion need not discourage firm investment in private benefits, and that firms in states with public PPL programs can expand PPL at lower net cost without sacrificing the workforce attachment gains that motivate the investment. Retention gains also depend on policy design: they are largest when PPL expansions are implemented alongside complementary family-friendly benefits, and are attenuated when introduced alongside unrelated benefit changes. This is consistent with Bode et al. (2015), who find that the retention effects of firm initiatives depend on their salience and credibility; bundling PPL with complementary family-friendly benefits may increase both.

Finally, the cost analysis highlights that while PPL can be expensive in principle, its realized cost depends on demographic composition, fertility, interaction with public programs, and actual leave usage. The sensitivity analysis reveals that retention benefits to firms exceed PPL program costs under a broad range of PPL utilization and replacement rates.

Future research could extend this work in several directions. First, directly linking firm PPL entitlements to realized leave usage would allow decomposition of estimated retention effects into anticipatory and return-to-work components. This would also enable testing of whether the attenuation of women's retention gains at higher seniority levels reflects realized absence costs or differential take-up. Further work could also examine career trajectories and promotion outcomes to clarify whether PPL preserves firm-specific human capital or merely delays departure. If women who are retained by PPL subsequently experience slower advancement, the long-run retention value may be lower than the short-run estimates suggest. Additionally, the mechanism multiplicity framework developed here may apply to other non-wage benefits that combine non-transferability, contingent utilization, and potential career costs, such as sabbatical programs or tuition reimbursement with service commitments. Such work could test whether the patterns documented here generalize beyond PPL. Finally, studying how managerial practices and organizational norms shape the translation of formal benefits into leave-taking behavior would clarify the extent to which the retention effects of PPL depend not only on formal policy design but also on the informal workplace context in which policies are implemented.

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9 Appendix

9.1 Sample Construction and Age Imputation

The analysis is restricted to U.S.-based workers and positions. Observations without valid employment start dates are excluded. Employment spells without an observed end date are treated as ongoing employment. As a robustness check, alternative specifications restrict the sample to employment spells with observed end dates only.

Employee age is not directly observed in the Revelio data and is therefore imputed when required using documented educational attainment and graduation year information. Birth year is estimated by subtracting the assumed typical age at degree completion from the reported graduation year. The following assumptions are applied: high school diploma (18), associate degree (20), bachelor's degree (22), master's degree (28), MBA (28), and doctorate (30). To ensure plausible employment histories, age-based analyses are restricted to workers with imputed ages between 18 and 75. In addition, observations with graduation years prior to 1940 are excluded from age imputation.

When individuals report multiple degrees with valid completion dates, age imputation prioritizes degrees based on the likelihood of providing an accurate age indicator. This hierarchy reflects typical patterns of educational attainment, with earlier degrees completed at more uniform ages than later degrees. Specifically, the priority order is as follows: high school, bachelor's, associate, master's, MBA, and doctorate. Individuals without sufficient information to impute age are retained in the main sample but are excluded from analyses that require age-based measures.

9.2 Descriptive Statistics

9.2.1 Mean Departure by Sex and Treatment Status

Table 22: Descriptive Statistics for Monthly Departure Observations

Group	Mean	SD	N
Overall	0.01780760	0.1322516	196,407,437
Panel A: Pre vs. Post Firm Policy Change			
Pre (all)	0.01791944	0.1326587	99,330,746
Post (all)	0.01769317	0.1318337	97,076,691
Female, Pre	0.01815036	0.1334950	43,598,815
Female, Post	0.01773814	0.1319981	42,668,405
Male, Pre	0.01773879	0.1320005	55,731,931
Male, Post	0.01765790	0.1317046	54,408,286
Panel B: Effective Treatment			
Untreated (all)	0.01810537	0.1333325	116,523,011
Treated (all)	0.01737326	0.1306577	79,884,426
Female, Untreated	0.01835342	0.1342258	50,482,317
Female, Treated	0.01737238	0.1306544	35,784,903
Male, Untreated	0.01791576	0.1326453	66,040,694
Male, Treated	0.01737397	0.1306603	44,099,523

Notes: The unit of observation is an employee-month within the ± 36-month window around the focal firm's PPL policy change. Departure equals 1 if the worker exits the focal firm in a given month, 0 otherwise. Panel A splits observations by whether the focal firm's PPL policy change has taken effect ($Post = 1$ from the firm's policy effective date onward). Panel B splits observations by sex-specific effective treatment status, equal to 1 from the first month in which the firm's new PPL offering provides strictly more weeks than the maximum of the worker's state public PPL program and the firm's pre-policy PPL provision for the worker's sex group.

9.3 Fertility Rates Used to Impute Expected PPL Utilization at the Firm Level

Individual fertility is not directly observed in the employment records, so fertility risk is assigned probabilistically based on demographic characteristics.

