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Parenthood on Job Performance, Human
Capital, and Career Advancement**

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ABSTRACT

Baby Bumps in the Road: The Impact of Parenthood on Job Performance, Human Capital, and Career Advancement*

This paper explores whether and why a maternal “child penalty” to earnings would emerge even without changes in employment and hours worked. Using a matched event study design, we trace monthly changes in determinants of wages (job performance, human capital accumulation, and promotions). Data come from a usefully unusual setting with required multiyear employment and detailed personnel data: the United States Marine Corps. Mothers’ job performance initially declines, and gaps in promotion grow through 24 months postbirth. Fathers’ physical fitness performance drops somewhat but recovers. These patterns lead mothers to earn relatively lower wages, even absent changes in employment postbirth.

JEL Classification: J24, J16, J18, J45

Keywords: parenthood, child penalty, gender wage gap, promotion

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After having a first child, mothers' earnings drop precipitously and remain 20–60% lower than fathers' for the decade that follows (Kleven et al., 2019a,b). This pattern, termed the child penalty, is remarkably consistent across countries and contexts (e.g., Aguilar-Gomez et al., 2019; Andresen and Nix, 2022; Angelov et al., 2016; Barth et al., 2017; Bertrand et al., 2010; Fernández-Kranz et al., 2013; Kleven, 2022 and Kleven et al., 2019a). It also drives most of the overall male-female earnings gap in higher-income countries (Cortes and Pan, 2020; Kleven et al., 2020, 2019b). Prior research shows the child penalty comes from three sources: exits from the labor force, reductions in hours worked (conditional on employment), and lower wages among women relative to men after having a child (Fernández-Kranz et al., 2013; Kleven et al., 2019b).¹ This paper focuses on the wage channel behind the child penalty. We explore whether and how determinants of mothers' and fathers' wages respond differently to childbirth. We leverage a usefully unusual setting: the U.S. Marine Corps. Detailed administrative data allow us to trace immediate, month-by-month consequences of parenthood on workers' job performance, human capital accumulation, and career advancement—all of which determine wages in the Marines and in the broader labor market. Workers in the Marines sign multiyear job contracts, meaning we isolate impacts largely absent confounding factors like labor market exits, reductions in hours worked, and cross-firm job changes (Kleven et al., 2019b; Laffers and Schmidpeter, 2022). In turn, our empirical analysis addresses the following question: If men and women were to stay on the job at their prebirth employer working similar hours after having a child, would a child penalty in earnings emerge, and why?

Our findings come from workers in the world's largest employer, the U.S. Department of Defense (DoD), whose policies and actions have global reach. Gender-based disparities in work outcomes in this setting are of particular concern because they may limit who advances and contributes to decision-making at higher ranks. Beyond implications for the U.S. military, our data provide a rare opportunity to examine job-relevant outcomes in higher-physicality jobs, which comprise a

¹A longstanding literature in economics documents the negative impact of fertility on maternal employment, hours worked, and wages (Agüero and Marks, 2011; Angrist and Evans, 1998; Bronars and Grogger, 1994; Cools et al., 2017; Cruces and Galiani, 2007; Jacobsen et al., 1999). The recent child penalty literature shows that adjustments on these margins among women (and not men) create a mother-father earnings gap that persists over the life course. *Why* only women adjust in these ways after having a baby remains an open question.

large share of the U.S. workforce and are less studied in the current literature. To contextualize the specialized degree of health and fitness required among those in our sample, Figure 1 presents data on the physical strength and stamina required for work among (1) male and female Marines, (2) those in the top 10 most common female occupations, and (3) those studied in prior literature on the job productivity and promotion consequences of parenthood. Values shown for our sample of Marine men and women reflect the median O*NET level of dynamic strength and stamina of Marine jobs mapped to their civilian equivalents.² The most common job for civilian women (registered nurse) requires more strength and stamina than the median Marine woman’s job, and many other common jobs for women (e.g., elementary and middle school teacher, retail sales supervisor, and cashier) also require some level of physicality. Prior research on postbirth productivity has focused on the impacts of parenthood among individuals in less-physical jobs, including economics professors (Antecol et al., 2018), lawyers (Azmat and Ferrer, 2017), professionals with a master’s degree in business administration (Bertrand et al., 2010), and scientists (Kim and Moser, 2021).

Our outcomes include job performance, human capital accumulation, and career advancement. We measure job performance using scores from job-related physical fitness tests, supervisor ratings, and evaluations of marksmanship skills. Measures of human capital include the number of months workers received job-specific training and their accumulation of years of formal education. Last, career advancement tracks the number of promotions Marines received over time. Though we study these outcomes in a Marine context, the same factors—job performance, human capital, and promotion—also determine civilian wages in most industries.

Our empirical strategy uses the precise month of childbirth as an exogenous shock to parents’ work performance outcomes. We assign “placebo births” to nonparent Marines, those who do not have a child during the study window. Changes in nonparents’ outcomes help us account for secular time trends that would have occurred absent parenthood. To assign placebo births, we first match nonparents to parents exactly on tenure (in months) in the Marine Corps, job rank, active

²O*NET dynamic strength levels capture a worker’s ability to exert muscle force repeatedly or continuously over time (National Center for O*NET Development, 2022; Department of Labor, 2022). Dynamic strength levels required for an occupation range from “not relevant” for jobs like Postsecondary Economics Teachers to 66 for dancers. Stamina measures the ability of a worker to extend themselves without getting out of breath over long periods.

duty (full-time) or reservist (part-time) status, and calendar year of the observation. We further restrict matches based on observable characteristics that predict a first birth. Our analyses compare outcomes between parents and nonparents before and after birth (or placebo birth). Compared to a traditional two-way fixed effects model, the placebo birth strategy better corrects for observable differences between parents and nonparents that might affect parents' outcomes in the absence of having a child. The strategy allows us to study cumulative changes in outcomes as well (e.g., total promotions since pregnancy), given that we can measure changes in outcomes relative to the event/placebo event for both parents and nonparents. The matching strategy also allows us to explore subgroup differences based on prepregnancy characteristics, given that we can define prepregnancy for the nonparents. We estimate all models separately for women and men to allow for the possibility that parenthood impacts both women and men along the margins we study. If men are affected, fathers would not be a good comparison group for mothers.

We find persistent effects of parenthood on work outcomes for mothers. Mothers' physical fitness, job performance ratings, and marksmanship scores decline immediately postbirth, as soon as they are observed after any testing exemptions around pregnancy/birth are lifted. Mothers' job performance and marksmanship scores recover by the child's second birthday, but their physical fitness remains 0.2 standard deviations lower than if they had not had a child. Mothers also accumulate fewer months of training relative to nonmothers, with gaps beginning during pregnancy and continuing through two years postbirth. Consistent with these patterns, mothers' promotion trajectories slow during pregnancy, with gaps widening rather than narrowing, during the two years postbirth. These patterns broadly hold across subgroups.

We observe minimal impacts of parenthood on fathers' outcomes. The birth of a child leads to small, short-lived declines in physical performance, job performance, and training one month postbirth. We do not find effects on fathers' years of formal education or accumulated promotions.

In supplementary analyses, we show men who have a recorded medical event (e.g., an injury) that limits their ability to fulfill work duties experience changes in their physical fitness, supervisor ratings of job performance, propensity to engage in training, and promotions similar to those of

mothers. These findings suggest discrimination against mothers specifically does not drive the entirety of the decline in job performance and career advancement among women after having a child. However, discrimination against individuals who encounter medical limitations, including but not limited to pregnancy, is still possible and would disproportionately affect women.

Together, our results show that the immediate period after having a child is uniquely challenging for women who remain on the job as U.S. Marines. Our findings suggest that the strain of parenthood—whether social, physical, or both—accrues more acutely to mothers and limits their ability to perform on the job and advance professionally after having a child.

Last, we explore whether changes to the length of paid maternity leave during our study window predict changes in the impact of parenthood on mothers' outcomes. We compare mothers who were eligible for 6 weeks of leave, 6 weeks plus 12 flexible weeks of leave after returning to work, or 18 weeks of leave based on the timing of their birth. We do not find statistically significant differences in the magnitude of parenthood impacts across the three policies.

Our paper's main contribution is to the literature on child penalties to earnings that women, but not men, face after having a baby. We provide direct evidence on the factors behind the *within-employer* wage effects of parenthood for women. A number of studies show that changes to women's wages due to parenthood stem from two factors: (1) mothers sort into firms that are less productive with lower pay after their first birth, and (2) they experience slower within-employer wage growth, even if they remain at their prebirth employer (Bruns, 2019; Casarico and Lattanzio, 2023; Hotz et al., 2018). We add to this literature a deeper understanding of *why* women who remain at the same employer after having a child might see slower career progression and wage growth. We observe unusually rich measures of on-the-job determinants of wage and, thanks to the unique DoD setting, we focus on a group of workers who are limited in their ability to select out of work with their prebirth employer. In this way, our paper speaks to whether and why child penalties might still emerge even if women maintained all dimensions of their prebirth employment.

Our paper also speaks to a number of studies on the role of biology as a driver of child penalties. While we cannot directly isolate whether the biological impact of pregnancy and childbirth under-

lies the negative impacts we document among mothers, we find suggestive evidence that factors *besides* biological impacts play a role, given that fathers in our setting experience small, short-lived declines in physical fitness performance due to parenthood as well. If only the biological effects of pregnancy and childbirth were at play, we would expect men to be entirely unaffected. Instead, our results that men's job-related physical fitness is somewhat affected suggests scope for caregiving responsibilities (e.g., sleepless nights caring for a newborn) or broader social influences as possible mechanisms behind declines in work performance and career advancement associated with parenthood. That these factors accrue more to women aligns with prior research on same-sex and adoptive parents that finds even non-birthing mothers experience child penalties to earnings (Andresen and Nix, 2022; Kleven et al., 2021).

Finally, unlike much of the existing literature on child penalties, our paper traces parents' work outcomes month-by-month rather than year-by-year. We show that parenthood begins to impact women's job performance and career advancement *within* the first year after childbirth. Immediate postbirth declines in mothers' ability to perform at work and advance their job skills may be one of the reasons mothers more broadly (those who are not constrained by contracts like in our setting) opt to exit the labor market, reduce the number of hours they work, or move to different firms (perhaps with lower intensity, less well-paid work) by the time these outcomes are first measured one or more years postbirth in other papers (Andresen and Nix, 2022; Angelov et al., 2016; Barth et al., 2017; Kleven et al., 2019a,b). Our focus on month-by-month impacts also allows us to detect that fathers initially face some physical job performance consequences of parenthood, despite recovering within two years following the birth.

Several studies related to ours document adverse productivity shocks associated with parenthood. Similar to our work, Kim and Moser (2021) find female scientists in the 1950s patented less during their childbearing years, relative to fathers and other women, while Azmat and Ferrer (2017) show female lawyers with young children bill fewer annual hours than male lawyers with young children. Gallen (2018) explores how firm-level output in Denmark varies by the gender and parental status of employees in private firms. She finds that mothers are less productive than

other workers using a firm-level production function model, particularly during their childbearing years. Our paper focuses on precursors to work output and productivity—job performance and skill accumulation—at the individual level. We build on Healy and Heissel (2022) to show that parenthood impacts a range of mothers’ job performance and human capital outcomes, and that these impacts give rise to gender disparities in career advancement among parents.

1 Institutional Background

The DoD employs 2.3 million active-duty and reserve service members. We focus on the U.S. Marine Corps, a service branch within the DoD, where administrative records on job performance and career advancement are readily available. Marines are an immediate response force, ready to deploy quickly to support combat missions on sea or land. Marines make up 14% of active-duty forces, and the majority (92%) are men (Department of Defense, 2018). With women less likely to serve, negative impacts of parenthood on women’s work outcomes may be especially noteworthy.

Marines begin work either as a junior enlisted service member, akin to an entry-level civilian worker; or as an officer, akin to a civilian manager.³ Enlisted ranks span E1 to E9, and officer ranks span O1 to O10, with ranks equivalent to pay grades. Most Marines (88%) are enlisted (Department of Defense, 2018), meaning they make up the majority of our analytic sample.

Based on job tenure and job performance, Marines can advance to better paid, higher-responsibility ranks. Promotion trajectories vary between enlisted and officers, with junior enlisted personnel eligible for promotions more often than senior enlisted and officers. Criteria for promotion are very specific. Junior enlisted Marines (E1 through E4) receive points based on standardized job performance assessments, increases in their education or skills, and increases in time in service.⁴ Marines accumulate time in service even if they are on leave. Once a junior enlisted Marine receives a sufficient number of points, they are automatically promoted to the next rank.⁵ For officers and more

³Enlisted service members must have a high school degree and be between 18 and 29 years old when they begin. Officers must have at least a bachelor’s degree. Most Marines are of prime childbearing age, with 81% under 30 years old. More than a quarter are parents (Department of Defense, 2018).

⁴Marines can also receive bonus points for recruiting new service members.

⁵Promotions at the lowest enlisted ranks are relatively automatic after a given number of months in service and

senior enlisted Marines, the same performance metrics are reviewed by an evaluation board to determine promotion. The formalized promotion structure for both junior and more senior Marines means individuals can determine exactly what they need to advance, and they have an especially strong incentive to perform well on measured assessments.

Performance evaluations in the Marines assess both the mental and physical acuity of workers. Marines take twice yearly physical fitness tests (e.g., timed runs, sit-ups, and push-ups), and supervisors rate Marines' job proficiency at least annually, akin to a performance review. Marines also complete marksmanship evaluations where their rifle or pistol shooting accuracy is scored, though these tests become optional as they advance to higher ranks. All individuals are evaluated using these same performance metrics regardless of their career field (e.g., legal services vs. infantry). At some job ranks, specific scores on these measures garner promotion points, and the points required for promotion vary by job field. However, regardless of rank and field, the Marines maintain a culture of high effort and strong performance. Of the three standardized job performance assessments—physical fitness tests, supervisor ratings, and marksmanship evaluations—supervisor ratings factor most heavily into the promotion points calculation as well as into the decisions made by promotion review boards.

There are over 35 career fields in the Marine Corps, and while many are specific to the military (e.g., infantry), many are also present in the civilian labor market (e.g., food services, financial management, police and corrections, and more). Marines can also serve as active duty personnel or reservists. Active duty marines work full time, while reservists work part time. Reservists typically have civilian careers or enroll in higher education while they fulfill their part-time Marine Corps duties.⁶ Our analytic sample predominantly consists of active-duty Marines who work full time. Most active duty service members (83%) live and work on or near a military base and are stationed in the U.S. (Department of Defense 2018), though some are stationed abroad. We include reservists

time spent in rank, while promotions to E4 and E5 are more competitive. (Larger, 2017). Fifty-six percent of active duty Marines are E3–E5 (Department of Defense, 2018).

⁶Service in the reserves requires participating in training drills one weekend per month and attending a two-week program each year. Reservists can be called for active duty, at which point they are active reserves and work as full-time Marines. We group active reservists with active duty service members for the purposes of this study.

in our sample because they may better reflect how civilians would respond to parenthood; we can also explore heterogeneity in our results among these two groups.

Marines sign a legally binding contract that outlines their required length of service. Initial enlisted contracts typically require four years of active-duty service, while officer commitments are typically three years.⁷ These contracts limit Marines' ability to exit the labor force after they have a baby depending on the length of the contract that remains.

The DoD provides several family-friendly benefits, including subsidized childcare and fully paid parental leave. In a supplementary analysis, we focus on variation in the effects of parenthood across policy changes to the length of paid leave for primary caregivers (most often women).⁸

2 Data

We use data from the Marine's Total Force Data Warehouse for all active-duty and reserve Marines who served at any point from January 2010 to December 2019. Our data include descriptive information (age, gender, race/ethnicity, education, and AFQT/GCT scores—which measure aptitude and intelligence), job characteristics (job type, rank, time in service, and unit location), and dependent characteristics for spouses and children (date of birth and whether a spouse is in the military).

Our first set of outcomes consists of the three primary measures of job performance used for promotion and retention decisions. First, we use scores from fitness tests to capture Marines' ability to perform physically demanding work tasks. Marines take two standardized tests per year: the Physical Fitness Test (PFT; timed running, crunches, and pull-ups/push-ups) in the first half of the year (typically May–June) and the Combat Fitness Test (CFT; timed running, a combat-related obstacle course, and an upper-body strength test measured by ammunition can lifts) in the second half of the year (typically October–December). Marines generally receive a few weeks' notice before they must take a fitness test, but some take it later if they face competing priorities.⁹

⁷Active duty contracts can stipulate additional service and require additional years of service in the reserves. At the end of each contract, Marines can decide whether to re-enlist, which involves another contract with a time commitment. About 75% of Marines only complete one contract (U.S. Marine Corps, 2021).

⁸Paternity leave changed from 10 to 14 days during this time. We do not examine paternity leave in this paper.

⁹Appendix Figure A.1 displays the distribution of physical fitness test timing by month and parenthood status.

Scores are awarded on a 300-point scale, which is adjusted for age and gender such that women do not need to do as many pull-ups as men, and older service members do not need to run as fast as younger ones to achieve the same score. We standardize the points-based physical fitness scores by year, gender, and test type. We combine the Z -scores for the two tests into one measure, generally observed twice per year per Marine. During our study window, women were not required to take the test when pregnant nor for 6 months postbirth. We resume measurement of physical performance for women at 8 months postbirth due to concerns that commanding officers may allow some women who are 7 months postbirth in December to skip the CFT that cycle.

The second job performance outcome we measure consists of supervisor evaluations of Marines' work performance. Supervisors regularly assess Marines using one of two rating scales, depending on the Marine's rank. During our study window, junior enlisted received proficiency and conduct marks ("ProCons"), and senior enlisted and officers receive Fitness Reports ("FitReps"). Both assessments require supervisors to assess a Marine's performance across a range of professional domains, such as technical knowledge, effectiveness, and communication skills. Marines receive these evaluations even if they are on leave; performance on a given assessment is then based on the time from their last evaluation until they went on leave. We standardize supervisor ratings by year, gender, and assessment type. We combine the Z -scores into one outcome we call job performance. We generally observe ratings at least twice per year among junior enlisted, once in the first half and once in the second half. For senior enlisted and officers, we observe supervisor ratings at least once per year. If a Marine is transferred, discharged, or promoted, or if their supervisor changes, they receive additional performance ratings.¹⁰ We are missing supervisor ratings for junior enlisted who left the Marines before October 2017. We observe the full history of performance ratings (including prior to October 2017) for any Marine who was active as of October 2017. The subjective

There is more variation across months than across groups. Mothers are slightly more likely to take a test in June than other groups, either because they have waivers before that or because they otherwise delay the test. They are required to take the PFT by June and CFT by December; if all mothers took the PFT in June we would still have a distribution of scores across months relative to birth, which is what we focus on. An F -test of the mean month of the fitness test does not indicate mothers statistically take the test later than other groups ($F(3, 26, 557) = 1.23$, p -value = 0.296, with standard errors adjusted for clusters from the main analysis).

¹⁰Marines, including supervisors, are relocated every few years. Decisions on relocation are made from a central location, which prevents Marines from manipulating their scores by selecting their supervisors (Cunha et al., 2018).

nature of supervisor ratings means we cannot distinguish true changes in job performance from supervisors' *perceptions* of changes in performance using this measure.

Our third job performance outcome captures rifle and pistol marksmanship assessment scores. Marksmanship assessments evaluate Marines' target shooting skills. While this measure is clearly a firm-specific indicator of job proficiency, strong performance is partially cognitive and requires practice, focus, and concentration. We standardize marksmanship scores by year, gender, and weapon (rifle/pistol). The marksmanship assessment is required once a year at junior levels and becomes optional for more senior Marines. Those who perform at the highest level (rated "expert") are exempt from re-testing the following year, making outcome data on this measure sparse. Like physical performance tests, the marksmanship test requirement is waived for pregnant mothers and not assessed when mothers are on parental leave. We resume measurement of marksmanship scores for mothers starting 5 months postbirth, given that some mothers in our study window could have been on leave for 18 weeks. Appendix Figure A.2 provides visual evidence of testing exemptions around pregnancy/birth for physical performance and marksmanship evaluations and indicates which months we exclude from our analyses due to possible exemptions.

Our second set of outcomes covers human capital development. We measure firm-specific human capital based on participation in job-related training assignments. We count the number of months of training each Marine receives since the time point 10 months before they have a child. A value of 1 on the variable indicates 1 additional month of training since just before pregnancy. We capture training when an individual's "duty status" is updated to reflect enrollment in a training program. We do not observe any training that an entire unit receives together and, as a result, we likely undercount total training. Most commonly, a Marine's supervisor decides whether, where, and when an individual will go to training based on the needs of the Marine Corps. Enrollment in training is a firm-side investment in the individual and is not remedial. Training could be located out of state, and dependents would stay behind at the original base location while the Marine attends the training. The mean training spell lasts 5 months (IQR = 2–6 months).

To measure general human capital, we track increases in Marines' formal education levels,

which increase as they gain credits from institutions of higher education. This is reported as years of education (e.g., an associate degree is 14 years of education). Increases in formal education are considered in the promotion process, so Marines are incentivized to keep this information updated. While the Marine Corps may send officers to obtain master's degrees to fit its needs, in general, educational attainment among enlisted personnel is initiated by the individual on their own time.

Finally, we track promotions over time. We observe each Marine's job rank on a monthly basis and can trace successive increases in rank over time. We count the number of increases in job rank (i.e., promotions) a Marine achieves relative to 10 months before they have a child. A value of 1 on the variable indicates 1 promotion received since just before pregnancy.