Fertility rates are obtained from the U.S. Census Bureau's Public Use Microdata Sample (PUMS) for the years

2005–2023. I estimate fertility using a household composition model applied to the PUMS data, which allows fertility propensity to be constructed consistently for both women and men. Fertility rates (births per 1,000 individuals) are computed by sex, age group, education level, and calendar year, and then assigned to individual workers in the Revelio sample based on their observed sex, age group, education, and year of observation. Age groups are used rather than exact age because estimating fertility at the full age–education–sex–year level is not feasible given data sparsity in many demographic cells. For worker observations prior to 2005, I apply fertility rates from 2005, and for observations after 2023, I apply fertility rates from 2023, reflecting the bounds of available PUMS data.

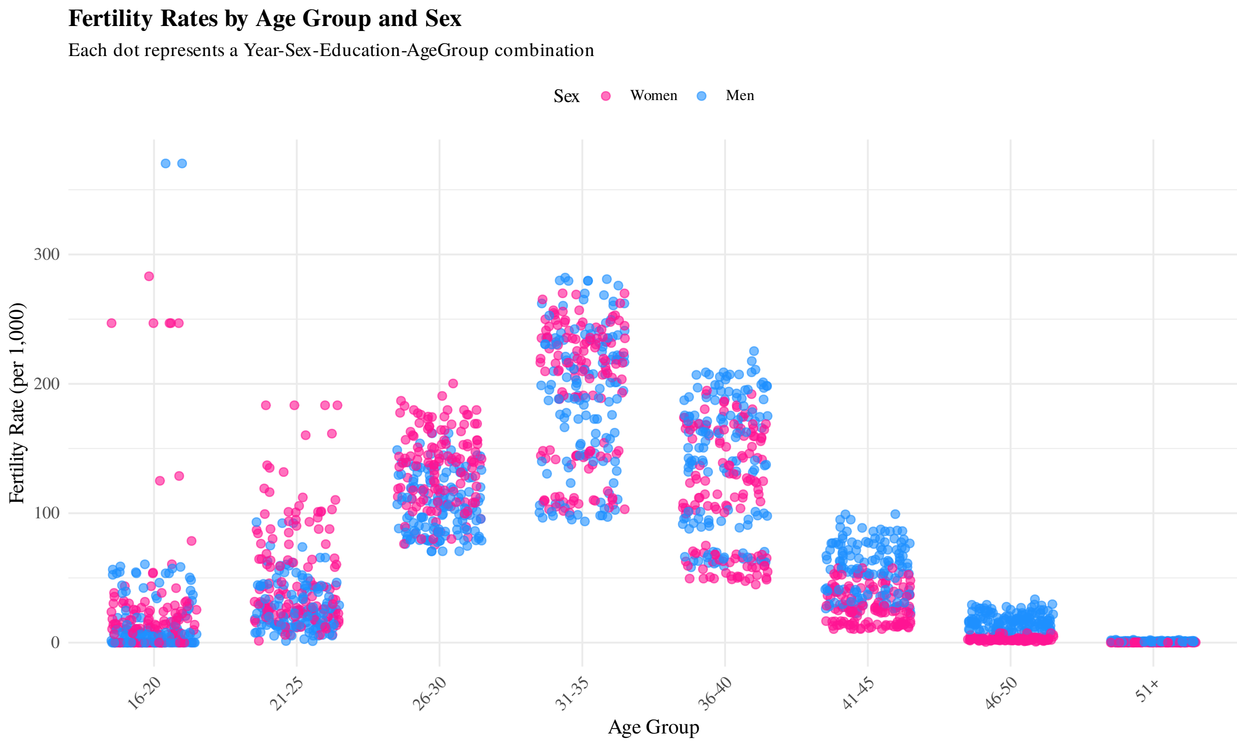

Figure 5: Fertility Rates by Age Group and Sex in the Sample. Figure shows fertility rates (births per 1,000 individuals) by age group and sex used to assign fertility propensity to workers in the sample. Each point represents a unique combination of year, sex, education level, and age group from the U.S. Census Bureau's Public Use Microdata Sample (PUMS) data for 2005–2023 that matches a worker in the Revelio sample used for the analysis. The PUMS fertility rates were assigned to workers in the sample based on worker sex, education level, age group, and year of observation.

Figure 5 illustrates the underlying fertility rates by age group and sex used in this assignment procedure. Each point represents a unique combination of year, sex, education level, and age group in the PUMS data that matches at least one worker-month observation in the Revelio sample.

9.4 Robustness Checks

9.4.1 Effective Compensated Weeks

Here I account for the economic magnitude of PPL benefits, as opposed to just the weeks of leave provided. In practice, the economic magnitude of firm PPL entitlements varies structurally across salary and state jurisdictions based on state program replacement rates, state program benefit caps, and firm top-ups.