Our sample uses a semi-balanced panel of observations to ensure our results are not driven by selective attrition. We require first-time parents and their potential matches to be observed continuously for 12 months prior to birth and 24 months after ($n=2,801$ mothers). We also require individuals to have a military entrance exam score (AFQT or GCT), at least one observed prebirth fitness score and for parents to have at least one potential nonparent match with the same months of service, rank, and reserve status in the same prepregnancy calendar year ($n=2,492$ mothers).

We rely on the Standard Occupational Classification (SOC) system from O*NET, a federal standard used to classify workers into occupational categories, to explore the distribution of job types among Marines relative to civilians. We crosswalk Marine job codes to SOC codes and find that outside of military-specific occupations the largest share of first-time Marine fathers work in natural resources, construction, or maintenance, while first-time Marine mothers work in sales or office roles. A small share of first-time Marine parents in our sample are officers (akin to civilian managers): 7% and 14% of Marine mothers and fathers, respectively. Results from our analyses may generalize best to younger workers with low levels of formal education.¹¹

¹¹Appendix Table A.1 describes Marine parents and employed civilian parents with a first child under age 1.

3 Empirical Approach

We use the timing of first births to identify the effect of the transition to parenthood.¹² If the transition to parenthood affects work outcomes, then birth should generate a sharp change in these outcomes at predictable time points. We can attribute any discontinuity in the outcomes at those time points to the pregnancy or birth itself if we assume that other factors that shape job outcomes do not also undergo a sharp change at those same times.

We employ a version of a two-way fixed effects (TWFE) event study strategy using the shock and precise timing of birth events. Our goal is to minimize two sources of bias. The first source of bias, which we call TWFE bias, arises when treatment timing varies (as it does in our setting) and already-treated units serve as comparisons for later-treated units, contaminating estimates of counterfactual time trends.¹³ To address TWFE bias, Callaway and Sant’Anna (2021) and Sun and Abraham (2021) propose isolating cohorts of units treated at the same time and selecting comparison cases for each treatment time group that *exclude* already-treated units. This functionally aligns event-time and calendar time across treatment and control cases within each treatment time group.

The second source of bias, which we call counterfactual bias, arises if the chosen comparison units do not approximate counterfactual trends in outcomes. In our setting, not all Marines without a birth (or yet to have a birth) would provide a good estimate of counterfactual time trends for those who do have a first birth, especially for promotion outcomes. Promotion eligibility and timing depend on a Marine’s current job level and time in service. A second-year enlisted Marine becomes eligible for promotion sooner and can promote faster than a fifth-year officer. Active duty vs.

¹²Gallen et al. (2023) show that child penalty estimates from event studies are very similar to those obtained using a plausibly exogenous shock to pregnancy. Their results provide evidence of the exogeneity of the childbirth event.

¹³A traditional TWFE approach incorporates time and unit fixed effects to estimate posttreatment impacts (whether dynamic or constant). Already-treated, not-yet-treated, and never-treated units (if included) contribute to estimates of counterfactual time trends, or the time fixed effects. The estimate of the causal parameter under a traditional TWFE model is then a weighted average of all of two-group/two-period difference-in-difference estimators in the data. As treatment timing gets later, more of the two-group/two-period difference-in-difference estimators compare the change in a unit moving from untreated to treated against units who are treated in both the before and after period. This statistical fact requires stronger assumptions than previously recognized, with both a parallel trends assumption *and* an assumption of consistent treatment effects over time and units needed for identification (Goodman-Bacon, 2021). Baker et al. (2021) show that not accounting for the bias in settings that do not meet these assumptions can affect the point estimates of the policy or treatment in question.

reserve contexts also give rise to different promotion probabilities. As a result, we face large counterfactual bias if we do not thoughtfully select comparison units for estimates of promotion effects. The same concern around counterfactual bias holds true for our estimates of other work outcomes, given that professional expectations for job performance and human capital development depend (less formally) on job rank, tenure, and active duty/reserve contexts.

Ideally, our empirical strategy would isolate groups of parents with first births in the same month-year (to address TWFE bias) and then draw nonparents with the same job tenure, job rank, and reserve status (to address counterfactual bias). However, under this approach, some groups of parents have few to no nonparent comparison units. Nonparent comparison units also vary from parents on other dimensions (e.g., average physical performance, education, age) not defined in the exact-match requirements, which generates new concerns around counterfactual bias.

Given this tension, we require that parents and nonparents match on tenure, rank, and reserve status, but we do not require exact treatment time matching. Instead, we connect parents to nonparents within the same calendar year. Parents' and nonparents' birth/placebo birth events do not occur in precisely the same month-year, but they all occur within 12 months of one another. We hypothesize, in our context, that this method provides the best counterfactual to the treated group. We are willing to risk some TWFE bias to reduce counterfactual bias.¹⁴

3.1 Assigning Nonparents to Placebo Births

We use adaptive ridge LASSO (least absolute shrinkage and selection operator) with 10-fold validation to select the best predictors of a first birth in our sample. Possible predictors include months

¹⁴Appendix B explores alternative specifications in our study context: a standard TWFE model, a stacked TWFE model that exactly matches month-year treatment timing, and an event study with nonparents assigned to placebo births where we do not control for month-year fixed effects. The broad take-aways are consistent across models, but the size of the point estimates differ, highlighting the importance of carefully choosing a comparison group. For example, the negative impact of parenthood on promotion is 55% larger for mothers in the stacked TWFE model than the preferred model; for fathers, the sign flips direction and gains statistical significance. Promotion is the outcome we are most concerned about in terms of the selection of comparison cases, and the sensitivity of the results to the standard TWFE stacked specification underscores our concern. Our preferred specification prioritizes defining an appropriate counterfactual while still considering how to minimize TWFE bias. This strategy may be useful in settings where other variables (e.g., months of service) strongly shape potential outcomes.

of service, an indicator for officer (relative to non-officers), an indicator for reservists (relative to active duty), year, age, race/ethnicity, AFQT scores¹⁵, marital status, an indicator for whether a spouse is also in the military, years of education, occupational groups, most recent physical performance score, and interactions among all variables. We restrict the sample to parents and potential matches we would be able to observe at least 12 months before and 24 months after the birth/placebo birth. We run this predictive model separately among women and men, measuring all characteristics of first-time parents 10 months before an observed birth (prepregnancy). We then obtain a predicted propensity score for each parent and nonparent.

Among groups of parents and nonparents with exact matches on number of months of service, job rank, reserve status, and observation calendar year, we select up to five nonparents for every parent that are closest in terms of their predicted propensity to have a baby 10 months later.¹⁶ We assign nonparents to a placebo birth event 10 months after the time of the match. Analyses then compare the changes in outcomes for first-time parents to the average change in outcomes for up to 5 most observably similar nonparents to whom they match. Each parent receives a weight of 1 in the analysis, while each nonparent receives a weight of 0.2 per match-month in the case where 5 distinct nonparents match to each parent.

Table 1 displays descriptive characteristics of parents and matched nonparents separately by gender. We require matches to be exact on months of service, job rank, reserve status, and calendar year, so the groups exactly match on this first set of variables. The next set of variables is included in the LASSO model predicting likelihood of first birth. First-time parents and nonparents with placebo births look almost identical, with a few small differences within gender. These differences are functionally small (e.g., mothers are 22.57 years old while their placebos are 22.70 years old).

¹⁵We have missing AFQT score data for many officers, who generally take GCT exams. We use a cubic model of standardized GCT scores to predict standardized AFQTs for those with both scores, then use this model to predict AFQT scores for those with only GCT scores.

¹⁶We conduct nearest-neighbor matching with replacement, meaning the same nonparent can be matched to different first-time parents, or parents can match to fewer than 5 nonparents, who then get higher weights. About 2.8% of mothers and 0.5% of fathers match to fewer than 5 nonparents.

3.2 Flexible Event Study Estimation

We create a series of event study datasets across exact-match groups (defined by month of service, job rank, reserve status, and calendar year of the prepregnancy time point of the match). We include all month-years of data from before and after the birth/placebo birth, then stack these exact-match-group datasets.¹⁷ Nonparents assigned placebo births approximate counterfactual time trends in outcomes that first-time parents would have experienced, assuming outcomes would have evolved similarly between the two. An unbiased estimate in this setting requires an assumption of conditional parallel trends between parents and placebos, which is a weaker assumption than that of unconditional parallel trends had we drawn on all nonparents from the sample (Roth et al., 2022).

We estimate a fully flexible event study specification separately for men and women as follows:

$$Y_{igtr} = \sum_{r=k_{min}}^{k_{max}} \mathbb{1}(t = t_{ig}^* + r)\theta_r + \pi P_i + \sum_{r=k_{min}}^{k_{max}} \mathbb{1}[(t = t_{ig}^* + r) \times P_i] \beta_r + \alpha_g + \phi_t + \varepsilon_{igtr} \quad (1)$$

where t_{ig}^* is the month-year of the real or placebo birth for individual i in match group g based on calendar time t . We measure month relative to birth as r . Coefficients θ_r estimate changes in the outcome for each month r after birth (or before, if $r < 0$). This is analogous to event time fixed effects, estimated among both parents and nonparent matches. P_i is a binary variable equal to 1 for all first-time parents, and we expect π to be zero given that parents and placebos are similar in the preperiod. Then, β_r represents how much the parents differ from their placebos at a particular time relative to birth. Effects are measured relative to month $r = -10$, which corresponds to 10 months prior to birth and approximately 1 month before the start of the pregnancy for the parents. We focus on month-by-month effects starting 24 months before birth through 24 months after for most outcomes; for job performance ratings we instead focus on effects starting at $r = -18$ because the evaluation requires workers to be on the job for roughly 6 months before it is typically assessed. We bin event time endpoints below $r = -24$ (or $r = -18$ for job performance) and

¹⁷Each month-year observed for a given parent will appear exactly once as they only have one first birth. A given month-year for nonparents may occur multiple times if the same nonparent is matched in multiple month of service/rank/reserve status/year cells. This matched individual would have different relative event-time points, defined by the time of their match and corresponding placebo birth, even for the same observations of calendar month-year.

above $r = 24$. Including binned event time endpoints allows us to estimate time fixed effects ϕ_t to account for month-by-year changes over time in the outcome (e.g., changes in fitness test standards in a particular year) separately from event time fixed effects. Excluding $|r| > 24$ and dropping the binned end points produces nearly identical results (see Appendix B). We include α_g to create a within-match-group comparison and ε_{igt} as the error term. Thus, θ_{12} represents the average outcome 12 months after birth for nonparents, relative to $r = -10$, while β_{12} estimates whether this change is more positive, more negative, or the same for parents.

Eq. (1) allows us to estimate prepregnancy differences in outcomes between first-time parents and nonparents, reflected by β_r when $r < -10$. Our placebo birth assignment procedure does not require outcomes between parents and nonparents to move together when $r < -10$. We expect β_r for $r = [-24, -11]$ to be zero if nonparents provide a suitable comparison to parents. We generally find evidence that this is the case, lending confidence to this estimation strategy.

3.3 Incorporating Linear Splines

We can improve precision in our estimates by using a semi-parametric spline specification, similar to Lafortune et al. (2018). This model is especially helpful for outcomes with smaller samples and those that are not observed every month. We modify Eq. (1) by defining parameters for level shifts in outcomes during pregnancy ($Pregnancy_{igtr}$), trends during pregnancy ($PregnancyTrend_{igtr}$), level shifts immediately following birth ($Postbirth_{igtr}$), and any recovery trends following the immediate impact of birth ($Recovery_{igtr}$), including any change in slope of the recovery trend during the second year postbirth ($\Delta Recovery_{igtr}$). We then interact these with an indicator for

being a parent (P_i) as opposed to a nonparent match. Our spline model is:

$$\begin{aligned}
Y_{igtr} = & \theta_0 \text{Pregnancy}_{igtr} + \theta_1 \text{PregnancyTrend}_{igtr} + \theta_2 \text{Postbirth}_{igtr} + \theta_3 \text{Recovery}_{igtr} \\
& + \theta_4 \Delta \text{Recovery}_{igtr} + \pi P_i + \beta_0 (\text{Pregnancy}_{igtr} \times P_i) + \beta_1 (\text{PregnancyTrend}_{igtr} \times P_i) \\
& + \beta_2 (\text{Postbirth}_{igtr} \times P_i) + \beta_3 (\text{Recovery}_{igtr} \times P_i) + \beta_4 (\Delta \text{Recovery}_{igtr} \times P_i) \\
& + \alpha_g + \phi_t + X_{igtr} \gamma_j + (X_{igtr} \times P_i) \delta_j + \varepsilon_{igtr}
\end{aligned} \tag{2}$$

where effects are measured relative to the prepregnancy average ($r \leq -10$), similar to Borusyak et al. (2021).¹⁸ Parents and their matches contribute to all coefficient estimates θ_j , while slope parameters β_j are specific to parents and captures any change above and beyond that of nonparents. We present a diagram and more details of this model in Figure A.3. We use this semi-dynamic spline specification to estimate postbirth effects for men and women at key time points (e.g., 12 and 24 months postbirth). The vector X_{igtr} includes binary indicators for binned event time endpoints for event times $r < -24$ (or $r < -18$ for job performance) and event times $r > 24$ for both parents and matched nonparents to mirror our estimation strategy from Eq. (1).

Abadie and Spiess (2021) show that clustering should be at the match-group level when doing matching without replacement to account for within-group correlation induced by the matching procedure. That does not solve the problem in matching with replacement that individuals' outcomes are correlated if they are part of multiple matched groups. To address this latter concern, we include two-way clustering at the individual and match-group level.

¹⁸Using all pretreatment periods is more efficient than using only the period just before the event takes place, but it is also more biased if parallel trends do not hold (de Chaisemartin and D'Haultfœuille, 2021). The event study estimates from Eq. (1) use the period just before treatment as the reference group and thus avoid this issue.

4 Results

4.1 Job Outcomes

Figure 2 presents results from Eq. (1), our flexible event study model, estimated separately among men and women. The bottom left of each panel includes the p -value of an F -test of whether the prepregnancy point estimates jointly equal zero. A joint zero estimate during the prepregnancy period increases our confidence in the suitability of nonparents as a comparison group. Figure A.4 provides additional evidence of the suitability of nonparent comparisons, displaying unadjusted mean outcomes over time for parents and placebos. Figure A.5 displays differences in outcomes between mothers and fathers in a triple difference event study framework as an additional check.

Figure 2 Panel A presents event study results on job-related physical performance scores. The prebirth period is not jointly statistically different from 0 (p -value of an F -test of joint significance=0.152), indicating that before giving birth, mothers' average scores did not differ from nonmothers. We exclude outcomes for women during pregnancy and seven months after birth, as policies allowed women to opt out of the physical assessments while pregnant and postbirth. Once women take the test postbirth, their performance declines are large and persistent. Even 24 months postbirth, women's physical performance scores are lower than expected, after accounting for general time trends using the nonparent women. For men, performance declines begin during the mother's pregnancy and reach their lowest point 1 month postbirth before beginning to recover.

Figure 2 Panel B shows evidence of lower supervisor ratings of job performance for women in the two years postbirth. Estimates are noisier than for physical performance because we are missing observations for those who left the Marines before 2017. Having a child does not appear to affect fathers' supervisor-rated job performance.

Finally, Figure 2 Panel C shows parenthood is unrelated to marksmanship for mothers. Recall, women are not tested during pregnancy nor while on leave following birth. If anything, marksmanship improves for fathers. Marksmanship assessments are infrequent, thus estimates are noisy.

Table 2 displays results from our spline model, Eq. (2). This model has the advantage of

smoothing the estimates using data in nearby time points, which is particularly helpful for estimates of outcomes that are not observed monthly (i.e., physical performance, supervisor ratings of job performance, and marksmanship). Rather than present coefficients for each spline parameter (e.g., the slope of the postbirth period), we predict effects at various time points using coefficients from the model.¹⁹ Estimates show changes in parents' outcomes from prepregnancy to the time specified, net of secular trends in the outcome.

Beginning at 8 months postbirth, mothers' physical performance is 0.50 standard deviations below their expected average. Mothers recover somewhat, and by 12 months postbirth their scores are 0.29 standard deviations below expectations. Mothers' physical fitness recovery slows in the child's second year of life. Two years after having a baby, mothers' predicted physical performance remains 0.18 standard deviations lower than before the pregnancy.²⁰

Mothers' supervisor ratings do not change during pregnancy (see Appendix Table A.2). One month postbirth, mothers' performance ratings are 0.17 standard deviations lower than expected, relative to changes in the placebos. This difference is 0.18 standard deviations at one year postbirth and a non-statistically significant 0.07 standard deviations by 24 months postbirth. Marksmanship scores are lower for mothers than their placebos after having a child, though by 24 months postbirth the difference is no longer statistically significant.

The patterns for fathers' job-relevant physical performance is consistent with mothers' but smaller in magnitude (Panel B of Table 2). Fathers' scores begin falling during pregnancy (see Appendix Table A.2), then drop to 0.12 standard deviations below expectations in the month after birth. By 12 months postbirth, the fathers are 0.05 standard deviations below expectations, and the effect is a precise zero by 24 months postbirth. For job performance, scores are slightly negative in the first month postbirth, but return to match the placebos by the child's first birthday. For marksmanship, if anything, fathers may improve over time.

¹⁹We include the slope parameters in Appendix Table A.2 for reference.

²⁰Physical performance assesses a combination of cardiovascular health, endurance, and strength. Appendix Table A.3 presents raw scores for each item on the fitness assessments. Mothers run more slowly (i.e., run times increase) and complete fewer crunches, pull-ups, and lifts.

4.2 Training, Education, and Promotion

We next turn to human capital development and promotion outcomes. These outcomes are recorded monthly, meaning every sample member has an observation for every month-year, giving us more precision in our estimates.²¹ Figure 3 presents results from Eq. (1), our flexible event study model. Figure A.6 displays unadjusted mean outcomes over time for parents and their placebos. Figure A.7 displays the differences in outcomes between mothers and fathers in a triple difference framework.

Figure 3 Panel A shows the effects of parenthood on months of job-specific training among mothers and father separately. The Y-axis measures total months of training relative to $r = -10$. Prepregnancy point estimates do not jointly differ from zero, but there appears to be a downward trend. Yet, starting in pregnancy mothers' accumulation of months spent in training slows even more relative to nonmothers'. The gap does not close after birth. Table 2 shows that the gap grows from 0.41 months immediately after birth to 0.83 months 24 months postbirth, about a 51.6% difference from the nonparent mean growth of 1.61 months in $r = [-10, 24]$. We do not observe any meaningful impacts on fathers' accumulated job-specific training. We control for a linear pretrend in Appendix Table A.4. The magnitude of the effects among mothers is smaller, though the addition of more model constraints also increases the standard error on the primary coefficients. Among fathers, controlling for pretrends suggests fathers' time spent in training increases relative to nonfathers after birth. We interpret training results with caution, given that estimates are sensitive to the specification.

Figure 3 Panel B shows the impact of a first birth on total years of formal education, a measure of general human capital. Mothers have slightly lower educational attainment than their placebos by 24 months postbirth, but there is some evidence of pretrends; when we control for pretrends in Appendix Table A.4 we do not observe any impact of a birth on mothers' total years of education. Among fathers, there is a statistically significant but practically small increase in education relative to the placebos (+0.01 years at 24 months post) that disappears when we control for pretrends in the outcome. The magnitude of the education effect is small in all models. Our overall conclusion

²¹Education is missing for 0.3% of respondents; these individuals are excluded from the education analysis.

is that the transition to parenthood has minimal impacts on Marine parents' educational attainment.

The final row in Figure 3 displays promotions, which is a cumulative count relative to $r = -10$. Mothers and their placebos move together before pregnancy, but a gap emerges during pregnancy, even before the birth. Mothers never catch up to their placebos; if anything, the gap grows over time. From Table 2, the promotion gap is 0.03 at $r = 1$ but grows to 0.09 promotions by $r = 24$. The placebos averaged 1.31 promotions in $r = [-10, 24]$, so this is a 6.7% reduction. Fathers are statistically about the same as their placebos on this outcome.²²

We conduct a placebo analysis in Appendix Table A.5 to confirm results are not mechanically created by the matching algorithm. Results are statistically indistinguishable from zero, as expected.²³ Appendix Table A.6 shows a descriptive analysis to assess whether the other five outcomes predict the cumulative count of promotions among placebo mothers, placebo fathers, and actual fathers. Physical fitness consistently predicts promotions across groups, with a 1 standard deviation increase in average physical performance in $r = [-9, 23]$ predicting 0.15 more promotions for placebo mothers, conditional on baseline descriptive statistics (p -value=0.000).²⁴ Mean supervisor ratings and marksmanship scores also predict promotions for placebo mothers. This analysis provides evidence that the job performance measures we study are relevant for promotion.

4.3 Heterogeneity across Subgroups

Subgroup analyses inform whether the impacts of parenthood are broad-based or concentrated among specific groups in ways that might point at the underlying mechanisms driving our results.

We focus first on differences in effects between reserve and active duty service members. Most

²²In a similar analysis, we find comparable negative effects on mothers' fitness scores and promotions outcomes in the Army and Navy. Fathers are not affected. Different branches have different promotion processes, which limits the likelihood that the promotion effect is mechanical or specific to the Marine promotion system.

²³To conduct the placebo analysis, we remove all mothers from the sample and randomly assign women from the potential matching pool as placebo parents. We run the LASSO model and matching process to identify matches to these placebos, then run the main analysis. The pretend mothers do not differ from their matches.

²⁴Predicting promotions is difficult because it is a dynamic, continuous process, which makes it difficult to model which skills matter – and when they matter. This is a simple model that averages from pregnancy start to 23 months postbirth for the job outcomes, and predicts cumulative promotions at 24 months. We focus on the nonmother groups so that motherhood effects do not complicate the analysis.

reservists have full-time civilian jobs but the same physical and job performance requirements as active duty Marines. If we see larger (or smaller) impacts of parenthood among reservists, it may give a sense for how our findings would translate to civilians as compared to those who work full-time as service members.²⁵ We then consider variation in impacts based on whether the Marine parent works in an especially physically demanding job. All Marines are expected to maintain job-related physical fitness, but those in more physically demanding roles may have different incentives to stay fit or more ground to lose in terms of their physical ability after childbirth.²⁶ Next, we explore impacts among unmarried parents, those with potentially more caregiving responsibilities and less support at home. Our last three subgroups of interest include: (1) junior enlisted who may be less attached to their jobs than senior enlisted or officers, less able to advocate for themselves, and who are more frequently eligible for promotions and thus have more potential ground to lose; (2) parents who have a second child within 24 months after the birth of their first child and who may drive longer-run effects 24 months postbirth; and (3) Marines who leave the military within three years after having a child and who may be less invested in recovering their performance.

We conduct subgroup analyses by interacting an indicator variable for a characteristic (e.g., reservist) with the variables from Eq. (2) to measure whether the parenthood effect differs by subgroup.²⁷ We lack power to test small differences between subgroups of mothers but are well-powered to detect even small differences among subgroups of fathers. We present results from heterogeneity analyses in Figure 4, displaying physical fitness, training, education, and promotion outcomes. We are underpowered to detect subgroup differences in job performance ratings and

²⁵We do not have the power for subgroup analyses for physical performance, job performance, and marksmanship scores for women reservists due to a small number of female reservists and the sporadic nature of the assessments.

²⁶We categorize Marines as working in a high-physicality (*> physical*) or low-physicality (*< physical*) job based on whether their job responsibilities place them in the top or bottom half of our sample's distribution of O*NET's dynamic strength index. We exclude individuals working in jobs with no link to an O*NET classification (about 9% of mothers and 32% of fathers). The median mother in the low-physicality group is equivalent to an office clerk or cartographer; the median father in the low-physicality group is equivalent to a musician or administrative services manager. For both mothers and fathers, the median in the high-physicality group is equivalent to an aircraft mechanic.

²⁷Definition of the subgroup is based on the matched parents' characteristics for the placebos, rather than characteristics of placebos themselves. This ensures placebos are in the same group as their matched parents, even if they do not exactly match on a characteristic. We use $r = -10$ to define most subgroups, but use $r = 0$ for marital status because we are most interested in family characteristics when the baby arrives. This time gives couples surprised by a pregnancy some time to marry.

marksmanship scores and present these results in Appendix Figure A.8. Cross-group differences that differ at the 1% level have filled-in markers in Figures 4 and A.8. We focus our discussion on subgroup patterns in physical fitness and promotion outcomes, our two most robust results.

Declines in physical performance and promotion among mothers appear largely broad based, with two key exceptions.²⁸ There appears to be no lasting impact of parenthood at $r = 24$ on physical fitness and promotion outcomes among (1) senior enlisted/officers and (2) mothers who remain in the Marines 36+ months postbirth. In other words, negative physical fitness and promotion impacts are concentrated among junior enlisted and mothers who leave the military within three years of having a child. It is perhaps not surprising that promotion effects are concentrated among junior enlisted rather than officers. Officers' expected promotion cadence is less frequent and often requires more than two years between promotions. Many officers who have a child may not be eligible for promotion during the two year period following childbirth. It is more difficult, however, to interpret the pattern of results for mothers who leave the military within three years of having a child vs. those who stay; mothers who leave may not have strong incentives to perform well on military-specific tasks because they plan to leave, or they may leave *because* of birth-related performance declines. We cannot distinguish between these two possibilities.²⁹

Among fathers, we observe lower physical performance immediately postbirth, but the size of the impact varies across groups. Reservists, those in more physical jobs, single fathers, junior enlisted, and those who stay on the job for less than 36 months after a birth experience larger negative effects than their counterparts. Moreover, null main effects on training, education, and promotion mask some subgroup differences. For instance, fathers working in less physical jobs experience declines in months of training, increases in years of education, and more promotions at 24 months postbirth. Fathers who remain employed as Marines for 36 months or longer after a birth show increases in months of training, years of education, and number of promotions. There

²⁸Across subgroups, almost all mothers have lower physical performance when measured again after the birth and 24 months later, but there is some variation in the size of the effects. For example, single mothers, those from more physical jobs, and those who have a second child experience larger declines in their outcomes, but even married mothers, those from less physical jobs, and those who do not have a second child still experience the negative impacts of parenthood at $r = 24$.

²⁹In our sample, 25% of parents leave the Marines in the third year of their child's life (25–35 months postbirth).

is some indication, then, that among fathers in less physically demanding jobs and among those who remain employed for three years after a birth, the impact of parenthood could be positive.³⁰

4.4 Channels Behind Delays in Mothers' Promotion Trajectories

In our setting, workers are promoted as they accumulate tenure and perform their job roles well. At the same time, discrimination can shape promotion outcomes. Assessments may be subjective and the decision to promote someone may involve a degree of individual discretion.³¹ We document declines in supervisor-rated job performance among mothers after childbirth, and these ratings allow for more subjectivity than, say, the timed three-mile run on physical fitness tests. Supervisor performance ratings are also the most heavily weighted input into the promotion system. Lower job performance ratings would reduce promotion likelihood more than similarly sized declines in physical fitness scores, making bias on supervisor performance ratings even more consequential.

To explore for potential bias against mothers, we conduct an extension of our main analysis where we focus on Marine men who experience a medical event that impacts their ability to work. If men with medical limitations display worse job-related outcomes, including fewer promotions, we can rule out mother-specific (i.e., birth-related) discrimination as the sole driver of declines in job performance and promotion among mothers.³²

We first capture whether men who do not have a child during the study window have an official “medical” status in their files or miss a fitness test with a medical waiver. Both of these designations are ones that women receive when exempted from certain work responsibilities (e.g., deployment and fitness tests) during pregnancy and the postpartum period. Medical events outside of pregnancy

³⁰In a similar analysis on only married individuals, Marine fathers married to active duty individuals have consistently larger increases in promotion compared to those married to civilians. There are no differences for married Marine mothers by whether her spouse is active duty.

³¹Structural features of the promotion process, rather than individual-level discrimination, may also constrain the odds of promotion for certain groups. For example, assessments may systematically disadvantage women even if they are gender-neutral on their face (e.g., testing abdominal strength of all Marines despite that this will be more difficult for women soon after childbirth).

³²Note, we cannot rule out discrimination against mothers entirely. Some of the effects of motherhood might still be due to discrimination. However, we feel confident the *entirety* of the mother effect is not driven by mother-specific bias if men who are not fathers also show declines in performance and promotion outcomes after illness/injury.

and birth are infrequent but similar in incidence to parenthood/birth. Of Marines who do not have a baby in the study window, 11.5% have a medical event (as we define it) in their records, while 14.4% of Marines have a baby in the study window. Medical events include physical injuries (e.g., stress fractures or severe shoulder pain) and other health conditions that rise to the level of interfering with daily work function (e.g., discovery of a kidney tumor that requires surgery). We do not observe details in the administrative records on the exact nature of men's medical conditions that we include in our analysis. All medical statuses we observe require some restricted work duty (from 30 days up to 6 months), and 40% of the medical statuses we see are the more serious of two possible administrative designations (medically restricted "limited duty") at their onset.

We identify the first appearance of a medical status among men as $r = 0$. We then proceed as in the main parent analysis by identifying a group of nonmedical comparisons at $r = -10$. We allow 10 months between the match and start of the medical status as we do with childbirth in case there is a delay in recording the medical status measure (e.g., a person is injured, but their command only updates their status when it causes them to miss a fitness test). We include only nonparents in this analysis to avoid contamination with parenthood effects. We focus on men to obtain a large enough sample of medical events and avoid the potential for gender bias against women.

Figure 5 shows men whose records have a documented medical status perform 0.2 standard deviations worse on physical fitness tests initially following status onset; this is an underestimate of the effect many Marines with more extreme medical limitations are exempt from physical fitness tests initially. The physical performance impact dissipates in magnitude but remains around 0.1 standard deviations lower at $r = 24$. Job performance, months of training, and promotions also decline for men who have a medical event. Twenty-four months after we observe the medical status turn on, men are rated 0.1 SD lower on job performance evaluations, accumulate 0.6 fewer months of training, and achieve 0.07 fewer promotions relative to their matched comparisons. While we see some pre-period differences among the sample of medically limited men and their matched comparisons, the pre-period differences are small compared to the post-period changes.

"Medical events" capture a range of physical conditions and limitations, so it is not clear

whether we would expect men on a medically restricted duty status to experience larger, smaller, or same-sized effects as new mothers.³³ Nevertheless, the analysis provides evidence that, like mothers, men with medical limitations score more poorly on assessed physical fitness, receive lower job performance ratings, accumulate less training, and experience slower promotions. These findings suggest *against* discrimination against mothers as the only driver of the impacts of parenthood on mothers. Instead, physical limitations (and any potential time away from work or increased time demands related to recovery) are associated with delayed promotion. That said, because women experience pregnancy and childbirth, the professional consequences of childbirth—even if similar to other physically limiting medical events—will disproportionately affect women in this setting.

4.5 Variation by Maternity Leave Length

Prior to 2015, all DoD branches provided active-duty women 6 weeks of paid maternity leave. In July 2015, the Secretary of the Navy announced that primary caregivers in the Navy and Marine Corps would be entitled to 18 weeks of leave. Women who had given birth earlier in January 2015 or later could retroactively take advantage of the 18-week leave policy before their child turned one. Women who had already returned to work after 6 weeks of leave tended to use the additional 12 weeks of paid time off discontinuously (i.e., as flexible time off). Women who were on leave at the time of the announcement of expanded leave, or gave birth after the announcement, generally took the additional leave consecutively (Bacolod et al., 2022). We analyze these groups separately, referring to the different leave arrangements as “6 weeks + 12 flex” to indicate discontinuous extended leave used as flexible time off, and “18 weeks” to signal the stretch of continuous extended

³³For this reason, we focus on the impact of medical events among men alone, not compared to the impact of motherhood on women. However, we can test differences in the size of the impacts between medically limited men and mothers in a triple difference framework. We present these results in Appendix Figure A.9 and then replicate the analysis using only a subset of men where, at onset, the medical event was classified as the more serious of two possible administrative designations (known as medically-restricted limited duty) with results shown in Appendix Figure A.10. The motherhood effect is larger (on average) than the effect of medical events on men but more similar in magnitude to the impact of medical events on men when limited to only the more serious “limited duty” status medical restriction. We also note that mothers in our sample are more junior than men assigned to a medical status. This means mothers are more likely to receive promotions in the two-year window we follow. As a result, we may detect larger impacts on mothers’ promotions than among medically restricted men, given that the men are more senior and less likely to get promoted as quickly (absent any treatment).

weeks of leave. In early 2016, the Secretary of Defense standardized maternity leave to 12 weeks for all military branches. The 12-week policy applied to pregnancies that began 31 days after the announcement (i.e., pregnancies that began on March 3, 2016, or later, per doctor estimation).

We disaggregate the effects of having a child on women’s outcomes by the length of maternity leave, determined by when she gave birth. The key question of interest is whether longer leave predicts better or worse outcomes when women return to work. Variation in leave length is, at times, quasi-randomly assigned. Some policy changes were unexpected and applied to women who were already pregnant, while other changes were prospective, allowing women to potentially select into parenthood and a particular leave policy. Selection presents the biggest concern for women who gave birth under the latter part of the 18-week policy and all of those who gave birth under the 12-week policy, given that these women would have known their leave length was greater than 6 weeks before becoming pregnant. We focus on women who expected to receive 6 weeks of leave at conception and were surprised with the announcement of additional leave. We compare three distinct lengths of leave: 6 weeks, 6 weeks + 12 flexible weeks after returning to work, and 18 weeks. Appendix Table A.7 presents descriptive characteristics of the women who gave birth under these three leave policy groups. There are some differences across groups, though not in any ways that suggest systematic bias in one policy regime or another.³⁴ We conduct the analysis by defining indicator variables for the “6 weeks + 12 flex” and “18 weeks.” The “6-week” policy is the baseline group. We interact the additional leave indicators with the variables in Eq. (2) and make policy-specific predictions for the initial birth drop (i.e., 8 months for physical performance and 1 month for training, education, and promotion), 12 months postbirth, and 24 months postbirth.³⁵

³⁴There are statistically significant differences in percent of officers, years of education, and combat job type. If there was selection into fertility, we may expect it to be discontinuous at the policy change. Figure A.11 shows month-by-month variation in the density of births, including a test for any discontinuity across policy thresholds, following Cattaneo et al. (2018). None of the differences across the policy thresholds reach statistical significance, suggesting the policies did not influence female fertility itself. In supplemental analyses we set aside our concern about selection into birth and include mothers who knew they would receive additional leave at conception; that analysis includes more observations in the “18 weeks” period and an additional group of women under the “12 weeks” of leave period. Appendix Table A.8 is the balance table for this sample; the results are in Appendix Table A.9. Because babies born in November–December 2016 could have fallen under either the 18- or 12-week policy depending on date of conception, we exclude these mothers (and their matches) from the policy analysis.

³⁵We do not include supervisor-rated job performance or marksmanship. Supervisor ratings for junior enlisted are only available for those who remained in service as of October 2017, which complicates analyses of policy changes

Table 3 first replicates the main analysis for the subsample who expected 6 weeks of leave at conception (and their matches) and then shows results by policy period. Across all leave policies, we find physical performance drops when initially observed 8 months after having a baby. Mothers with longer maternity leave had larger initial declines, particularly those under the “6 weeks + 12 flex” policy. These mothers returned to work after 6 weeks of leave, then received 12 additional weeks they could use discontinuously by their child’s first birthday. An F -test of physical performance differences across policies at $r = 8$ produces a p -value of 0.054. Mothers with flexible leave (+12 weeks) also had larger gaps in education at $r = 12$ ($p(\text{diff})$, all effects=0.058) and $r = 24$ ($p(\text{diff})$, all effects=0.017), though they had the smallest gaps in training by $r = 24$ ($p(\text{diff})$, all effects=0.055). We take this as evidence that parental leave length has limited effects on the outcomes we study. We note standard errors are relatively large given our small sample size.³⁶

5 Summary and Conclusions

We use repeated, direct measures of work performance, human capital accumulation, and career advancement to explore the link between parenthood and workers’ outcomes. We combine an event study approach based on the timing of a first birth and a matching design that assigns placebo births to observably similar nonparents and find both men’s and women’s physical performance respond negatively to the transition to parenthood. However, mothers experience larger declines in job-related physical performance that persist for two years postbirth, while fathers experience smaller, shorter-lived declines that fade by their child’s second birthday. Women’s supervisor-rated job performance and marksmanship scores also decline in the year after having a child, while men’s do not. Mothers’ accumulated time in on-the-job training slows, while fathers are largely unaffected. These patterns are consistent with our finding that women’s promotion trajectories slow during pregnancy and after birth, while men’s do not. Among women, promotion delays accumulate

that took place in 2015 and 2016. We lack power to subdivide marksmanship scores given sparse observations.

³⁶We also estimate impacts of variation in leave on fitness and promotion in the Army/Navy context where there is more power to detect effects. The effects do not differ by leave length.

over time; the difference in number of promotions between mothers and nonmothers is largest 24 months postbirth. Nonfathers with documented medical limitations at work also experience lower physical performance, supervisor ratings of job performance, and training, suggesting that discrimination against mothers does not exclusively drive our main findings.

The small declines we observe in job-relevant physical fitness among fathers (who do not give birth) suggest that factors outside of pregnancy and birth, such as sleep deprivation or time constraints, can affect work performance. Consistent with this finding, research shows non-birthing mothers (e.g., mothers who adopt children or whose same-sex partners give birth) also experience child penalties to earnings, suggesting against the biological impacts of birth as a key mechanism (Andresen and Nix, 2022; Kleven et al., 2021). Many of the impacts on mothers in our sample last beyond a year postbirth, at which time mothers have largely recovered physiologically from pregnancy and birth, both in terms of hormone levels (Chauhan and Tadi, 2022) and from changes to their underlying biological cardiac functioning that occurred during pregnancy (Melchiorre et al., 2016). Yet, research suggests the sleep and time effects of parenthood persist for years. Parenthood disrupts adults' sleep for up to six years postbirth (Parsons et al., 2023), and many women report struggling with nutrition, exercise, and other wellness behaviors after having a child (Bellows-Riecken and Rhodes, 2008; Declercq et al., 2014). We consider these patterns as support for the idea that the time and sleep effects of motherhood may, in part, drive declines in the determinants of wage for Marine women, especially the declines that persist beyond a year postbirth.

Slower career advancement for mothers in our setting will directly lead to gender-based pay gaps, given that a Marine's job rank determines their pay grade. At the same time, the pay increase a Marine receives when promoted tends to be small, meaning the size of the created pay gap due to the promotion impacts of parenthood will also be small.³⁷ By 24 months postbirth, the average mother would make \$40,596 in basic pay, according to the Marine Corps pay schedule (excluding any bonuses or housing allowances).³⁸ The impact of birth on promotions means that mothers go

³⁷For example, an E4 with 4 years of service made approximately \$350 per month more than an E3 with the same job tenure in 2022.

³⁸We use 2022 basic pay scales for this estimate. Basic pay is calculated by years of service and rank. Marines also have housings allowances that increase with first dependents (thus counteracting the mothers' gap for single mothers)

from an average of \$0 difference in pay compared to matched nonmothers at 10 months before birth to \$332 lower pay 24 months postbirth. The mother-father wage differential grows from \$5,789 10 months before birth to \$5,890 at 24 months postbirth (a \$101 increase in the wage gap).³⁹

Last, we find that additional maternity leave does not alter the size of the motherhood penalty in our setting, especially when we examine differences in effects two years after birth. Other research, mainly in European contexts, shows longer parental leaves of one year or more do negatively impact women's labor market outcomes (e.g., Ginja et al., 2023; or see Rossin-Slater (2018), for a review). In our context, however, even the longest DoD leave policy was relatively short (18 weeks). If the goal of maternity leave is to provide bonding time with children and physical/mental recovery time for mothers, our findings may be good news: declines in job-related outcomes were not exacerbated by more generous parental leave policies of the lengths studied in our setting.

Our results come from data on a select population—the U.S. Marines—where physical health is an essential requirement for effective job performance. However, many civilian jobs are also physically demanding. To contextualize our results and their potential application to jobs other than the Marines, we present the share of women in occupations in the U.S. by the amount of strength and stamina required in the top panel of Figure 6. The figure draws on O*NET data on work-related physical ability requirements and connects that data to the U.S. Department of Labor's employment and earnings data from 2021 using Standard Occupational Classification (SOC) codes. We then weight occupations shown in Figure 6 by the number of women in that occupation in the U.S. as of 2021. We find that over 25% and 35% of civilian women worked in occupations that required at least the same strength and stamina, respectively, of the average Marine woman's job, suggesting our paper's results may apply to a sizable share of women in the civilian labor market.

We also consider whether giving birth might be particularly costly for women in more physical jobs where, to begin with, women tend to be underrepresented. We rely on estimates from Fontenay et al. (2023), who characterize variation in the size of child penalties across 12 industries using data

and also increase with rank (thus increasing the gap if promotions are delayed), as well as other bonuses or incentives that may differ for mothers (e.g., combat pay). We focus on basic pay because it aligns most closely to civilian wage and is straightforward to calculate.

³⁹Higher average prepregnancy ranks among fathers compared to mothers generate a prepregnancy wage gap.

from Belgium. We crosswalk O*NET data on physical ability requirements for SOC occupations in the U.S. to the industries they study. We then regress their estimates of the child penalty in each industry on the mean strength and stamina required (weighted by the number of women in the U.S. in that industry). The bottom panel of Figure 6 presents our results, and Data Appendix B.3 contains additional details. We find that for every 10-point increase in strength or stamina, the child penalty is 4.3 or 4.8 percentage points higher, respectively. With only 12 data points and a rough occupation-to-industry translation, our estimates are suggestive and noisy. Nonetheless, we find that child penalty is correlated with the physical requirements of the job, with more physically demanding jobs having larger child penalties. Thus, understanding the postbirth dynamics of new parents may be particularly important for women in these occupations and industries.

Our findings provide a new angle on the longstanding literature that shows parenthood reduces mothers' employment, hours worked, and wages, while having minimal effect on fathers. Most papers in this literature have focused on either (a) administrative data on the universe of workers or (b) specialized, high-skill professions that tend to be less physical in nature (e.g., professors or lawyers). We offer insight into the factors behind the *within-employer* wage effects of parenthood for women by focusing on a unique setting with rich data that allows us to speak to the effects of parenthood in more physically demanding work contexts. The present research also takes place in an environment with guaranteed health insurance coverage, fully paid parental leave, and (where available) subsidized childcare. Yet, the demands of parenthood still impact key determinants of wages for women. Our findings indicate that keeping mothers in the labor force, working the same hours will not eliminate child penalties to women's earnings.

Finally, that delays in promotion accumulate for women, but not for men, in the years following birth also underscores the need for increased policy- and firm-level support for recent parents. The Pregnant Workers Fairness Act, signed into law in December 2022, requires employers to provide reasonable accommodations to workers' pregnancy- and childbirth-related medical conditions. Such accommodations are in line with our results on the particular importance of the postbirth period and may be especially important for new parents in physically demanding jobs.