The main specification treats a week of firm PPL and a week of state PPL as equivalent units of treatment, abstracting away from heterogeneity in the wage replacement value of PPL across workers and states. State PPL programs typically replace less than 100% of pre-leave earnings, and statutory caps (Table 2) bind for higher-earning workers.

As a result, a firm expansion that does not increase the total weeks of paid leave can nonetheless raise the value of paid leave by topping up state benefits to full wage replacement during weeks of overlap.

To address this, I construct an alternative treatment indicator that expresses leave entitlements in units of full-wage-equivalent weeks. The effective replacement rate from state PPL for worker i in state s at time t is:

$$\rho_{i,s,t} = \min\left(r_{s,t},\ \frac{B_{s,t}}{w_i}\right) \tag{17}$$

where w_i is the worker's weekly wage (measured at period -1 to anchor at the firm policy change moment), $r_{s,t}$ is the state's statutory replacement rate, and $B_{s,t}$ is the state's weekly benefit cap. For workers whose stated state benefit does not exceed the weekly cap, ρ_i equals the headline replacement rate; for higher-earning workers, the cap binds and $\rho_i < r_{s,t}$.

Assuming firm PPL provides 100% wage replacement during firm-covered weeks (consistent with the cost analysis in Section 7), and that the firm tops up state benefits to full wage during the overlap window, the effective compensated weeks (ECW) available to worker i at firm f in state s in month t is:

$$\text{ECW}_{i,g,f,s,t} = \text{FirmWeeks}_{g,f,t} + \max\bigl(0,\ \text{StateWeeks}_{g,s,t} - \text{FirmWeeks}_{g,f,t}\bigr) \cdot \rho_{i,s,t} \tag{18}$$

The first term captures firm-covered weeks at 100% replacement (whether paid directly by the firm or via topup of state benefits to full wage). The second term values any additional state-only weeks beyond firm coverage at the state's effective replacement rate. When the state has no public PPL program $\rho_i = 0$ and ECW collapses to FirmWeeks; when firm coverage matches or exceeds state coverage, the second term is zero and ECW likewise collapses to FirmWeeks.

The ECW-based treatment indicator replaces EffectiveTreatment from Equation 4 and turns on at the firm policy change date for worker-events whose post-policy ECW strictly exceeds their pre-policy ECW:

$$\text{EffectiveTreatmentECW}_{i,g,f,s,t} = \mathbf{1}\{t \geq T^{\text{start}}_{g,f,s}\} \cdot \mathbf{1}\{\text{ECW}^{\text{post}}_{i,g,f,s} > \text{ECW}^{\text{pre}}_{i,g,f,s}\} \tag{19}$$

Under this construction, two workers in the same firm-state-sex cell may receive different treatment status if the firm's expansion creates an effective increase in compensated weeks for one but not the other. For instance, a firm's new policy may exceed the prior maximum effective entitlement for a higher-earning worker whose state benefit was heavily capped, but leave a lower-earning worker in the same cell unchanged if the state benefit was already close to full wage replacement.

Table 23: Monthly Departure Regressed on Effective Compensated Weeks Treatment × Woman

	(1)	(2)
EffectiveTreatmentECW	-0.0551^{***}	-0.0473^{**}
	(0.0100)	(0.0146)
Woman × EffectiveTreatmentECW		-0.0163
		(0.0200)
Firm × State × Sex FEs	Yes	Yes
Year × Month × Sex FEs	Yes	Yes
Mean (departure)	1.7774	1.7774
SD (departure)	13.2131	13.2131
Num. obs.	193,650,244	193,650,244
Num. clusters	14,326	14,326

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. EffectiveTreatmentECW is defined in Equation 19: it equals 1 for worker-months in which the post-policy ECW strictly exceeds the pre-policy ECW, evaluated at the worker's weekly wage at period -1 and the state PPL parameters prevailing at the firm policy change. Sample restricted to event windows with no state PPL policy changes and to observations within ± 36 months of the firm policy change date. Standard errors clustered at the firm-state-sex level.

This construction requires worker-level wage data. Revelio salaries are imputed from public professional profile information rather than drawn from administrative records, so individual salary measurements contain measurement error that propagates into worker-level ρ_i and ECW values. While this noise averages out at the level of aggregate retention effects, ECW is more sensitive to wage measurement error than the duration-only EffectiveTreatment used in the main specification. For this reason, I report ECW as a robustness check rather than the main specification.

Table 23 reports DID estimates using EffectiveTreatmentECW in place of EffectiveTreatment, otherwise replicating Equation 4 and the sample restrictions described in Section 5. The pooled retention response is broadly consistent with the main specification: a 0.0551 percentage point reduction in monthly departure rates, or a 3.1% reduction relative to the pre-treatment mean of 1.78%. The differential effect for women is somewhat larger under ECW than in the main specification (-0.0163 vs. -0.0049 percentage points in Table 5), though it remains statistically insignificant. This pattern is consistent with the income-insurance channel operating more strongly for women: firm topups of partially-replaced state benefits create greater retention value for workers in states with limited public coverage, and women are the more intensive users of PPL.