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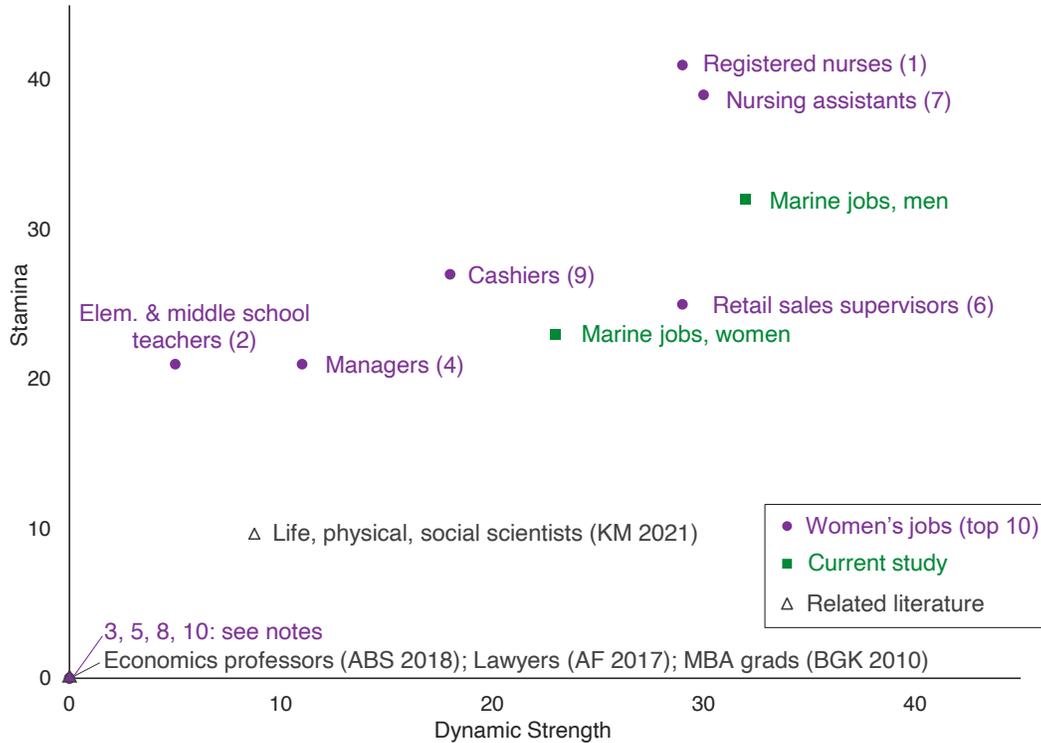
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6 Tables and Figures

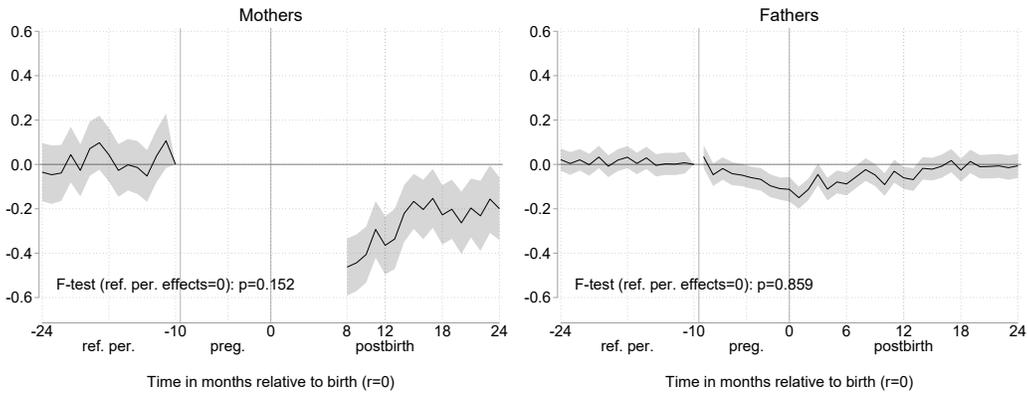
Figure 1: Distribution of Dynamic Strength and Stamina Required by Various Occupations



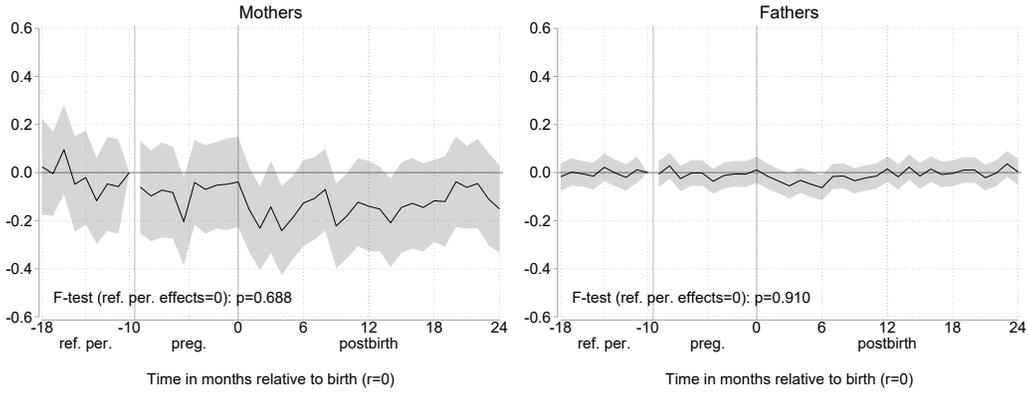
Notes: Figure displays required dynamic strength and stamina based on O*NET data for various jobs (National Center for O*NET Development, 2022). Top 10 occupations for women in the United States come from the U.S. Department of Labor (Department of Labor, 2022), with the rank given in parentheses. Top women's occupations with no strength or stamina requirements include: administrative assistants (3), customer service representatives (5), accountants and auditors (8), and receptionists (10). Estimates of the job requirements for Marines display the median O*NET score obtained by connecting Marines' military occupations to their civilian equivalents and giving any military occupation without a civilian equivalent the highest observed level (firefighters with dynamic strength = 56 and stamina = 54). Prior related literature has examined changes in productivity and career advancement around birth among physical, biological, and social scientists (Kim and Moser, 2021); economics professors (Antecol et al., 2018); lawyers (Azmat and Ferrer, 2017); and MBA graduates in the corporate and financial sectors (Bertrand et al., 2010). We designate these papers as KM 2021, ABS 2018, AF 2017, and BGK 2010, respectively. Plotted data for the scientist category weights O*NET requirements for occupations in the physical, life and social sciences by their 2019 distribution. Plotted data for the MBA graduate category relies on common occupation destinations for these graduates in the corporate and financial sectors (e.g., chief executive, financial managers, management analysts), which have zero strength or stamina requirements.

Figure 2: Event Study Estimates of the Impact of Birth on Job Outcomes

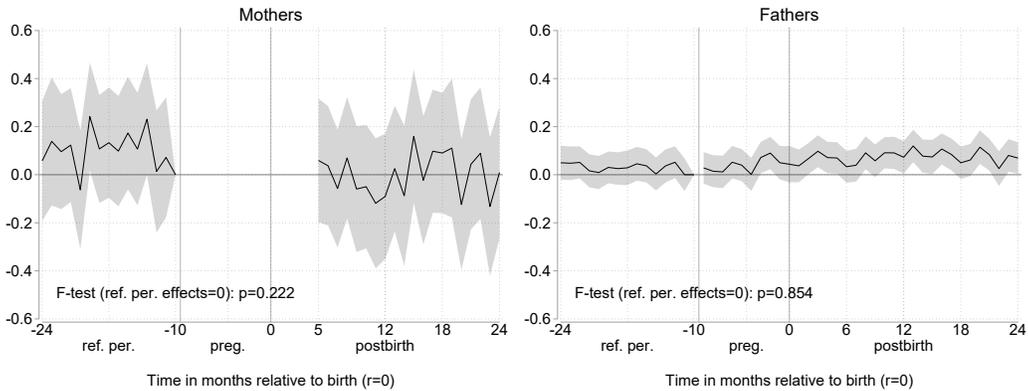
(a) Physical Performance (sd)



(b) Job Performance (sd)



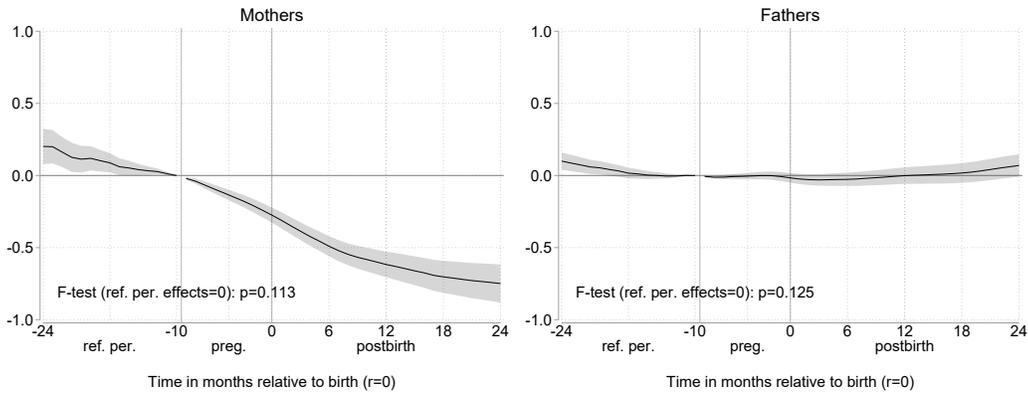
(c) Marksmanship (sd)



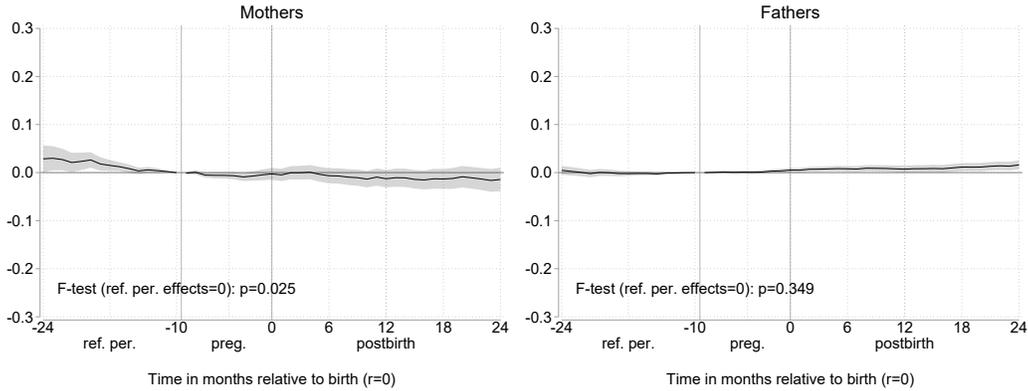
Notes: Figure displays coefficients from event study regressions using the placebo matched sample. Outcomes include standardized scores from (a) physical/combat fitness tests, (b) job performance evaluations, and (c) rifle/pistol marksmanship evaluations. We require nonparents be an exact match on rank, number of months in service, reserve status, and observation year as parents at $r = -10$. Among those, we match parents to a maximum of five most similar nonparents in their propensity to have a child based on age, race/ethnicity, AFQT scores, marital status (and if their spouse is in the military), education level, months of training, occupational field, and most recent physical performance score as of $r = -10$. Regressions include match-group and month-year fixed effects. The reference month is $r = -10$. Vertical lines reflect the start of the pregnancy ($r = -9.5$) and birth ($r = 0$). Standard errors are clustered by individual and match-group and are included as shaded areas representing a 95% confidence interval.

Figure 3: Event Study Estimates of the Impact of Birth on Human Capital and Promotions

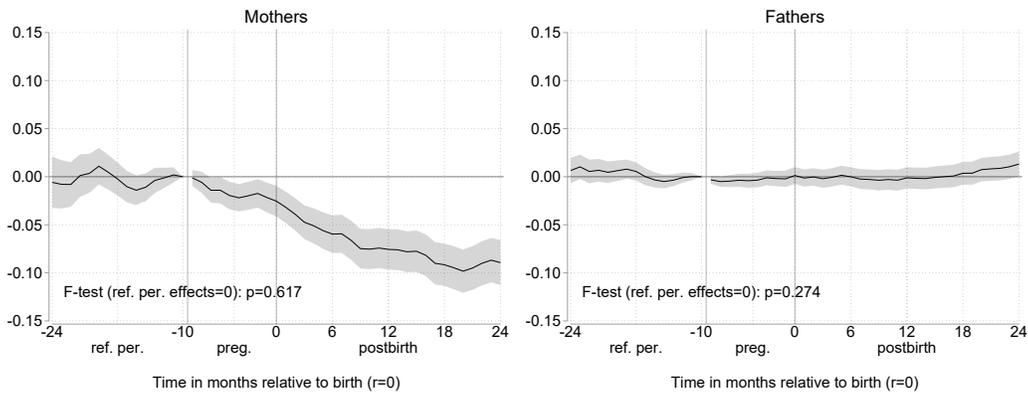
(a) Training (months)



(b) Education (years)



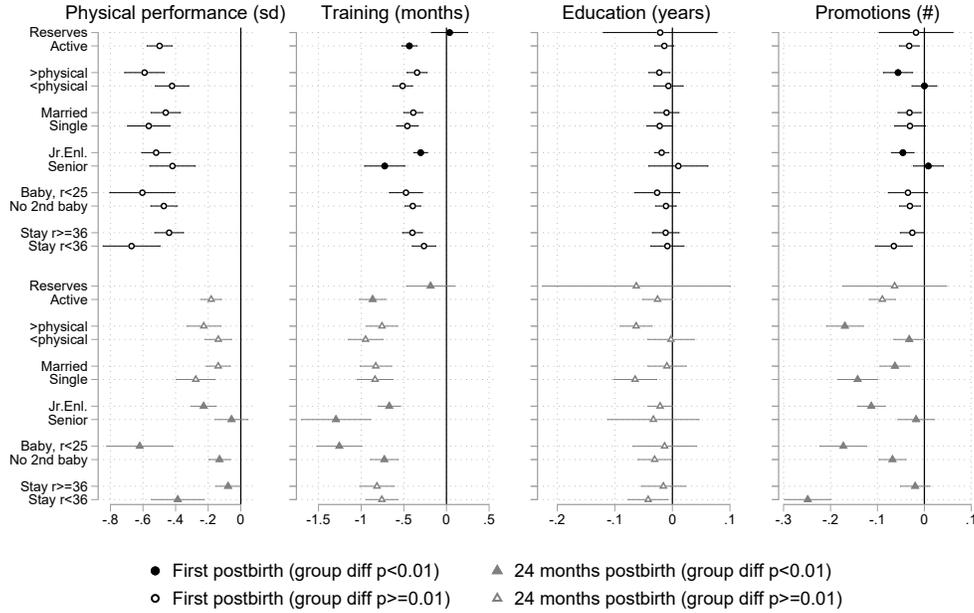
(c) Promotions (#)



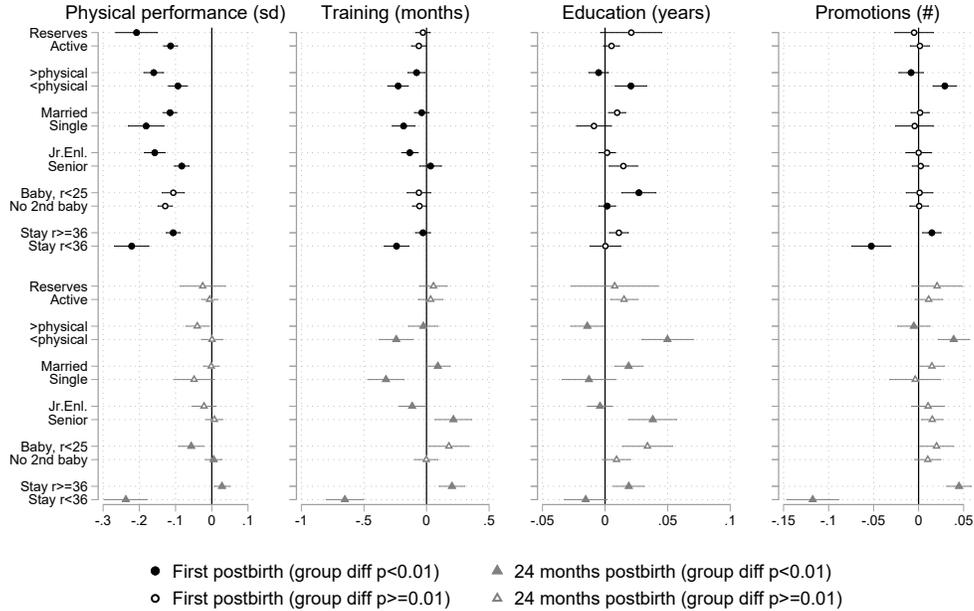
Notes: Figure displays coefficients from event study regressions using the placebo matched sample. Outcomes include (a) cumulative months of training (relative to $r = -10$), (b) cumulative count of years of education (relative to $r = -10$), and (c) cumulative count of promotions (relative to $r = -10$). We require nonparents be an exact match on rank, number of months in service, reserve status, and observation year as parents at $r = -10$. Among those, we match parents to a maximum of five most similar nonparents in their propensity to have a child based on age, race/ethnicity, AFQT scores, marital status (and if their spouse is in the military), education level, months of training, occupational field, and most recent physical performance score as of $r = -10$. Regressions include match-group and month-year fixed effects. The reference month is $r = -10$. Vertical lines reflect the start of the pregnancy ($r = -9.5$) and birth ($r = 0$). Standard errors are clustered by individual and match-group and included as shaded areas representing a 95% confidence interval.

Figure 4: Event Study Estimates of Subgroup Heterogeneity

(a) Mothers

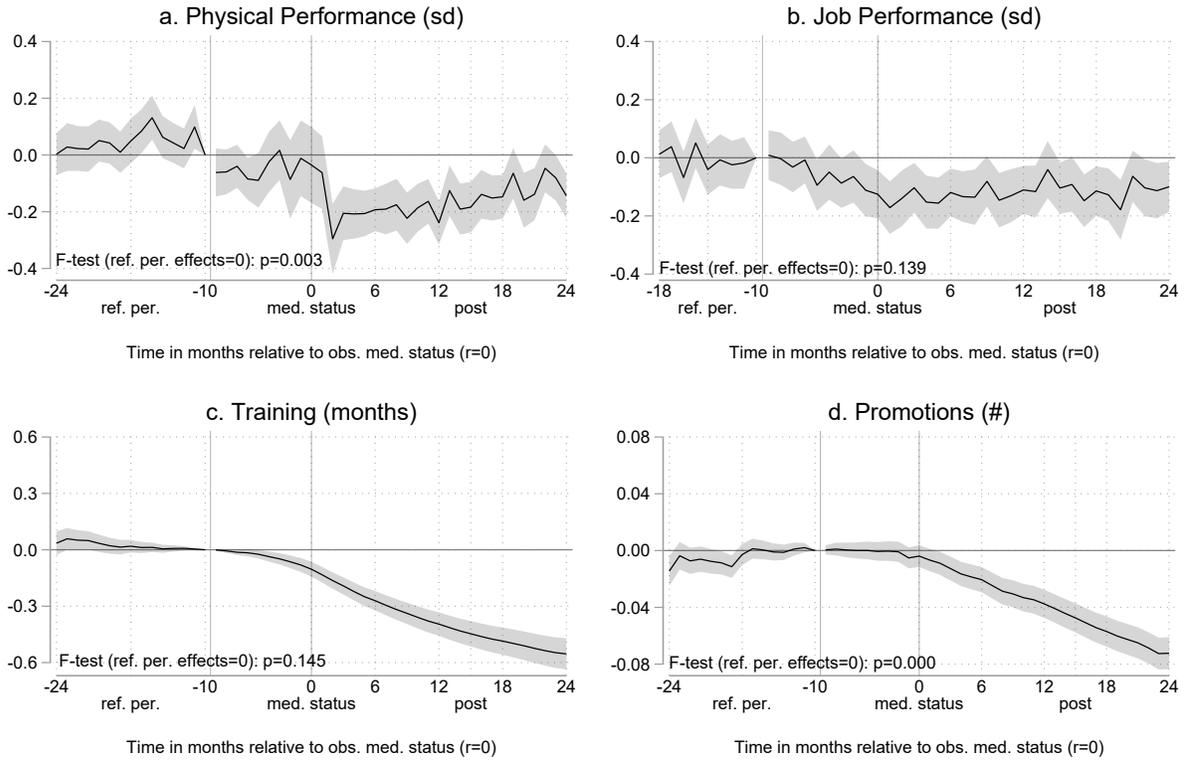


(b) Fathers



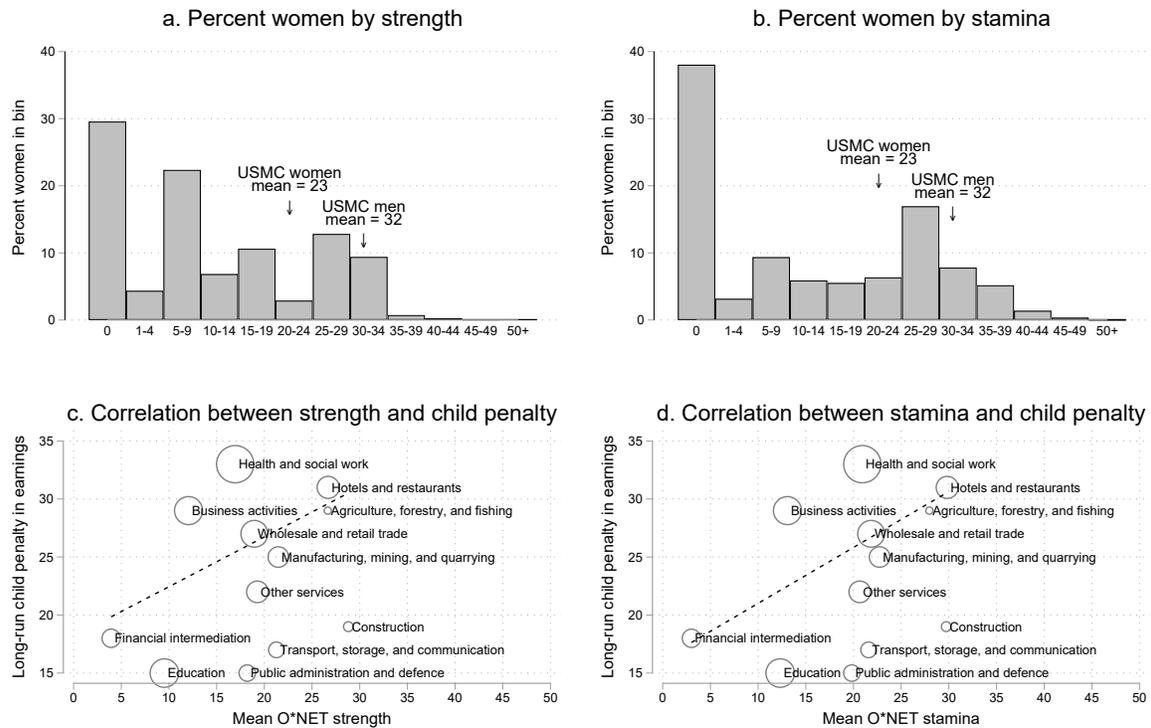
Notes: Figure displays gaps in the physical performance, months of training, years of education, and number of promotions relative to pre-pregnancy between first-time parents and placebo parents across birth events for the first postbirth observation (black line) and 24 month postbirth (light gray line) by subgroups. Each comparison (e.g., reserve vs. active) is based on one regression by interacting an indicator variable with a group indicator (e.g., reserve). Filled-in markers indicate a statistically significant difference between groups at the $p < 0.01$ level. Classifications for parents are as follows: “Reserves” are not on active duty and likely working a civilian job; “Active” work their military job full-time. “>physical” are those whose military job type above the median physicality level in our sample based on O*NET classification; “<physical” are at or below the median, among those whose jobs are classified by O*NET. “Married” are married at $r = 0$; “Single” are not. “Jr. Enl” are in enlisted grade E1–E4 at $r = -10$; “Senior” are E5 and up or officers. “Baby $r < 25$ ” have an additional baby within two years postbirth; “No 2nd baby” do not. “Stay $r > 36$ ” stay in the military at least three years after the birth event; “Stay $r \leq 36$ ” leave between $r = [24, 36]$. Vertical solid lines reflect a zero effect. Horizontal lines indicate 95% confidence intervals.

Figure 5: Event Study Estimates of the Impact of a Medical Event on Men’s Outcomes



Notes: Figure displays coefficients from event study regressions using assignment to a medical status at $r = 0$ as the event and using a placebo event matched sample. Sample only includes individuals we do not observe having a baby in our study window and who remain in the sample at least $r = [-12, 24]$. Outcomes include standardized scores from (a) physical/combat fitness tests, (b) job performance evaluations, (c) cumulative months of training (relative to $r = -10$), and (d) cumulative count of promotions (relative to $r = -10$). We require non-medical individuals be an exact match on rank, number of months in service, reserve status, and observation year as parents at $r = -10$. Among those, we match individuals with a medical event to a maximum of five most similar non-medical individuals in their propensity to have a medical event based on age, race/ethnicity, AFQT scores, marital status (and if their spouse is in the military), education level, months of training, occupational field, and most recent physical performance score as of $r = -10$. Regressions include match-group and month-year fixed effects. The reference month is $r = -10$. Vertical lines reflect the month after the match ($r = -9.5$) and the start of the observation of medical status ($r = 0$). We include a lag between the match and the start of the observed medical status in case there are delays in updating the medical status. Standard errors are clustered by individual and match-group and are included as shaded areas representing a 95% confidence interval.

Figure 6: Civilian Occupations, Strength and Stamina Requirements, and Child Penalties



Notes: Panels A and B display the distribution of the O*NET strength and stamina occupations weighted by the number of women in the occupation from the Department of Labor (DOL) Employment and Earnings by Occupation table in 2021. Includes a marker for the mean requirements for Marine occupations for men and women. Panels C and D link strength/stamina occupation requirements to long-run child penalty by industry estimates from (Fontenay et al., 2023). Categories are weighted by number of women in the occupation in 2021. We map Nomenclature statistique des activités économiques dans la Communauté européenne (NACE)-based categories used in Fontenay et al. (2023) to Standard Occupation Codes (SOC) used by O*NET and DOL as follows: Agriculture, Forestry, and Fishing is linked to SOC 45-0000 Farming, Fishing, and Forestry Occupations; Manufacturing, Mining, and Quarrying is linked to SOC 51-0000 Production Occupations, 47-5000 Extraction Worker, and 51-8000 Plant and System Operators; Construction is linked to SOC 47-2000 Construction Trades Workers, 47-3000 Helpers, Construction Trades, and 47-4000 Other Construction and Related Workers; Wholesale and Retail Trade is linked to SOC 41-0000 Sales and Related Occupations; Transport, Storage, and Communication is linked to SOC 53-0000 Transportation and Material Moving Occupations, 27-3000 Media and Communication Workers, 27-4000 Media and Communication Equipment Workers, 43-2000 Communications Equipment Operators, 49-2000 Electrical and Electronic Equipment Mechanics, Installers, and Repairers, and 15-1000 Computer Occupations; Financial intermediation is linked to SOC 13-2000 Financial Specialists and 43-3000 Financial Clerks; Education is linked to SOC 25-0000 Educational Instruction and Library Occupations, Health and Social Work is linked to 21-0000 Community and Social Service Occupations, 29-0000 Healthcare Practitioners and Technical Occupations, and 31-0000 Healthcare Support Occupations; and Other (which included Arts, Entertainment, and Recreation; Other Service Activities; Activities of Households as Employers and Activities of Extraterritorial Bodies) is linked to 27-0000 Arts, Design, Entertainment, Sports, and Media Occupations and 39-0000 Personal Care and Service Occupations. We cannot directly link SOC codes to the categories Hotels and Restaurants; Real Estate, Renting, and Business Activities; or Public Administrators and Defense used by Fontenay et al. (2023); these occupations are excluded.

Table 1: Descriptive Characteristics

| Variable | Women | | | Men | | |
|------------------------------------|--------------------|--------------------|--------------------|--------------------|--------------------|----------------------|
| | Mothers (1) | Placebos (2) | Difference (3) | Fathers (4) | Placebos (5) | Difference (6) |
| <i>A. Exact match variables</i> | | | | | | |
| Months of service | 39.209 [42.829] | 39.209 [42.822] | 0.000 (0.400) | 58.570 [49.579] | 58.570 [49.578] | 0.000 (0.300) |
| Officer | 0.073 [0.260] | 0.073 [0.260] | 0.000 (0.003) | 0.137 [0.343] | 0.137 [0.343] | 0.000 (0.003) |
| Reservist | 0.051 [0.221] | 0.051 [0.221] | 0.000 (0.002) | 0.112 [0.316] | 0.112 [0.315] | 0.000 (0.002) |
| <i>B. Other matching variables</i> | | | | | | |
| Age | 22.565 [4.172] | 22.700 [4.345] | -0.134* (0.059) | 24.640 [4.623] | 24.907 [4.989] | -0.267*** (0.037) |
| Black | 0.156 [0.363] | 0.150 [0.357] | 0.006 (0.009) | 0.097 [0.295] | 0.081 [0.273] | 0.016*** (0.002) |
| Hispanic | 0.224 [0.417] | 0.223 [0.416] | 0.001 (0.010) | 0.143 [0.350] | 0.127 [0.333] | 0.017*** (0.003) |
| Other | 0.097 [0.296] | 0.091 [0.288] | 0.006 (0.008) | 0.073 [0.260] | 0.081 [0.272] | -0.008** (0.003) |
| Cognitive test (Z-score) | -0.168 [0.938] | -0.181 [0.927] | 0.013 (0.019) | 0.022 [0.997] | 0.130 [0.991] | -0.108*** (0.010) |
| Married | 0.413 [0.493] | 0.393 [0.488] | 0.020** (0.007) | 0.672 [0.470] | 0.668 [0.471] | 0.004 (0.004) |
| Military spouse | 0.264 [0.441] | 0.251 [0.434] | 0.013 (0.008) | 0.040 [0.195] | 0.037 [0.188] | 0.003 (0.002) |
| Years of education | 12.481 [1.328] | 12.487 [1.324] | -0.006 (0.022) | 12.743 [1.557] | 12.761 [1.576] | -0.018 (0.013) |
| Recent fitness score | 0.068 [0.902] | 0.095 [0.871] | -0.027 (0.020) | 0.246 [0.840] | 0.195 [0.878] | 0.051*** (0.008) |
| Combat job type | 0.048 [0.214] | 0.045 [0.206] | 0.004 (0.005) | 0.288 [0.453] | 0.290 [0.454] | -0.002 (0.004) |
| Combat support job type | 0.626 [0.484] | 0.628 [0.483] | -0.002 (0.011) | 0.367 [0.482] | 0.352 [0.478] | 0.016** (0.005) |
| Aviation job type | 0.192 [0.394] | 0.199 [0.399] | -0.007 (0.009) | 0.242 [0.428] | 0.250 [0.433] | -0.008 (0.005) |
| Avg. analytic weight | 1.000 | 0.211 | | 1.000 | 0.202 | |
| Observations | 2492 | 12262 | 14754 | 24066 | 120047 | 144113 |
| Unique individuals | 2492 | 6444 | 2492 | 24066 | 30660 | 24066 |

Notes: Table displays means (SD in brackets) for parents (Columns 1 and 4) and their respective placebos (columns 2 and 5), and the difference in means (standard error clustered by person and match group in parentheses) between them (Columns 3 and 6) at the time of the match ($r = 10$), weighted by the analytic weight. Required exact match on months of service, rank (e.g., corporal or captain), reservist, and year, with further matching based on predicted propensity score from the remaining variables and their interactions. Includes the average analytic weight, number of unique person-month matches, and number of unique individuals. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 2: Impacts of Childbirth among First-Time Parents

| | Physical Performance (sd) (1) | Job Performance (sd) (2) | Marks- manship (sd) (3) | Training (months) (4) | Education (years) (5) | Promotions (count) (6) |
|--------------------------------|-------------------------------------|--------------------------------|----------------------------------|-----------------------------|-----------------------------|------------------------------|
| A. Mothers | | | | | | |
| 1-month effect | – | -0.174*** [0.000] | – | -0.410*** [0.000] | -0.014 [0.088] | -0.032** [0.003] |
| 8-month effect | -0.495*** [0.000] | -0.178*** [0.000] | -0.118** [0.005] | -0.602*** [0.000] | -0.022* [0.018] | -0.058*** [0.000] |
| 12-month effect | -0.289*** [0.000] | -0.181*** [0.000] | -0.132** [0.006] | -0.712*** [0.000] | -0.027* [0.012] | -0.074*** [0.000] |
| 24-month effect | -0.184*** [0.000] | -0.071 [0.107] | -0.065 [0.254] | -0.831*** [0.000] | -0.028* [0.040] | -0.088*** [0.000] |
| DV mean (nonparents $r = 24$) | 0.14 | -0.12 | 0.21 | 1.61 | 12.55 | 1.31 |
| Unique individuals | 8,936 | 6,489 | 8,220 | 8,936 | 8,936 | 8,936 |
| Observations | 129,495 | 79,949 | 55,866 | 1,155,300 | 1,152,532 | 1,155,300 |
| R ² | 0.26 | 0.27 | 0.19 | 0.38 | 0.76 | 0.79 |
| B. Fathers | | | | | | |
| 1-month effect | -0.124*** [0.000] | -0.038* [0.019] | 0.025 [0.229] | -0.057* [0.035] | 0.007* [0.031] | 0.001 [0.877] |
| 12-month effect | -0.046*** [0.000] | -0.008 [0.586] | 0.052** [0.004] | -0.034 [0.343] | 0.008 [0.075] | -0.003 [0.618] |
| 24-month effect | -0.008 [0.468] | 0.030 [0.076] | 0.009 [0.666] | 0.036 [0.427] | 0.014** [0.007] | 0.012 [0.085] |
| DV mean (nonparents $r = 24$) | 0.02 | -0.08 | 0.25 | 2.01 | 12.84 | 0.96 |
| Unique individuals | 54,726 | 47,188 | 45,453 | 54,726 | 54,726 | 54,726 |
| Observations | 1,865,115 | 874,845 | 669,631 | 12,659,523 | 12,611,748 | 12,659,523 |
| R ² | 0.25 | 0.29 | 0.18 | 0.40 | 0.81 | 0.76 |

Notes: Table displays predicted values from Eq. (2), the semi-parametric specification. Outcomes include (1) standardized (mean=0, SD=1) scores from physical/combat fitness tests conducted 2x per year, (2) standardized scores (mean=0, SD=1) from supervisor-rated job performance evaluations conducted 1-2x per year, (3) standardized scores (mean=0, SD=1) from rifle or pistol tests conducted 1 or fewer times per year, (4) cumulative months of training, (5) cumulative degree counts relative to $r = -10$, and (6) cumulative promotion counts relative to $r = -10$. We exclude women's physical performance scores 9 months before through 7 months after birth because women are not required to take fitness tests during and after pregnancy. We exclude women's marksmanship scores 9 months before through 4 months after birth because women are not required to take marksmanship exams during pregnancy or while on leave. All outcomes for women and men exclude $r = 0$. Regressions include match-group and month-by-year fixed effects. Predicted p -value of whether the value statistically differs from zero are shown in brackets, based on heteroscedasticity-robust F -test and standard errors clustered by match-group and individual. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

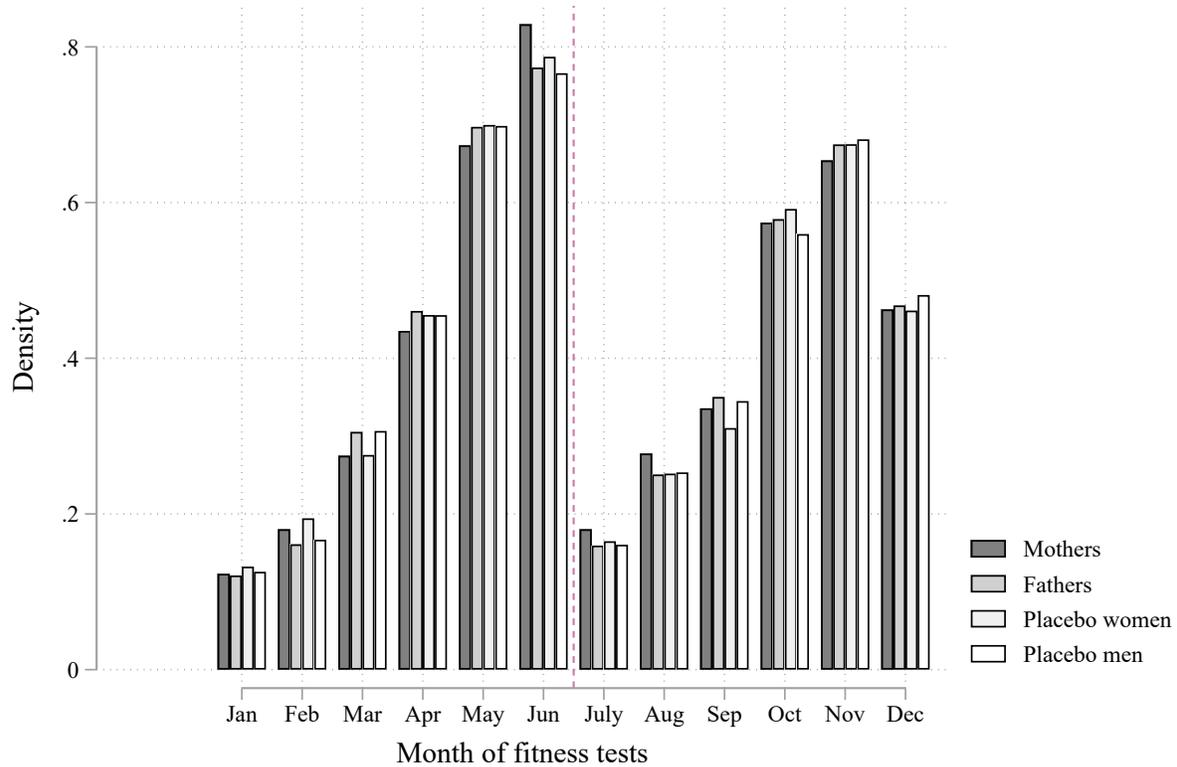
Table 3: Women’s Outcomes by Leave Length

| | Post-birth drop | | 12 months post | | 24 months post | | N |
|-------------------------------|-----------------|----------|----------------|----------|----------------|----------|---------|
| | Effect size | <i>p</i> | Effect size | <i>p</i> | Effect size | <i>p</i> | |
| A. Physical performance (sd) | | | | | | | |
| Main effect: | -0.499*** | 0.000 | -0.293*** | 0.000 | -0.138*** | 0.000 | 94,796 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.450*** | 0.000 | -0.295*** | 0.000 | -0.141** | 0.002 | 94,796 |
| 6 weeks + 12 flex | -0.795*** | 0.000 | -0.286* | 0.011 | -0.382** | 0.008 | |
| 18 weeks | -0.653*** | 0.000 | -0.283*** | 0.000 | -0.042 | 0.612 | |
| <i>p</i> (diff), all effects | 0.054 | | 0.986 | | 0.119 | | |
| B. Training (months) | | | | | | | |
| Main effect: | -0.331*** | 0.000 | -0.637*** | 0.000 | -0.771*** | 0.000 | 858,694 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.326*** | 0.000 | -0.623*** | 0.000 | -0.709*** | 0.000 | 858,694 |
| 6 weeks + 12 flex | -0.405 | 0.050 | -0.474 | 0.051 | -0.529 | 0.087 | |
| 18 weeks | -0.302* | 0.017 | -0.747*** | 0.000 | -1.181*** | 0.000 | |
| <i>p</i> (diff), all effects | 0.909 | | 0.607 | | 0.055 | | |
| C. Years of education | | | | | | | |
| Main effect: | -0.018 | 0.073 | -0.032* | 0.013 | -0.028 | 0.082 | 856,742 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.009 | 0.455 | -0.017 | 0.229 | -0.008 | 0.656 | 856,742 |
| 6 weeks + 12 flex | -0.060* | 0.034 | -0.139** | 0.008 | -0.138** | 0.006 | |
| 18 weeks | -0.042 | 0.056 | -0.053 | 0.054 | -0.076* | 0.016 | |
| <i>p</i> (diff), all effects | 0.158 | | 0.058 | | 0.017 | | |
| D. Promotions (#) | | | | | | | |
| Main effect: | -0.037** | 0.003 | -0.084*** | 0.000 | -0.090*** | 0.000 | 858,694 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.042** | 0.003 | -0.079*** | 0.000 | -0.083*** | 0.000 | 858,694 |
| 6 weeks + 12 flex | -0.047 | 0.301 | -0.171*** | 0.001 | -0.151** | 0.005 | |
| 18 weeks | -0.009 | 0.735 | -0.071* | 0.014 | -0.095** | 0.007 | |
| <i>p</i> (diff), all effects | 0.540 | | 0.165 | | 0.468 | | |

Notes: Regressions only include births before March 2016 because mothers could not plan for additional leave announced in July 2015 at conception. Outcomes include physical performance, months of training, years of education, and count of promotion. Postbirth drop is measured at 8 months postbirth for physical performance and at 1 month postbirth for all other outcomes. Regressions include match-group and month-by-year fixed effects. The first row replicates the main analysis for the smaller sample. The next rows display a separate regression from the policy interaction model. “6 weeks” is the predicted mother-placebo gap under the 6-week policy (for babies born December 2014 and prior). “6 weeks + 12 flex” is the predicted mother-placebo gap for mothers who gave birth under the 6-week policy but were retroactively given an additional 12 weeks of leave to use before their baby’s first birthday after they had returned to work (for babies born January 2015–mid-May 2015). “18 weeks” is the predicted mother-placebo gap for mothers who learned they would have 18 weeks of leave while pregnant or on leave (for babies born mid-May 2015–February 2016). The final row presents the *p*-value for an *F*-test of whether mother-placebo gaps are the same across all policy periods. ****p* < 0.001, ***p* < 0.01, **p* < 0.05.