To assess whether the ECW redefinition affects the salary-quartile heterogeneity reported in (Table 6), I estimate analogous quartile-stratified specifications using EffectiveTreatmentECW. Table 24 reports the results.

Table 24: Monthly Departure Regressed on ECW Treatment × Woman, by Salary Quartile

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
EffectiveTreatmentECW	-0.0761^{**}	-0.0312	-0.0458^{*}	-0.1015^{***}
	(0.0295)	(0.0213)	(0.0207)	(0.0201)
Woman × EffTreatmentECW	-0.1400^{**}	0.0369	0.0085	0.0608^{*}
	(0.0457)	(0.0304)	(0.0271)	(0.0271)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	2.539	1.649	1.731	1.189
SD (departure)	15.730	12.736	13.041	10.840
Num. obs.	48,403,645	48,401,149	48,402,471	48,401,991
Num. clusters	11,462	12,080	12,128	10,242

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. EffectiveTreatmentECW is defined in Equation 19. Salary quartiles are computed within the analysis sample (excluding the policy change month and observations outside ± 36 months of the firm policy change). Standard errors clustered at the firm-state-sex level.

The salary-quartile heterogeneity pattern is preserved under ECW. In the lowest salary quartile, women's incremental retention response remains large and statistically significant (-0.140 percentage points, $p < 0.01$), yielding a total women's response of -0.216 percentage points, or 8.5% relative to the Q1 pre-treatment mean. As in the main specification, this is the strongest retention response in the sample and is consistent with the income insurance, option value, return-to-work channels binding most tightly for lower-paid women. The retention response attenuates through the middle quartiles for both sexes, and in the highest salary quartile men show the largest retention response (-0.102 percentage points, or 8.5% relative to the Q4 mean) while women's net effect is attenuated to roughly 3%. The pattern of women's retention gains concentrating among low-paid workers and attenuating at higher pay is therefore robust to redefining treatment in terms of effective compensated weeks rather than duration alone.

9.4.2 Negative Weighting in Staggered Difference-in-Differences

The specification outlined in Equation (4) is a staggered difference-in-differences, in which firm PPL expansions take effect at different times across firm-state-sex units. Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2020) show that two-way fixed-effects (TWFE) estimators of staggered designs recover a weighted average of all underlying treated-versus-control comparisons, and that some of these comparisons use already-treated units as controls for later-treated units. When treatment effects are heterogeneous and dynamic, these "forbidden" comparisons can receive negative weights, biasing the pooled estimate. This can potentially attenuate the estimate toward zero or, in extreme cases, reverse its sign. This appendix assesses whether the headline retention estimate is an artifact of such weighting. I provide a direct decomposition of the implicit TWFE weights to address this.

I apply the de Chaisemartin and D'Haultfœuille (2020) decomposition, which expresses the TWFE coefficient as a weighted sum of the cell-level average treatment effects on the treated (ATTs) and reports the share of weight that is negative. Table 25 reports the decomposition separately by sex. For women, the coefficient is a weighted sum of 181,677 ATTs, of which 173,178 receive a positive weight (summing to 1.006) and 8,499 receive a negative weight (summing to -0.006); negative weights account for roughly 0.5% of the total weight mass. For men, 172,612 of 183,585 ATTs receive positive weight (summing to 1.029) and 10,973 receive negative weight (summing to -0.029), or roughly 2.8% of the total weight mass. In both cases the negative weights are an order of magnitude smaller than would be required to materially distort, let alone reverse, a coefficient of the estimated magnitude. The somewhat larger negative-weight share for men is consistent with men's PPL expansions being smaller and binding less often against state baselines, yielding a sparser treated panel; it nonetheless remains well below any threshold of concern. This decomposition indicates that the pooled TWFE estimate is, to a close approximation, a positively weighted average of ATTs rather than a Goodman–Bacon artifact.

Table 25: de Chaisemartin–D'Haultfœuille Weight Decomposition

	Women	Men
TWFE coefficient (β, pp)	-0.073	-0.080
Number of ATTs	181,677	183,585
ATTs, positive weight	173,178	172,612
Σ positive weights	1.006	1.029
ATTs, negative weight	8,499	10,973
Σ negative weights	-0.006	-0.029
Negative weight share	0.5%	2.8%

Notes. Decomposition follows de Chaisemartin and D'Haultfœuille (2020), computed on the full analysis sample. Treatment is *EffectiveTreatment*; groups are firm-state cells and periods are year-months. The coefficient is expressed in percentage points (a one-unit change in departure probability $\times 100$); weights are unitless and invariant to this rescaling.

9.4.3 Aggregation to Annual Analysis

Employment starts and departures in the sample exhibit legitimate seasonality and are also affected by Revelio's treatment of unknown employment dates (Revelio imputes January as the start or end month when the underlying user data do not report a month for an employment spell). This imputation skews the distribution of observations by calendar month, generating a pronounced spike in January start dates. Figure 6 shows the monthly distribution of employment starts and departures in the sample. To ensure that estimated retention effects of PPL expansions are not driven by firms changing policies in months with systematically higher or lower employment transitions, I conduct a robustness check that aggregates observations to the annual level.

I estimate a modified version of Equation 4 in event-time years by aggregating observations into 12-month periods relative to each firm's date of effective treatment for the relevant observed worker (e.g., Year 1 = months 1–12 following effective treatment, Year 2 = months 13–24 following effective treatment, and so on). In a staggered adoption design, event-time period indicators are collinear with the treatment indicator itself, as all firms are by construction untreated in pre-periods and treated in post-periods within the event-time framework. Therefore, the event-time specification includes sex-specific firm-state fixed effects ($\alpha^g_{f,s}$) but excludes event-time fixed effects. Formally, I estimate:

$$\begin{aligned}\text{Departure}_{i,f,s,y} = \beta_1 \text{woman}_i + \beta_2 \text{EffectiveTreatment}_{i,f,s,y} \\ + \beta_3(\text{woman}_i \times \text{EffectiveTreatment}_{i,f,s,y}) + \alpha^g_{f,s} + \varepsilon_{i,f,s,y}\end{aligned} \tag{20}$$

where $\text{Departure}_{i,f,s,y}$ equals 1 if individual i in firm f and state s departs at any point during event-time year y, and 0 otherwise. The treatment indicator is defined at the annual level as:

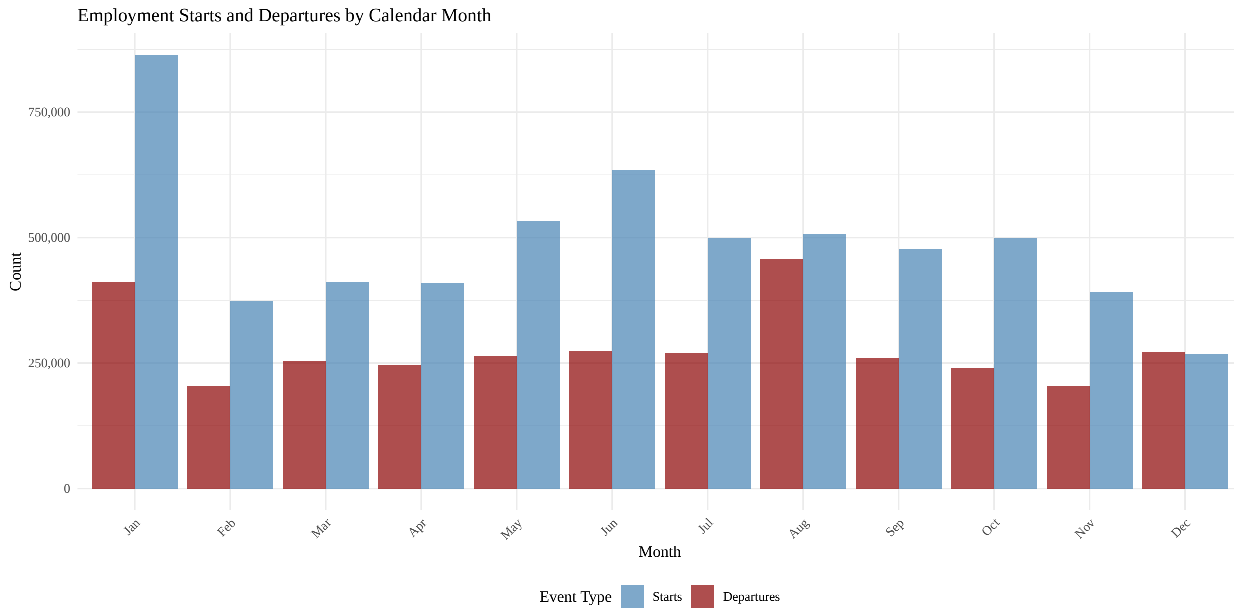

Figure 6: Seasonal Patterns in Employment Start and End Dates by Calendar Month. This figure displays the distribution of employment starts and departures in the sample across calendar months, aggregated over all years in the sample. N.B., the higher frequency of starts relative to departures reflects the right-censoring of the data as of June 2025, as well as the many employees who remain employed at the end of the ±36-month observation window.

$$\text{EffectiveTreatment}_{i,f,s,y} = \mathbf{1}\{y \geq Y_{g,f,s}^{\text{start}}\} \tag{21}$$

where $g \in \{\text{women}, \text{men}\}$ denotes the sex of worker i, and $Y_{g,f,s}^{\text{start}}$ is the first event-time year in which firm f in state s provides workers of sex g with effective treatment (as defined in Equations 3 and 4). The construction of effective treatment follows the same logic as the monthly specification: treatment begins in the first period where the firm's new PPL policy offers strictly more weeks than the maximum available from both state benefits and the firm's previous policy. Consistent with the main specification, standard errors are clustered at the firm-state-sex level.