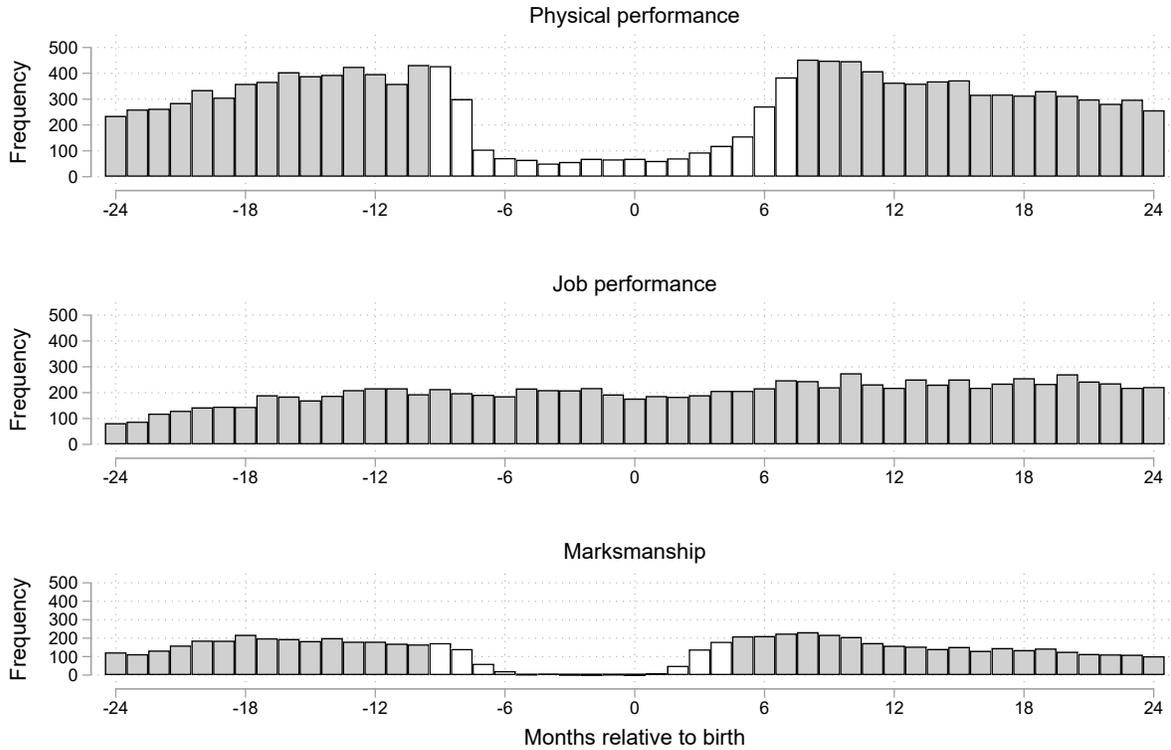
A Supplemental Tables and Figures for Online Publication

Figure A.1: Distribution of Physical Fitness Assessment Timing



Notes: Figure displays the distribution of mothers, fathers, placebo women, and placebo men by month of observed fitness assessments in $r = [-24, 24]$. The Physical Fitness Test (PFT) test cycle occurs in January–June and the Combat Fitness Test (PFT) test cycle occurs in July–December. Most Marines take the test in the later months of the cycle. Mothers in a unit that scheduled the test in May could run it in June either because they were still under the post-pregnancy fitness waiver in May or because their supervisors allowed them to take it later, even if the mother was off the waiver in May.

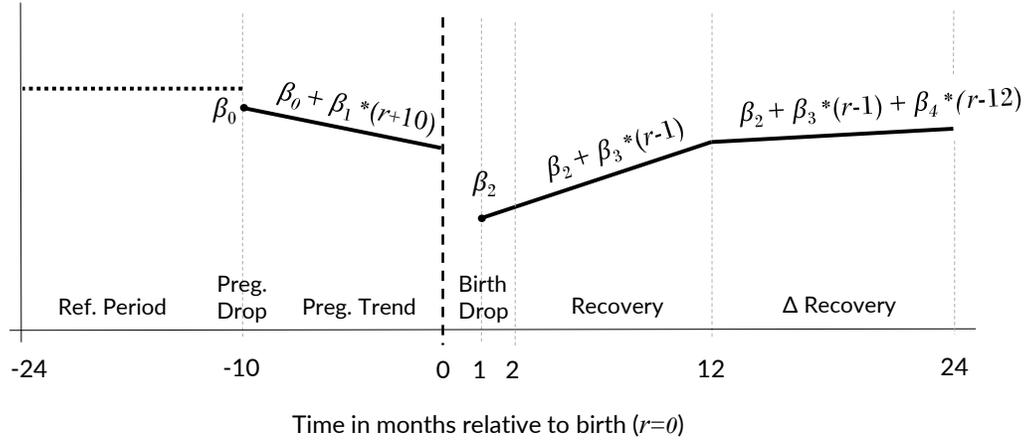
Figure A.2: Distribution of Mothers' Outcomes Relative to Birth



Notes: Figure displays the count of physical performance, job performance, and marksmanship scores for mothers by month relative to birth. Physical performance tests are the most common test among these outcomes; all ranks are expected to take them twice a year (the Physical Fitness Test in January–June and the Combat Fitness Test in July–December). The main analysis excludes physical performance scores at $r = [-9, 7]$ (because mothers did not have to take the tests in pregnancy through 6 months postpartum, and commanders may give them some leeway in month 7) and marksmanship scores at $r = [-9, 4]$ (because mothers did not have to take the tests during pregnancy or while on leave). Semi-dynamic specifications always exclude $r = 0$ due to ambiguity about outcome timing relative to birth. Excluded outcomes are in white. The count of physical performance observations declines 12 to 24 months postbirth because some mothers have additional children and become exempt from testing again.

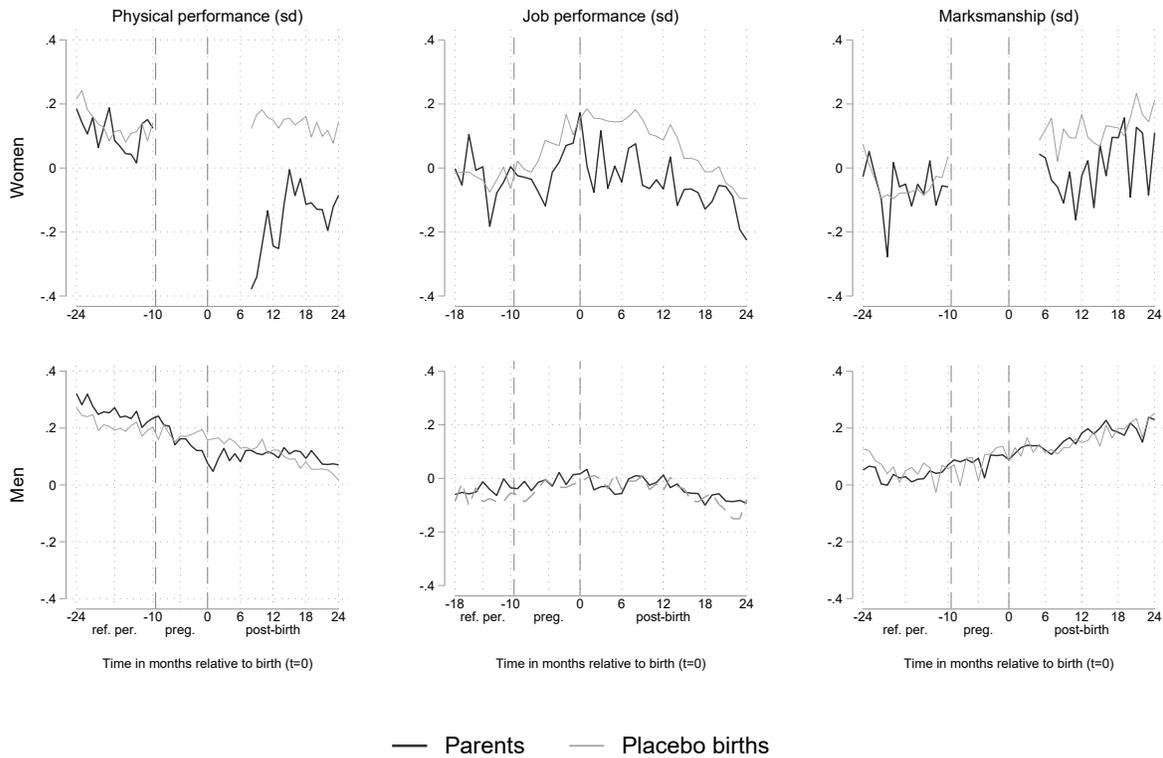
Figure A.3: Stylized Diagram of the Semi-Dynamic Specification, Eq. (2)

$$Y_{igtr} = \theta_0 \text{PregnancyDrop}_{igtr} + \theta_1 \text{PregnancyTrend}_{igtr} + \theta_2 \text{BirthDrop}_{igtr} + \theta_3 \text{Recovery}_{igtr} + \theta_4 \Delta \text{Recovery}_{igtr} + \pi P_i + \beta_0 (\text{PregnancyDrop}_{igtr} \times P_i) + \beta_1 (\text{PregnancyTrend}_{igtr} \times P_i) + \beta_2 (\text{BirthDrop}_{igtr} \times P_i) + \beta_3 (\text{Recovery}_{igtr} \times P_i) + \beta_4 (\Delta \text{Recovery}_{igtr} \times P_i) + \alpha_g + \phi_t + \varepsilon_{igtr}$$



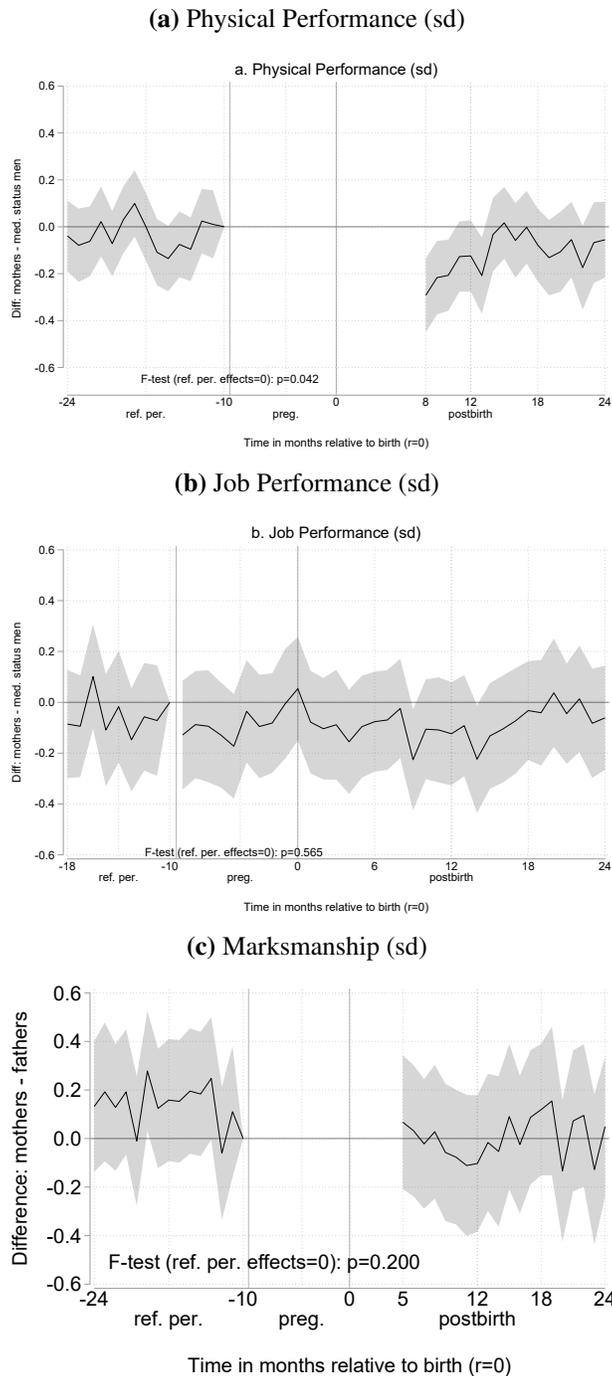
Notes: Figure provides a stylized representation of results from Eq.(2) with spline parameters defined as follows: $\text{Pregnancy}_{igtr} = 1$ during months relative to birth $r = [-9, -1]$ and 0 otherwise (for an intercept shift during pregnancy); $\text{PregnancyTrend}_{igtr} = [0, 8]$ corresponding to $r = [-9, -1]$ and 0 otherwise (for monthly trends beyond the intercept shift during pregnancy); $\text{Postbirth}_{igtr} = 1$ during months relative to birth $r = [q, 25]$, where $q = 1$ or the earliest time point when the outcome is able to be assessed again after birth (e.g., starting 8 months after birth for mothers' fitness tests), and 0 otherwise (for an intercept shift following birth); $\text{Recovery}_{igtr} = [q + 1, 25]$ corresponding to $r = [q + 1, 25]$ and 0 otherwise (for monthly trends beyond the intercept shift after birth); and $\Delta \text{Recovery}_{igtr} = [1, 13]$ corresponding to $r = [13, 25]$ and 0 otherwise (for any change to the monthly recovery rate that begins at 13 months). Note, the earliest time point at which we define q start at $q = 1$, the month after birth, due to ambiguity about whether outcomes measured in the month of birth itself $r = 0$ reflect pre- or postbirth measures.

Figure A.4: Average Job Outcomes among Parents and Matched Placebos across the Transition to Parenthood



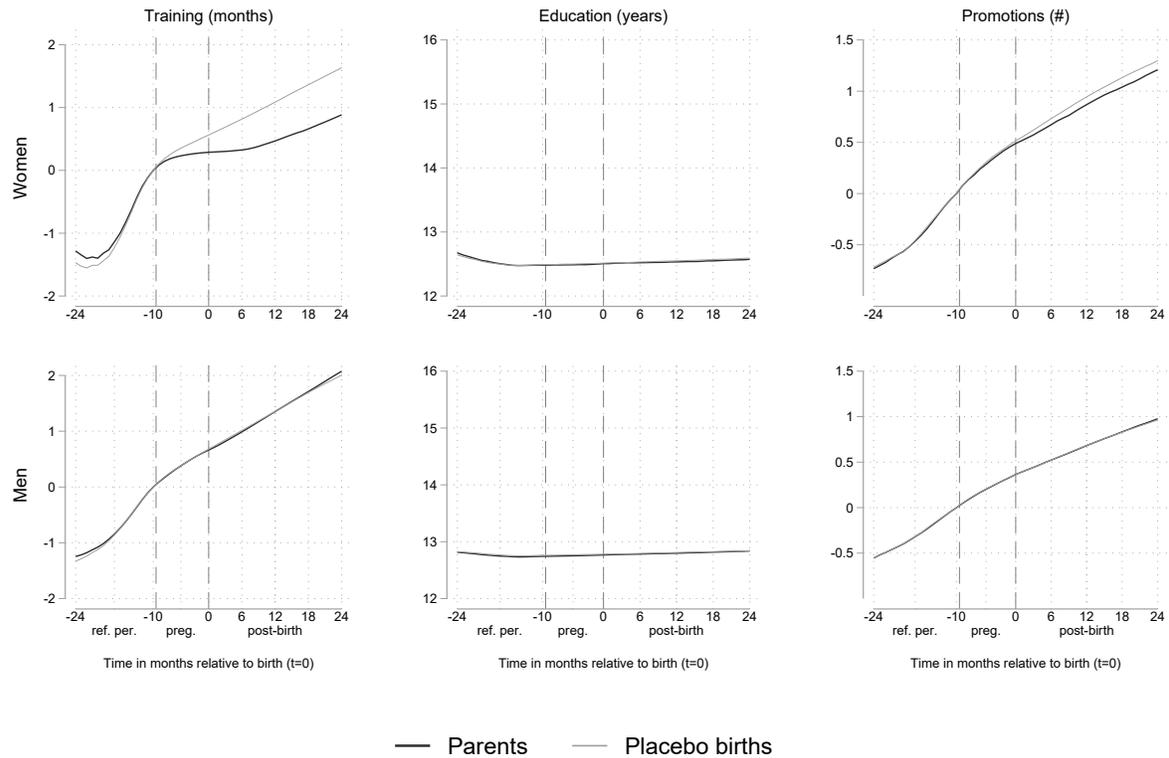
Notes: Figure displays weighted mean standardized scores from physical/combat fitness tests, job performance evaluations, and rifle/pistol marksmanship evaluations between first-time parents (solid line) and placebo parents (dashed line) over time. Nonparents assigned to placebo births are limited to those whose rank, number of months in service, reserve status, and year is an exact match with parents' 10 months before birth. Among those with an exact match, each parent's outcomes are compared to the five nonparents most similar to parents in their propensity to have a child based on age, race/ethnicity, military entrance exam scores (AFQT scores), marital status (including whether a spouse is also in the military), level of education, months of training, occupational field, and most recent physical performance scores.

Figure A.5: Job Outcomes of Mothers Compared to Fathers: Triple Difference Event Study Estimates



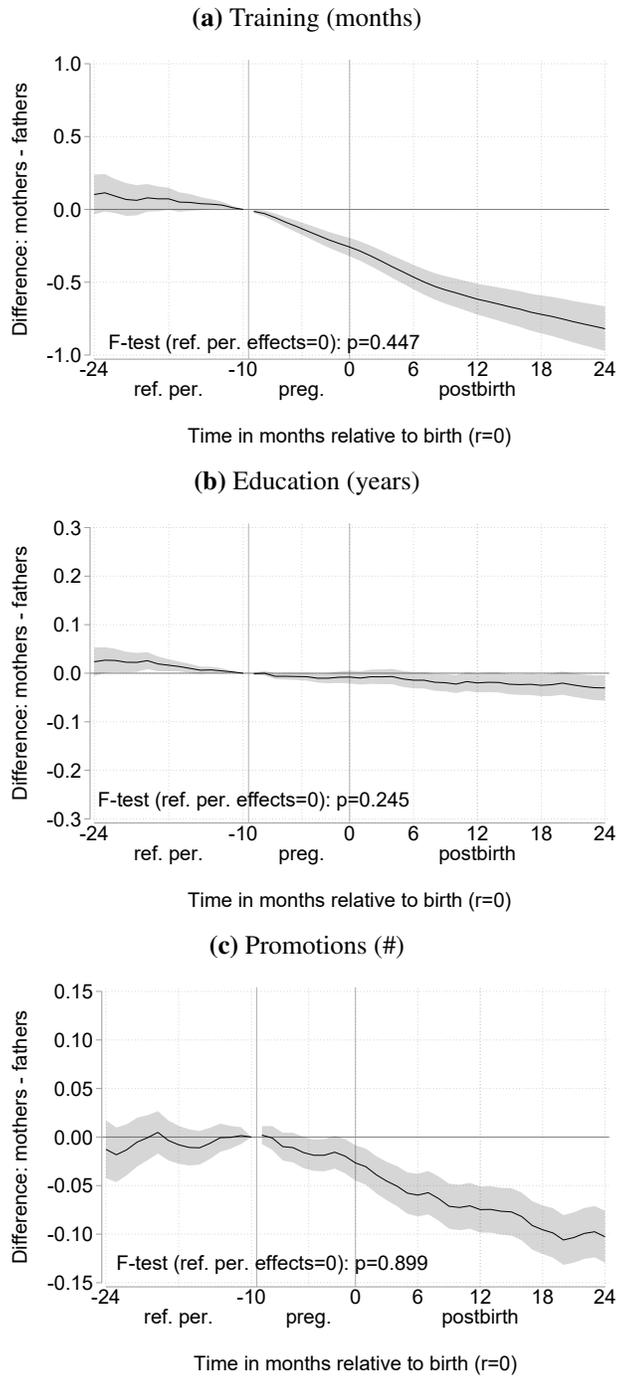
Notes: Figure displays coefficients from event study regressions using a triple difference framework that compares changes in outcomes before/after birth, between parents and their placebo matched sample, and between mothers and fathers. Outcomes include physical/combat fitness tests, job performance evaluations, and rifle/pistol marksmanship evaluations. Regressions include match-group and month-year fixed effects. The reference month is $r = -10$. Vertical lines reflect the start of the pregnancy ($r = -9.5$) and birth ($r = 0$). Standard errors are clustered by individual and match-group and are included as shaded areas representing a 95% confidence interval.

Figure A.6: Average Human Capital and Promotion Outcomes among Parents and Matched Placebos across the Transition to Parenthood



Notes: Figure displays weighted mean levels of cumulative months of training, cumulative count of years of education (relative to $r = -10$), and cumulative count of promotions (relative to $r = -10$) between first-time parents (solid line) and matched nonparents with a placebo birth (dashed line) over time. Nonparents assigned to placebo births are limited to those whose rank, number of months in service, reserve status, and year is an exact match with parents' 10 months before birth. Among those with an exact match, each parent's outcomes are compared to the five nonparents most similar to parents in their propensity to have a child based on age, race/ethnicity, military entrance exam scores (AFQT scores), marital status (including whether a spouse is also in the military), level of education, months of training, occupational field, and most recent physical performance scores.

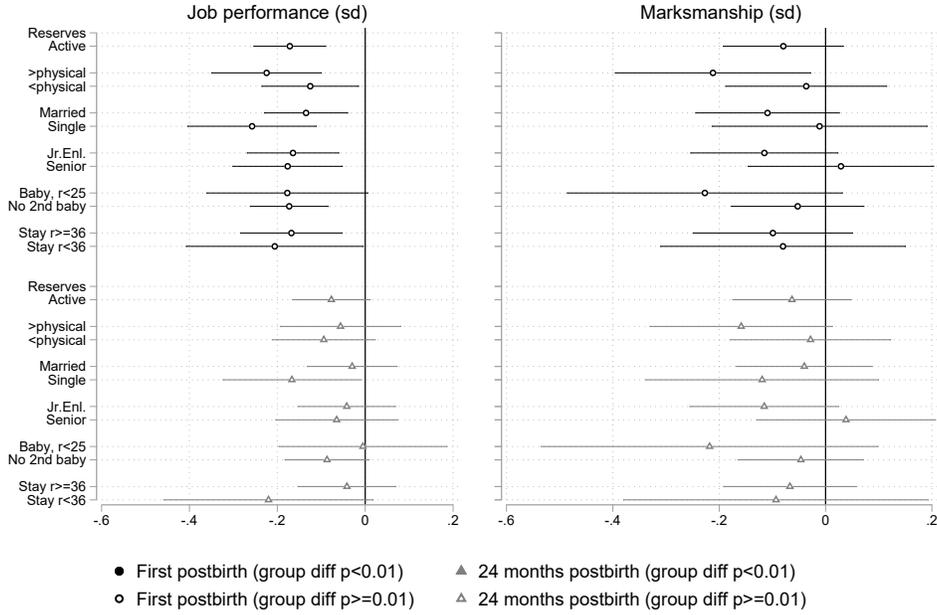
Figure A.7: Human Capital and Promotion Outcomes of Mothers Compared to Fathers: Triple Difference Event Study Estimates



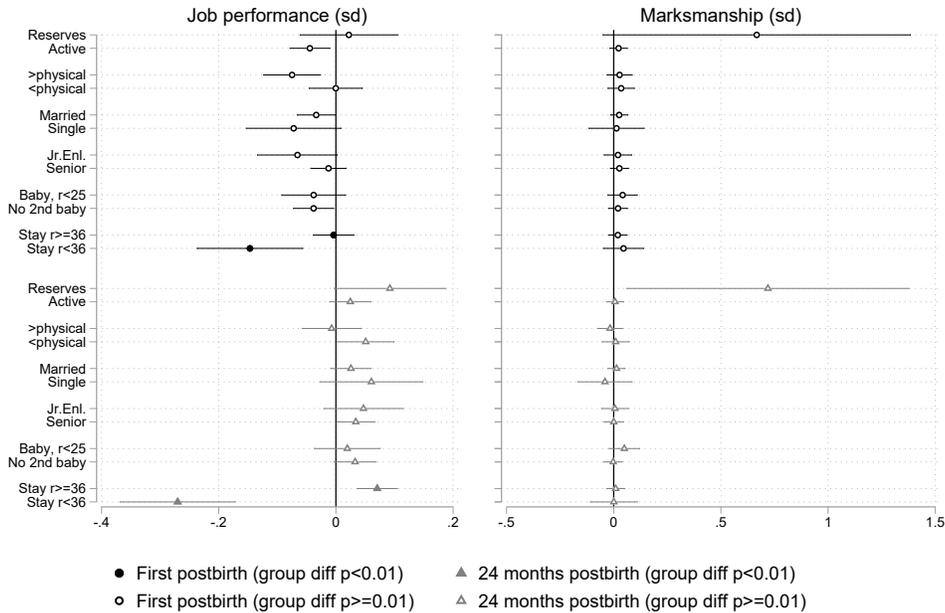
Notes: Figure displays coefficients from event study regressions using a triple difference framework that compares changes in outcomes before/after birth, between parents and their placebo matched sample, and between mothers and fathers. Outcomes include cumulative months of training, a cumulative count of years of education (relative to $r = -10$), and a cumulative count of promotions (relative to $r = -10$). Regressions include match-group and month-year fixed effects. The reference month is $r = -10$. Vertical lines reflect the start of the pregnancy ($r = -9.5$) and birth ($r = 0$). Standard errors are clustered by individual and match-group and are included as shaded areas representing a 95% confidence interval.

Figure A.8: Event Study Estimates of Subgroup Heterogeneity in Job Performance & Marksmanship

(a) Mothers

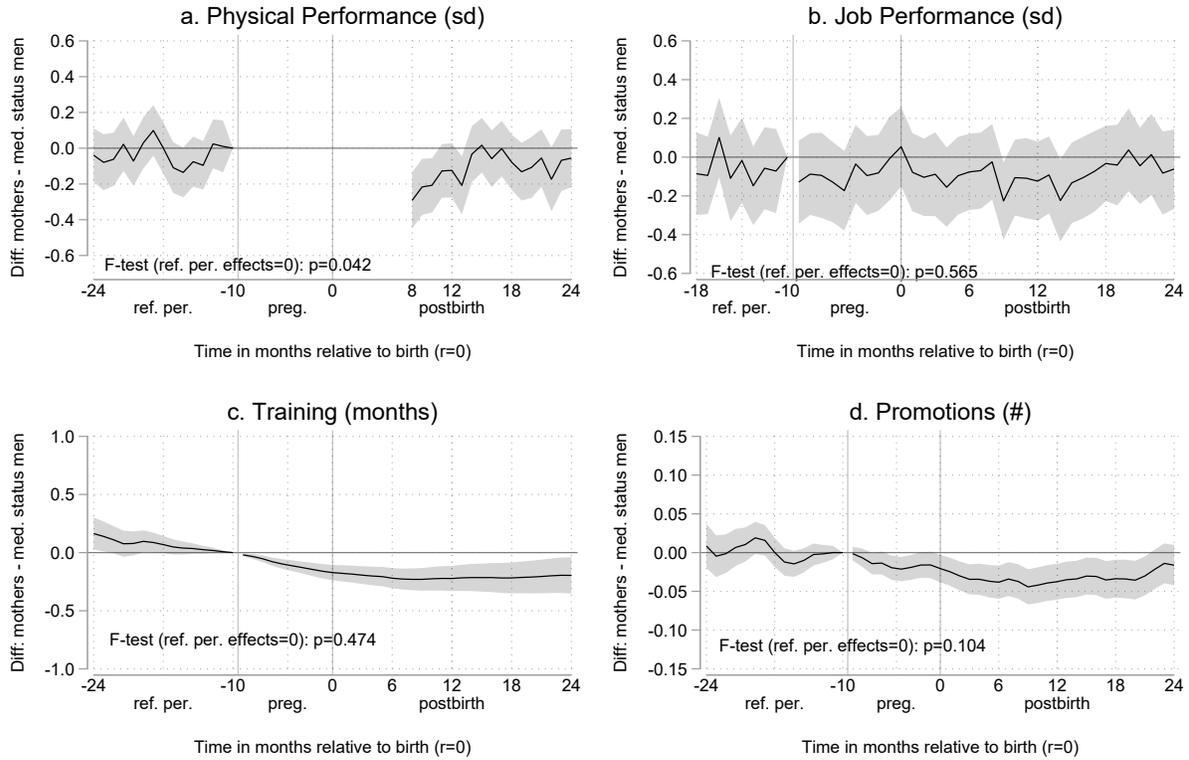


(b) Fathers



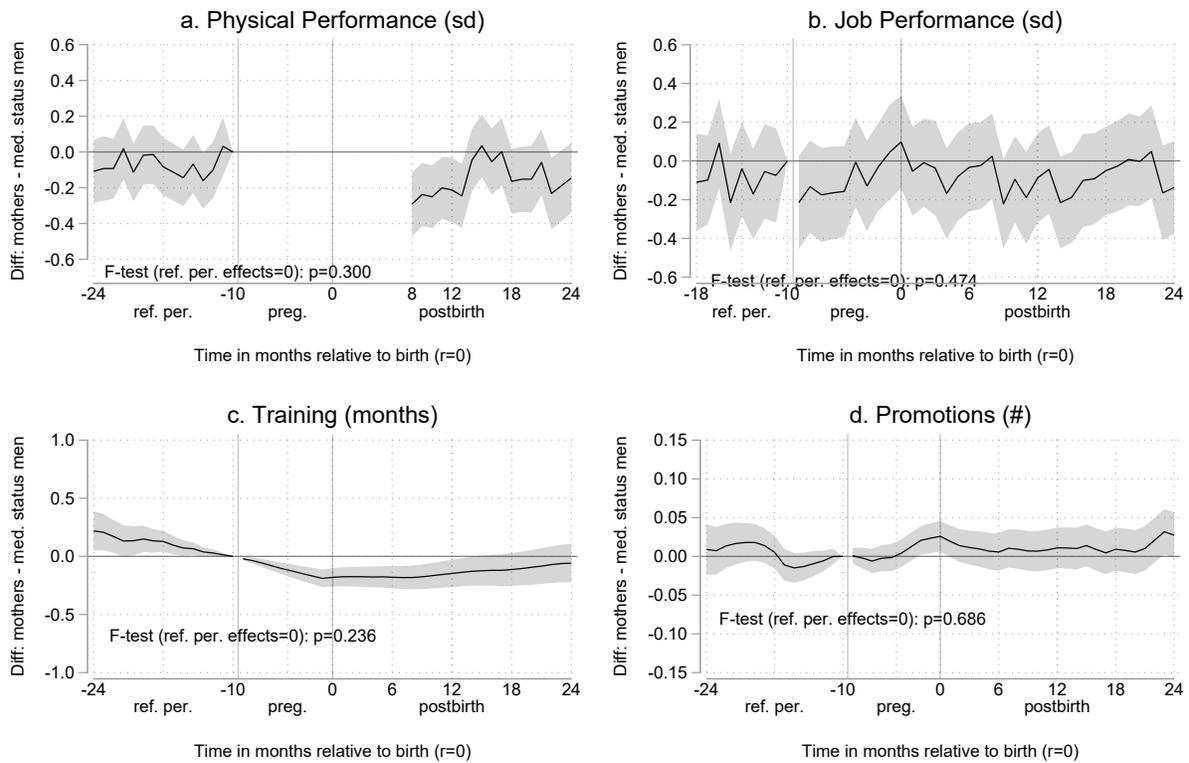
Notes: Figure displays gaps in the job performance and marksmanship scores relative to prepregnancy between first-time parents and placebo parents across birth events for the first postbirth observation (black line) and 24 month postbirth (light gray line) by subgroups. Each comparison (e.g., reserve vs. active) is based on one regression by interacting an indicator variable with a group indicator (e.g., reserve) with the parameters in Eq. (2). Filled-in markers indicate a statistical significant difference between groups at the $p < 0.01$ level. Classifications for parents are as follows: “Reserves” are not on active duty and likely working a civilian job; “Active” work their military job full-time. “>physical” are those whose military job type above the median physicality level in our sample based on O*NET classification; “<physical” are at or below the median, among those whose jobs are classified by O*NET. “Married” are married at $r = 0$; “Single” are not. “Jr. Enl” are in enlisted grade E1–E4 at $r = -10$; “Senior” are E5 and up or officers. Vertical solid lines reflect a zero effect. “Baby $r < 25$ ” have an additional baby within two years postbirth; “No 2nd baby” do not. “Stay $r \geq 36$ ” stay in the military at least three years after the birth event; “Stay $r < 36$ ” leave between $r = [24, 36]$. Vertical solid lines indicate a zero effect. Horizontal lines indicate 95% confidence intervals.

Figure A.9: Differences in Outcomes Between Mothers after a Birth and Men after a Medical Event: Triple Difference Event Study Estimates



Notes: Figure displays coefficients from event study regressions using a triple difference framework that compares changes in outcomes between mothers and men who are not fathers, before/after a first birth (mothers) or medical event (men), net of differences between each treated group (mothers and medical-event men) and their matched comparisons. We capture events at $r = 0$, using first births for women and assignment to a medical status for men as the events at $r = 0$. Sample only includes individuals who remain in the sample at least $r = [-12, 24]$. Outcomes include standardized scores from (a) physical/combat fitness tests, (b) job performance evaluations, (c) cumulative months of training (relative to $r = -10$), and (d) cumulative count of promotions (relative to $r = -10$). Regressions include match-group and month-year fixed effects. The reference month is $r = -10$. Vertical lines reflect $r = -9.5$, the start of pregnancy for mothers, and event time ($r = 0$) for both mothers and men. Standard errors are clustered by individual and match-group and are included as shaded areas representing a 95% confidence interval. See notes for Figures 2 and 5 for details on the matched comparison groups for mothers and medical-event men, respectively.

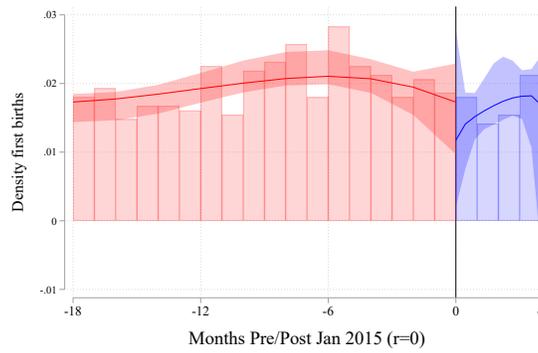
Figure A.10: Differences in Outcomes Between Mothers after a Birth and Men after a Medical Event Requiring Maximum Work Restrictions: Triple Difference Event Study Estimates



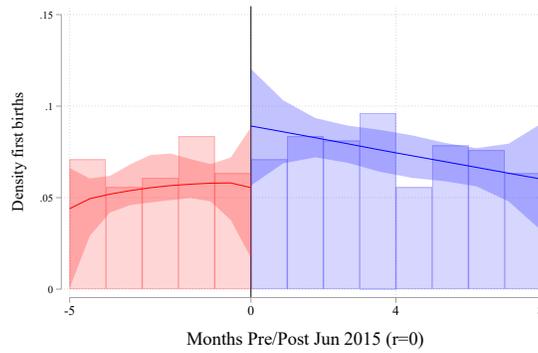
Notes: Figure displays coefficients from event study regressions using a triple difference framework that compares mothers and men who are not fathers, before/after a first birth (mothers) or medical event requiring maximum work restrictions (men), net of differences between each treated group (mothers and medical-event men) and their matched comparisons. Estimates are similar to those presented in Figure A.9 but include only a subset of men with medical events where, at onset, the event was classified as the most serious of possible administrative designations (medically restricted limited duty), requiring up to six months of medically restricted duty. As in Figure A.9, we capture events at $r = 0$, using first births for women and assignment to a medical status for men as the events at $r = 0$. Sample only includes individuals who remain in the sample at least $r = [-12, 24]$. Outcomes include standardized scores from (a) physical/combat fitness tests, (b) job performance evaluations, (c) cumulative months of training (relative to $r = -10$), and (d) cumulative count of promotions (relative to $r = -10$). Regressions include match-group and month-year fixed effects. The reference month is $r = -10$. Vertical lines reflect $r = -9.5$, the start of pregnancy for mothers, and event time ($r = 0$) for both mothers and men. Standard errors are clustered by individual and match-group and are included as shaded areas representing a 95% confidence interval. See notes for Figures 2 and 5 for details on the matched comparison groups for mothers and medical-event men, respectively.

Figure A.11: Density of First Births across Policy Periods

(a) 6 weeks vs. 6 + 12 flex weeks



(b) 6 + 12 flex weeks vs. 18 weeks



Notes: Histogram bars display the density of first births by month before and after $r=0$, which differentiates births subject to one leave-length policy period from another. Plotted curves and corresponding 95% confidence intervals come from a manipulation test using a local-polynomial density estimator developed by Cattaneo et al. (2018). The test for a discontinuity at $r=0$ is not statistically significant in Panel (a) or (b). The sample includes all women in the Marines with a first birth during the time window.

Table A.1: Characteristics of First-Time Parents

| | Mothers | | Fathers | |
|-----------------------------------|---------|-----------|---------|-----------|
| | Marines | Civilian | Marines | Civilian |
| Descriptive characteristics | | | | |
| Age | 23.40 | 29.97 | 25.48 | 31.80 |
| Black | 0.15 | 0.09 | 0.10 | 0.07 |
| Hispanic | 0.23 | 0.12 | 0.14 | 0.14 |
| Married | 0.68 | 0.81 | 0.87 | 0.86 |
| Some college | 0.05 | 0.28 | 0.05 | 0.27 |
| College | 0.09 | 0.59 | 0.16 | 0.48 |
| Job Classifications | | | | |
| Mngmt./Business/Science/Arts | 0.13 | 0.57 | 0.10 | 0.46 |
| Service | 0.07 | 0.15 | 0.04 | 0.11 |
| Sales/Office | 0.35 | 0.24 | 0.12 | 0.15 |
| Construction/Maint. | 0.18 | 0.00 | 0.29 | 0.15 |
| Production/Moving/Transpo. | 0.19 | 0.03 | 0.14 | 0.14 |
| Military | 0.08 | 0.00 | 0.31 | 0.00 |
| Military-Specific Characteristics | | | | |
| Officer | 0.07 | – | 0.14 | – |
| AFQT score (percentile) | 58.56 | – | 63.23 | – |
| GCT score (av=100; sd=20) | 103.38 | – | 111.29 | – |
| N of individuals | 2,492 | 3,638,695 | 24,059 | 4,557,719 |

Notes: Table displays characteristics of first-time parents in the Marine Corps in our sample alongside characteristics of first-time civilian parents in the labor market. Time-varying characteristics of Marines in our sample (e.g., age) are measured at the month of birth ($t=0$). Data on civilians come from the American Community Survey 1-year estimates, 2010 to 2018. We limit the civilian sample to adults who are employed in the civilian labor market and have a first child under age 1. Job categories correspond to Standard Occupational Classification (SOC) system groups applied to U.S. Marine Corps job codes and available in the American Community Survey. Military specific variables include whether a Marine is ranked as an officer (akin to manager) and AFQT and GCT scores, which are measures of intelligence. We do not observe these military-specific variables in the civilian sample.

Table A.2: Coefficients for Impacts of Childbirth among First-Time Parents

| | Physical Performance (sd) (1) | Job Perfor- mance (sd) (2) | Marks- manship (sd) (3) | Training (months) (4) | Education (years) (5) | Promotions (count) (6) |
|--|--|-------------------------------------|----------------------------------|-----------------------------|-----------------------------|------------------------------|
| A. Mothers | | | | | | |
| Pregnancy drop | – | -0.067 (0.053) | – | -0.061* (0.029) | -0.015** (0.005) | 0.003 (0.008) |
| Pregnancy trend | – | -0.004 (0.008) | – | -0.028*** (0.003) | -0.001 (0.001) | -0.002* (0.001) |
| Birth drop (<i>birth</i> – 24 <i>mos.</i>) | -0.495*** (0.038) | -0.174*** (0.041) | -0.106 (0.072) | -0.410*** (0.044) | -0.014 (0.008) | -0.032** (0.010) |
| Recovery (<i>birth</i> – 24 <i>mos.</i>) | 0.051*** (0.012) | -0.001 (0.004) | -0.004 (0.013) | -0.027*** (0.003) | -0.001 (0.001) | -0.004*** (0.001) |
| Δ recovery (13 – 24 <i>mos.</i>) | -0.043** (0.014) | 0.010 (0.007) | 0.009 (0.018) | 0.018*** (0.003) | 0.001 (0.001) | 0.003 (0.002) |
| DV mean (nonparents $r = 24$) | 0.14 | -0.12 | 0.21 | 1.61 | 12.55 | 1.31 |
| Unique individuals | 8,936 | 6,489 | 8,220 | 8,936 | 8,936 | 8,936 |
| Observations | 129,495 | 79,949 | 55,866 | 1,155,300 | 1,152,532 | 1,155,300 |
| R ² | 0.26 | 0.27 | 0.19 | 0.38 | 0.76 | 0.79 |
| B. Fathers | | | | | | |
| Pregnancy drop | 0.008 (0.013) | 0.021 (0.019) | 0.052 (0.031) | -0.035* (0.015) | -0.001 (0.002) | -0.004 (0.004) |
| Pregnancy trend | -0.014*** (0.002) | -0.004 (0.003) | -0.006 (0.005) | 0.001 (0.002) | 0.000 (0.000) | 0.000 (0.001) |
| Birth drop (<i>birth</i> – 24 <i>mos.</i>) | -0.124*** (0.009) | -0.038* (0.016) | 0.025 (0.021) | -0.057* (0.027) | 0.007* (0.003) | 0.001 (0.005) |
| Recovery (<i>birth</i> – 24 <i>mos.</i>) | 0.007*** (0.001) | 0.003 (0.002) | 0.002 (0.002) | 0.002 (0.002) | 0.000 (0.000) | -0.000 (0.000) |
| Δ recovery (13 – 24 <i>mos.</i>) | -0.004* (0.002) | 0.000 (0.003) | -0.006 (0.004) | 0.004* (0.002) | 0.001 (0.000) | 0.002* (0.001) |
| DV mean (nonparents $r = 24$) | 0.02 | -0.08 | 0.25 | 2.01 | 12.84 | 0.96 |
| Unique individuals | 54,726 | 47,188 | 45,453 | 54,726 | 54,726 | 54,726 |
| Observations | 1,865,115 | 874,845 | 669,631 | 12,659,523 | 12,611,748 | 12,659,523 |
| R ² | 0.25 | 0.29 | 0.18 | 0.40 | 0.81 | 0.76 |