The results of Equation 20 are shown in Table 26. The measured retention effects of effective treatment in the annual specification are larger in magnitude relative to those in the monthly specification. The annual specification coefficient on *EffectiveTreatment* ranges from -0.79 to -0.90 percentage points, representing a decrease in departures of 5.3% relative to the mean annual departure rate of 16.8%. By comparison, the coefficient on *EffectiveTreatment* from the monthly model represents a 2.6% reduction in departures relative to baseline (see Table 5). These differences may be due, at least in part, to the exclusion of time fixed effects in the annual model. The interaction term *Woman* $\times$ *EffectiveTreatment* has the expected negative sign but is statistically insignificant here, as is the case in the monthly specification. Taken together, these results confirm that the main findings are robust to annual aggregation.

Table 26: Annual Departures (in Event-Time Years) Regressed on Woman × Effective Treatment, with Firm-State Fixed Effects

	(1)	(2)
EffectiveTreatment	-0.009901^{***}	-0.008657^{***}
	(0.001163)	(0.001580)
Woman × EffectiveTreatment		-0.002785
		(0.002317)
Mean (departure)	0.170476	0.170476
SD (departure)	0.376051	0.376051
Num. obs.	16,193,718	16,193,718
Num. groups (firm–state–sex)	12,522	12,522
Fixed Effects	$\alpha^{g}_{f,s}$	
Clustering	Firm–state–sex	

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in levels. Monthly data are aggregated into annual event-time periods (Year 1 = months 1–12, Year 2 = months 13–24, etc.). Time fixed effects are excluded due to collinearity in staggered adoption design. Both specifications include sex-specific firm-state fixed effects.

9.4.4 Alternate Versions of Annual Analysis

Table 27 presents an alternative annual event-time specification to assess the sensitivity of the results to the fixed effects structure. Column (1) reproduces the baseline annual event-time model with sex-specific firm-state fixed effects, while Column (2) replaces these with non-sex-specific firm-state fixed effects. Both specifications omit event-time fixed effects, consistent with the event-time design in which treatment timing is mechanically tied to the post-period.

The estimated treatment effects are similar across the two specifications. The coefficient on *EffectiveTreatment* is negative and highly statistically significant in both columns, ranging from -0.87 to -0.88 percentage points. Relative to the mean annual departure rate of 17.05%, these estimates imply a reduction in departures of roughly 5.1–5.2%. The interaction term *Woman* × *EffectiveTreatment* is negative in both specifications, ranging from -0.26 to -0.28 percentage points, but is statistically insignificant in the specification with sex-specific firm-state fixed effects and statistically significant at the 5% level in the specification with non-sex-specific firm-state fixed effects. Overall, the annual retention results are not driven by the choice between sex-specific and non-sex-specific firm-state fixed effects.

Table 27: Annual Departures (Event-Time): Alternative Fixed Effects Specifications

	(1)	(2)
EffectiveTreatment	-0.8657^{***}	-0.8808^{***}
	(0.1580)	(0.1577)
Woman		-0.1897
		(0.1297)
Woman × EffectiveTreatment	-0.2785	-0.2630^{*}
	(0.2317)	(0.1203)
Mean (departure)	17.0476	17.0476
SD (departure)	37.6051	37.6051
Num. obs.	16,193,718	16,193,876
Num. groups (firm–state–sex)	12,522	–
Num. groups (firm–state)	–	7,009
Fixed Effects	$\alpha^{g}_{f,s}$	$\alpha_{f,s}$
Clustering	Firm–state–sex	

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. Column (1) includes sex-specific firm-state fixed effects. Column (2) includes firm-state fixed effects only. Both specifications omit event-time fixed effects and cluster standard errors at the firm-state-sex level.

9.5 Additional Subgroup Results

9.5.1 Revenue Growth

Tables 28 and 29 present heterogeneity in retention effects by firm revenue growth, measured in the three years before and after the PPL policy change, respectively, using revenue data from Capital IQ. Firms without revenue data are excluded. The results do not reveal a consistent or theoretically interpretable gradient: significant effects appear in different quartiles across the two windows, and the quartile composition shifts substantially between pre- and post-policy periods as firms' growth trajectories change. The *Woman* × *EffectiveTreatment* interactions are null throughout, consistent with the broader pattern. Consistent with the null heterogeneity in retention effects, there is no meaningful relationship between pre-policy revenue growth and either the level of PPL offered or the size of the PPL increase. These results are presented for completeness but do not support strong inference about the role of firm revenue growth in moderating PPL retention effects.

Table 28: Monthly Departure Regressed on Effective Treatment × Woman, Results by Pre-Policy Revenue Growth Quartile

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
EffectiveTreatment	-0.190^{***}	-0.061^{*}	-0.036	-0.157^{***}
	(0.033)	(0.029)	(0.027)	(0.045)
Woman × EffectiveTreatment	-0.013	-0.026	0.022	0.086
	(0.043)	(0.052)	(0.048)	(0.058)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	1.716	1.628	1.730	1.904
SD (departure)	12.986	12.655	13.039	13.665
Num. obs.	48,925,979	38,250,824	45,106,877	40,770,923
Num. clusters	3,567	1,349	3,735	3,303

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Revenue growth quartiles are based on the compound annual revenue growth rate in the three years prior to the PPL policy change, drawn from Capital IQ. Firms without revenue data are excluded. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

Table 29: Monthly Departure Regressed on Effective Treatment × Woman, Results by Post-Policy Revenue Growth Quartile

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
EffectiveTreatment	-0.090^{*}	-0.203^{***}	-0.007	-0.114^{**}
	(0.036)	(0.039)	(0.037)	(0.036)
Woman × EffectiveTreatment	-0.030	-0.002	-0.057	0.023
	(0.057)	(0.061)	(0.052)	(0.047)
Firm × State × Sex FEs	Yes	Yes	Yes	Yes
Year × Month × Sex FEs	Yes	Yes	Yes	Yes
Mean (departure)	1.520	1.721	1.765	2.011
SD (departure)	12.235	13.006	13.169	14.037
Num. obs.	44,066,866	35,752,357	41,790,182	36,179,210
Num. clusters	2,877	2,424	2,655	2,382

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Revenue growth quartiles are based on the compound annual revenue growth rate in the three years following the PPL policy change, drawn from Capital IQ. Firms without revenue data are excluded. Standard errors clustered at firm-state-sex level. Restricted to event windows with no state PPL policy changes.

9.5.2 Results by Magnitude of PPL Expansion

I examined how retention effects vary with the size of the PPL expansion by estimating Equation 4 separately for subgroups defined by the number of incremental PPL weeks provided under the new policy. Incremental weeks are defined as the net increase in effective PPL entitlement generated by the policy change, computed separately by sex as the difference between the post-policy maximum of firm and state weeks and the pre-policy maximum of firm and state weeks. This measure captures how many additional PPL weeks the policy change created for each sex, accounting for state coverage. I group firm-sex cells into four bins based on the size of the PPL increase: 1–4, 5–8, 9–11, and 12+ weeks.

Table 30: Monthly Departure Regressed on Effective Treatment × Woman, Results by Incremental PPL Weeks (Known Prior Policy Only)

	1–4	5–8	9–11	12+
EffectiveTreatment	-0.046	0.053	0.114	0.056
	(0.025)	(0.033)	(0.073)	(0.099)
Woman × EffectiveTreatment	-0.034	-0.094^{*}	-0.139	0.062
	(0.037)	(0.042)	(0.101)	(0.118)
Mean (departure)	1.783	1.778	1.729	2.141
SD (departure)	13.233	13.214	13.035	14.475
Num. obs.	61,301,145	36,563,962	13,251,564	6,673,278
Num. groups (firm–state–sex)	3,098	3,102	781	826
Num. groups (year–month–sex)	391	510	289	287
Fixed Effects	$\alpha^g_{f,s} + \lambda^g_t$			
Clustering	Firm–state–sex			

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Incremental weeks are defined as the net increase in effective PPL entitlement generated by the policy change, computed separately by sex as the difference between the post-policy maximum of firm and state weeks and the pre-policy maximum of firm and state weeks. Restricted to firms with known prior policy weeks. Specification includes firm-state-by-sex and month-year-by-sex fixed effects. Standard errors clustered at the firm-state-sex level. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table 31: Monthly Departure Regressed on Effective Treatment × Woman, Results by Incremental PPL Weeks (All Firms, Unknown Prior Weeks Assumed Zero)

	1–4	5–8	9–11	12+
EffectiveTreatment	-0.030	0.024	-0.029	-0.132
	(0.021)	(0.027)	(0.051)	(0.093)
Woman × EffectiveTreatment	-0.049	-0.069	0.019	0.085
	(0.034)	(0.036)	(0.076)	(0.097)
Mean (departure)	1.755	1.756	1.652	1.820
SD (departure)	13.132	13.134	12.748	13.367
Num. obs.	71,961,799	47,479,723	16,022,275	24,285,203
Num. groups (firm–state–sex)	3,961	4,510	1,148	4,002
Num. groups (year–month–sex)	391	510	311	372
Fixed Effects	$\alpha^g_{f,s} + \lambda^g_t$			
Clustering	Firm–state–sex			

Note: Coefficients and mean departure rate are expressed in percentage points. *EffectiveTreatment* = 1 when firm provides PPL weeks exceeding both state coverage and the firm's previous policy, calculated separately by sex. Incremental weeks are defined as the net increase in effective PPL entitlement generated by the policy change, computed separately by sex as the difference between the post-policy maximum of firm and state weeks and the pre-policy maximum of firm and state weeks. Firms with unknown prior policy weeks are assumed to have had zero prior weeks. Specification includes firm-state-by-sex and year-month-sex fixed effects. Standard errors clustered at the firm-state-sex level. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Tables 30 and 31 report estimates from Equation 4 run separately for subgroups defined by the number of incremental PPL weeks provided under the new policy. Table 30 restricts to firms with known prior policy weeks, while Table 31 includes all firms, assuming zero prior weeks where the previous policy is unknown. Neither specification produces statistically significant or consistent results across bins, and the two options yield meaningfully different point estimates, indicating sensitivity to assumptions about unknown prior weeks. The incremental weeks analysis does not add appreciably to the new weeks results reported in Table 16, suggesting that the level of the new policy matters more for retention than the size of the increase relative to prior coverage.

9.6 Distribution of Employee Eligibility for PPL

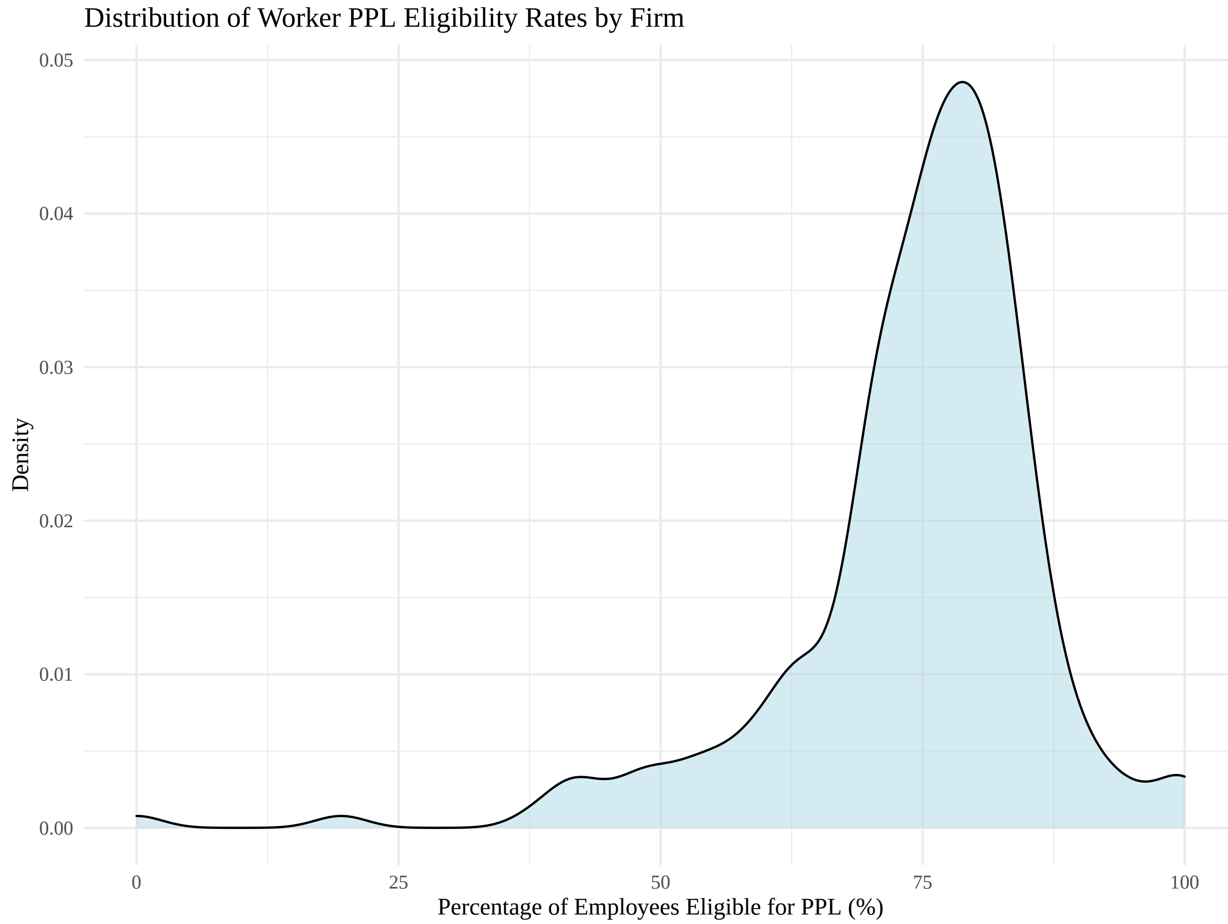

Figure 7: Distribution of PPL Eligibility Rates by Firm at Policy Implementation