Notes: Table displays coefficients from Eq. 2, the semi-parametric specification. Outcomes include (1) standardized (mean=0, SD=1) scores from physical/combat fitness tests conducted 2x per year, (2) standardized scores (mean=0, SD=1) from supervisor-rated job performance evaluations conducted 1-2x per year, (3) standardized scores (mean=0, SD=1) from rifle or pistol tests conducted 1 or fewer times per year, (4) cumulative months of training, (5) cumulative degree counts relative to $r = -10$, and (6) cumulative promotion counts relative to $r = -10$. We exclude women's physical performance scores 9 months before through 7 months after birth because women are not required to take fitness tests during and after pregnancy. We exclude women's marksmanship scores 9 months before through 4 months after birth because women are not required to take marksmanship exams during pregnancy or while on leave. All outcomes for women and men exclude $r = 0$. Regressions include match-group and month-by-year fixed effects. The parameter "Pregnancy drop" captures any immediate shift from prebirth to pregnancy, if observed. The parameter "Pregnancy trend" captures trends during pregnancy, if observed. "postbirth drop" is an indicator equal to 1 after birth, starting in $r = 1$ for all men's outcomes; $r = 8$ for women's physical performance; $r = 5$ for women's marksmanship; and $r = 1$ for women's job performance, training, education, and promotion. "Recovery trend" estimates monthly changes in the outcome for the entire postbirth period. " Δ Recovery trend" estimates any change in the slope in the second year postbirth. Robust standard errors are clustered by match group and individual, shown in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table A.3: Impact of Childbirth on Physical Performance Test Components

| | 3-Mile Run (seconds) (1) | Crunches (count) (2) | Pull-Ups (count) (3) | 880-Yard-Run (seconds) (4) | Lifts (count) (5) | Shuttle Run (seconds) (6) |
|--------------------------------|--------------------------------|----------------------------|----------------------------|----------------------------------|-------------------------|---------------------------------|
| A. Mothers | | | | | | |
| 8-month effect | 55.026*** [0.000] | -5.027*** [0.000] | -1.243*** [0.000] | 10.289*** [0.000] | -3.172*** [0.000] | 10.833*** [0.000] |
| 12-month effect | 24.220*** [0.000] | -4.015*** [0.000] | -0.691** [0.004] | 7.129*** [0.000] | -2.626*** [0.000] | 7.815*** [0.000] |
| 24-month effect | 19.276** [0.008] | -2.505*** [0.000] | -0.693** [0.005] | 2.081 [0.074] | -1.540* [0.012] | 1.395 [0.302] |
| DV mean (nonparents $r = 24$) | 1561.69 | 98.75 | 7.10 | 214.67 | 69.52 | 186.16 |
| Unique individuals | 8,918 | 8,925 | 4,651 | 8,870 | 8,871 | 8,870 |
| Observations | 69,950 | 70,280 | 20,796 | 68,542 | 68,232 | 68,535 |
| R ² | 0.23 | 0.23 | 0.31 | 0.19 | 0.30 | 0.20 |
| B. Fathers | | | | | | |
| 1-month effect | 23.613*** [0.000] | 0.072 [0.605] | -0.305*** [0.000] | 2.178*** [0.000] | -0.323* [0.036] | 2.021*** [0.000] |
| 12-month effect | 0.610 [0.728] | -0.080 [0.500] | -0.203*** [0.000] | 0.697** [0.003] | 0.027 [0.846] | 0.883** [0.001] |
| 24-month effect | -1.535 [0.490] | 0.081 [0.634] | -0.067 [0.326] | -0.306 [0.313] | 0.297 [0.107] | 0.001 [0.997] |
| DV mean (nonparents $r = 24$) | 1395.10 | 102.83 | 17.16 | 180.13 | 102.60 | 147.87 |
| Unique individuals | 54,707 | 54,716 | 54,715 | 54,688 | 54,693 | 54,688 |
| Observations | 975,367 | 979,614 | 966,401 | 953,194 | 952,541 | 953,148 |
| R ² | 0.24 | 0.35 | 0.23 | 0.18 | 0.45 | 0.20 |

Notes: Table displays coefficients from the semi-parametric specification in Eq. (2) for item-level outcomes by fitness test type. Columns 1–3 show performance on the Physical Fitness Test items, assessed January–June. Limited pull-up outcome data exist for women prior to 2017, during which time they could do push-ups instead. Columns 3–6 show performance on the Combat Fitness Test items, assessed July–December. The 880-yard-run (Column 4) captures scores on the Movement to Contact drill, designed to mimic the stresses of running under pressure in battle. Lifts (Column 5) measure the number of times a Marine can lift a 30-pound ammunition can overhead. Shuttle run (Column 6) displays timed performance on a 300-yard shuttle run obstacle, called the Maneuver Under Fire drill, which includes crawls, ammunition resupply, grenade throwing, agility running, and the dragging and carrying of another Marine. Robust standard errors clustered by ID in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table A.4: Impacts of Childbirth among First-Time Parents, Controls for Linear Prepregnancy Trends

| | Physical Performance (sd) (1) | Job Performance (sd) (2) | Marks- manship (sd) (3) | Training (months) (4) | Education (years) (5) | Promotions (count) (6) |
|--------------------------------|-------------------------------------|--------------------------------|----------------------------------|-----------------------------|-----------------------------|------------------------------|
| A. Mothers | | | | | | |
| 1-month effect | – | -0.137 [0.308] | – | -0.172** [0.006] | 0.026 [0.084] | -0.033* [0.040] |
| 8-month effect | -0.556*** [0.000] | -0.124 [0.519] | -0.096 [0.470] | -0.266** [0.005] | 0.035 [0.112] | -0.060** [0.005] |
| 12-month effect | -0.361*** [0.000] | -0.116 [0.610] | -0.107 [0.487] | -0.320** [0.005] | 0.040 [0.129] | -0.075** [0.003] |
| 24-month effect | -0.286** [0.008] | 0.024 [0.942] | -0.029 [0.893] | -0.271 [0.120] | 0.067 [0.084] | -0.089* [0.015] |
| DV mean (nonparents $r = 24$) | 0.14 | -0.12 | 0.21 | 1.61 | 12.55 | 1.31 |
| Unique individuals | 8,935 | 6,489 | 8,209 | 8,936 | 8,936 | 8,936 |
| Observations | 128,863 | 79,886 | 55,593 | 1,150,781 | 1,148,037 | 1,150,781 |
| R ² | 0.26 | 0.27 | 0.19 | 0.38 | 0.76 | 0.79 |
| B. Fathers | | | | | | |
| 1-month effect | -0.105*** [0.000] | -0.116* [0.019] | -0.056 [0.206] | 0.070* [0.033] | 0.011* [0.037] | 0.013 [0.178] |
| 12-month effect | -0.016 [0.581] | -0.144 [0.087] | -0.078 [0.239] | 0.175** [0.003] | 0.014 [0.140] | 0.019 [0.204] |
| 24-month effect | 0.035 [0.391] | -0.168 [0.169] | -0.176 [0.056] | 0.334*** [0.000] | 0.023 [0.094] | 0.045* [0.035] |
| DV mean (nonparents $r = 24$) | 0.02 | -0.08 | 0.25 | 2.01 | 12.83 | 0.96 |
| Unique individuals | 54,722 | 47,184 | 45,399 | 54,726 | 54,726 | 54,726 |
| Observations | 1,847,452 | 873,331 | 662,204 | 12,533,762 | 12,486,768 | 12,533,762 |
| R ² | 0.25 | 0.29 | 0.18 | 0.40 | 0.81 | 0.77 |

Notes: Table displays predicted values from a version of Eq. (2), the semi-parametric specification, that includes a linear slope parameter to control for any prepregnancy trends. All parameters in this model are measured relative to $r = -10$, 10 months before pregnancy, rather than relative to the entire prepregnancy period. See Table 2 for additional notes. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table A.5: Placebo Estimate: Impacts of Placebo Childbirth among Nonparent Women and Other Matched Nonparent Women

| | Physical Performance (sd) (1) | Job Perfor- mance (sd) (2) | Marks- manship (sd) (3) | Training (months) (4) | Education (years) (5) | Promotions (count) (6) |
|--------------------------------|--|-------------------------------------|----------------------------------|-----------------------------|-----------------------------|------------------------------|
| 1-month effect | – | -0.012 [0.810] | – | -0.116 [0.117] | -0.005 [0.679] | 0.003 [0.824] |
| 8-month effect | 0.039 [0.315] | -0.008 [0.852] | -0.042 [0.442] | -0.088 [0.305] | -0.006 [0.680] | -0.006 [0.661] |
| 12-month effect | -0.037 [0.216] | -0.005 [0.918] | 0.020 [0.732] | -0.072 [0.457] | -0.006 [0.702] | -0.010 [0.487] |
| 24-month effect | -0.022 [0.491] | -0.017 [0.746] | -0.053 [0.436] | -0.049 [0.689] | -0.013 [0.486] | 0.010 [0.557] |
| DV mean (nonparents $r = 24$) | 0.05 | 0.01 | 0.20 | 1.88 | 12.68 | 1.39 |
| Unique individuals | 6,349 | 4,756 | 5,668 | 6,349 | 6,349 | 6,349 |
| Observations | 78,065 | 50,374 | 31,467 | 690,780 | 693,097 | 690,780 |
| R ² | 0.25 | 0.26 | 0.19 | 0.40 | 0.73 | 0.78 |

Notes: Placebo test created by removing all mothers from the women’s sample, identifying a rank-weighted sample of placebos, matching the placebos to other nonmother women using the LASSO and exact-match process, and then running the main analysis. See Table 2 for additional notes. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table A.6: Predicting Promotion Count by Measurable Skills among Placebo Mothers, Placebo Fathers, and Actual Fathers

| | Women | | | | | | Men | Fathers |
|--------------------|---------------------|---------------------|---------------------|-------------------|------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Avg. fitness | 0.148*** (0.011) | | | | | 0.132*** (0.011) | 0.118*** (0.007) | 0.119*** (0.007) |
| Avg. job perf. | | 0.046*** (0.012) | | | | 0.034** (0.012) | 0.012 (0.007) | 0.009 (0.006) |
| Avg. marksmanship | | | 0.042*** (0.010) | | | 0.025** (0.009) | 0.010 (0.006) | 0.006 (0.006) |
| Months of training | | | | -0.003 (0.002) | | -0.005** (0.002) | -0.000 (0.001) | -0.001 (0.001) |
| Years of education | | | | | 0.012 (0.013) | 0.001 (0.013) | 0.034*** (0.007) | 0.032*** (0.008) |
| Observations | 12225 | 12225 | 12225 | 12225 | 12225 | 12225 | 119582 | 23903 |
| R^2 | 0.562 | 0.560 | 0.553 | 0.551 | 0.551 | 0.571 | 0.471 | 0.463 |

Notes: Models predict count of promotions accumulated in $r = [-10, 24]$ for placebo mothers (Columns 1–6), placebo fathers (Column 7), and actual fathers (Column 8). All models include controls for the baseline characteristics included in the prediction model at $r = -10$, as well as months of service by officer fixed effects to account for structural patterns of promotion by time of service and officer status. Average fitness, job performance, and marksmanship are the average of observed scores in $r = [-9, 23]$. Months of training are the months accumulated in $r = [-9, 23]$. Years of education is the observed years at $r = 23$. Only includes individuals with observed data for all listed skills (e.g., individuals with no observed fitness scores in $r = [-9, 23]$ are dropped). Robust standard errors are clustered by match group and individual, shown in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table A.7: Descriptive Characteristics for the Main Policy Analysis

| | Length of Paid Maternity Leave | | | |
|--------------------------|--------------------------------|-----------------|--------|---------|
| | 6 wks | 6 wks + 12 flex | 18 wks | p(diff) |
| Months of service | 38.36 | 40.17 | 37.91 | 72.41 |
| Officer | 0.06 | 0.07 | 0.11 | 0.07 |
| Reservist | 0.06 | 0.07 | 0.04 | 0.06 |
| Age | 22.40 | 22.77 | 22.65 | 25.30 |
| Black | 0.16 | 0.12 | 0.13 | 0.15 |
| Hispanic | 0.19 | 0.18 | 0.24 | 0.21 |
| Other | 0.10 | 0.12 | 0.11 | 0.11 |
| Cognitive test (Z-score) | -0.16 | -0.07 | -0.18 | -0.15 |
| Married | 0.40 | 0.40 | 0.48 | 0.75 |
| Military spouse | 0.26 | 0.26 | 0.30 | 0.44 |
| Years of Education | 12.40 | 12.60 | 12.65 | 12.55 |
| Recent fitness score | 0.07 | 0.00 | 0.01 | -0.12 |
| Combat job type | 0.05 | 0.02 | 0.08 | 0.05 |
| Combat support job type | 0.63 | 0.63 | 0.59 | 0.62 |
| Aviation job type | 0.19 | 0.21 | 0.22 | 0.19 |
| Observations | 1423 | 121 | 274 | 1818 |

Notes: Table displays means for mothers by policy period (columns 1–3) and the p -value of an ANOVA test of whether the values differ across groups (column 4 “p(diff)”). Excludes mothers whose first birth occurred March 2016 or later who could have known about extended leave length at the time of conception. Variables are those used in the matching procedure.

Table A.8: Descriptive Characteristics for the Supplementary Policy Analysis

| | Length of Paid Maternity Leave | | | | p(diff) |
|--------------------------|--------------------------------|-----------------|--------|--------|---------|
| | 6 wks | 6 wks + 12 flex | 18 wks | 12 wks | |
| Months of service | 38.36 | 40.17 | 40.97 | 39.43 | 0.69 |
| Officer | 0.06 | 0.07 | 0.10 | 0.07 | 0.03 |
| Reservist | 0.06 | 0.07 | 0.04 | 0.03 | 0.04 |
| Age | 22.40 | 22.77 | 23.06 | 22.42 | 0.02 |
| Black | 0.16 | 0.12 | 0.13 | 0.17 | 0.18 |
| Hispanic | 0.19 | 0.18 | 0.26 | 0.31 | 0.00 |
| Other | 0.10 | 0.12 | 0.10 | 0.07 | 0.18 |
| Cognitive test (Z-score) | -0.16 | -0.07 | -0.15 | -0.25 | 0.18 |
| Married | 0.40 | 0.40 | 0.48 | 0.38 | 0.01 |
| Military spouse | 0.26 | 0.26 | 0.30 | 0.24 | 0.17 |
| Years of Education | 12.40 | 12.60 | 12.67 | 12.48 | 0.00 |
| Recent fitness score | 0.07 | 0.00 | 0.07 | 0.07 | 0.88 |
| Combat job type | 0.05 | 0.02 | 0.07 | 0.03 | 0.05 |
| Combat support job type | 0.63 | 0.63 | 0.59 | 0.66 | 0.24 |
| Aviation job type | 0.19 | 0.21 | 0.21 | 0.18 | 0.58 |
| Observations | 1423 | 121 | 497 | 393 | 2434 |

Notes: Table displays means for mothers by policy period (columns 1–4) and the p -value of an ANOVA test of whether the values differ across groups (column 5 “p(diff)”). Excludes mothers whose first birth was in November–December 2016 due to ambiguity about the policy for such mothers. Variables are those used in the matching procedure.

Table A.9: Supplementary Analysis: Women’s Outcomes by All Maternity Leave Lengths

| | Post-birth drop | | 12 months post | | 24 months post | | N |
|-------------------------------------|-----------------|----------|----------------|----------|----------------|----------|-----------|
| | Effect size | <i>p</i> | Effect size | <i>p</i> | Effect size | <i>p</i> | |
| <i>A. Physical performance (sd)</i> | | | | | | | |
| Main effect: | -0.495*** | 0.000 | -0.286*** | 0.000 | -0.183*** | 0.000 | 126,513 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.453*** | 0.000 | -0.296*** | 0.000 | -0.139** | 0.002 | 126,513 |
| 6 weeks + 12 flex | -0.827*** | 0.000 | -0.276* | 0.014 | -0.373* | 0.010 | |
| 18 weeks | -0.582*** | 0.000 | -0.278*** | 0.000 | -0.159** | 0.009 | |
| 12 weeks | -0.419*** | 0.000 | -0.267*** | 0.000 | -0.318*** | 0.000 | |
| <i>p</i> (diff), all effects | 0.081 | | 0.979 | | 0.127 | | |
| <i>B. Training (months)</i> | | | | | | | |
| Main effect: | -0.401*** | 0.000 | -0.695*** | 0.000 | -0.814*** | 0.000 | 1,130,047 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.328*** | 0.000 | -0.624*** | 0.000 | -0.713*** | 0.000 | 1,130,047 |
| 6 weeks + 12 flex | -0.406* | 0.049 | -0.483* | 0.046 | -0.549 | 0.076 | |
| 18 weeks | -0.420*** | 0.000 | -0.795*** | 0.000 | -1.073*** | 0.000 | |
| 12 weeks | -0.634*** | 0.000 | -0.887*** | 0.000 | -0.928*** | 0.000 | |
| <i>p</i> (diff), all effects | 0.119 | | 0.244 | | 0.164 | | |
| <i>C. Years of education</i> | | | | | | | |
| Main effect: | -0.014 | 0.104 | -0.026* | 0.016 | -0.026 | 0.052 | 1,127,373 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.009 | 0.454 | -0.017 | 0.228 | -0.008 | 0.669 | 1,127,373 |
| 6 weeks + 12 flex | -0.060* | 0.032 | -0.139** | 0.008 | -0.136** | 0.006 | |
| 18 weeks | -0.007 | 0.707 | -0.016 | 0.479 | -0.045 | 0.088 | |
| 12 weeks | -0.021 | 0.179 | -0.029 | 0.163 | -0.032 | 0.285 | |
| <i>p</i> (diff), all effects | 0.348 | | 0.153 | | 0.095 | | |
| <i>D. Promotions (#)</i> | | | | | | | |
| Main effect: | -0.030** | 0.005 | -0.074*** | 0.000 | -0.089*** | 0.000 | 1,130,047 |
| Effects by paid leave length: | | | | | | | |
| 6 weeks | -0.043** | 0.003 | -0.078*** | 0.000 | -0.084*** | 0.000 | 1,130,047 |
| 6 weeks + 12 flex | -0.049 | 0.284 | -0.168*** | 0.001 | -0.158** | 0.003 | |
| 18 weeks | -0.020 | 0.345 | -0.082*** | 0.001 | -0.090*** | 0.001 | |
| 12 weeks | 0.006 | 0.805 | -0.021 | 0.444 | -0.087** | 0.004 | |
| <i>p</i> (diff), all effects | 0.315 | | 0.058 | | 0.618 | | |

Notes: Regressions exclude birth after March 2016 births given that these women would have known about extended leave before becoming pregnant. Outcomes include physical performance, months of training, years of education, and count of promotion. Postbirth drop is measured at 8 months postbirth for physical performance and at 1 month postbirth for all other outcomes. Regressions include match-group and month-by-year fixed effects. The first row replicates the main analysis for the smaller sample. The next rows display a separate regression from the policy interaction model. “6 weeks” is the predicted mother-placebo gap under the 6-week policy (for babies born December 2014 and prior). “6 weeks + 12 flex” is the predicted mother-placebo gap for mothers who gave birth under the 6-week policy but were retroactively given an additional 12 weeks of leave to use before their baby’s first birthday after they had returned to work (for babies born January 2015–mid-May 2015). “18 weeks” values show the predicted mother-placebo difference for mothers who gave birth when 18 weeks of leave was in place but who did not know of this change at the time of conception. For this policy 12 weeks of the leave could be used flexibly before the baby’s first birthday (for babies born mid-May 2015–February 2016). “12 weeks” is the predicted mother-placebo gap for mothers who gave birth knowing they would have 12 weeks of leave to use immediately following birth (for babies born January 2017 and later). The final row presents the *p*-value for an *F*-test of whether mother-placebo differences are the same across all policy periods. *** *p* < 0.001, ** *p* < 0.01, * *p* < 0.05.

B. Data Appendix for Online Publication

Our data come from U.S. Department of Defense administrative records. Below, we review several alternative specifications to our preferred model, as well as further details about our mapping from O*NET to NACE.

B.1 Alternative Specifications

Our preferred empirical strategy prioritizes identifying an appropriate set of comparison cases to model counterfactual trends that first-time parents would have experienced absent a birth. Tables B.1 and B.2 explore several alternative models for mothers and fathers, respectively. The table columns are our main outcomes of interest, while each row in a given segment shows results for alternative specifications.

The first row of each table segment shows a standard two-way fixed effect (TWFE) event study. The comparison group is all same-gender Marines who do not have a baby and remain in the Marine Corps at least three years. This model will involve 2×2 comparisons where the counterfactual draws on already treated units, which can lead to what we term TWFE bias. In our setting, we are less worried about TWFE bias because we have a large number of “never-treated” comparison individuals, meaning that most of our overall estimate of counterfactual time trends will come from comparisons of untreated-to-treated against never-treated controls. The main concern with this model is that parents are not the same as average nonparents and thus nonparents do not provide a helpful counterfactual. We cannot use the TWFE approach to estimate training or promotion impacts because we measure these outcomes cumulatively, and cumulative measures require a pregnancy starting point that the comparison group (not assigned a placebo birth) does not have. We specifically run the following model:

$$Y_{it} = \alpha_i + \phi_t + \beta_0 \text{PregnancyDrop}_{it} + \beta_1 \text{PregnancyTrend}_{it} + \beta_2 \text{BirthDrop}_{it} + \beta_3 \text{Recovery}_{it} + \beta_4 \Delta \text{Recovery}_{it} + \varepsilon_{it} \quad (\text{B.1})$$

Here, β_0 captures any immediate intercept shift and β_1 captures the monthly linear change in the outcome during the pregnancy period ($t = [-9, -1]$), relative to the prepregnancy period average ($t \leq -10$). The regression excludes $r = 0$ due to ambiguity about the timing of the outcome relative to birth for all outcomes; it also excludes fitness and marksmanship scores for excluded months. Coefficient β_2 represents the acute postnatal birth drop (if any) in the outcome in the first month parents are again assessed after childbirth. Then, β_3 captures the monthly linear recovery in the outcome following that initial drop, and β_4 captures any change in the monthly linear recovery rate in the child's second year of life ($t = [13, 24]$). All parameters are measured relative to the prepregnancy average ($t \leq -10$).

The second row of each table segment shows results from a stacked TWFE model, which identifies cohorts of units treated at the same time, excludes any already-treated units from each cohort, stacks the cohorts, and then runs TWFE models across cohort-specific groups.⁴⁰ Each parent is connected to five nearest neighbor nonparents from the LASSO prediction model such that parents' births and nonparents' placebo births occur in the same month and year, which eliminates the negative weighting that can occur in traditional TWFE models (Callaway and Sant'Anna, 2021; Cengiz et al., 2019; Sun and Abraham, 2021). The LASSO model used to match parents and nonparents is the same as in the preferred analysis and includes a linear control for months of service, an indicator for officer, and an indicator for active/reserve status. However, we do not require an exact match on months of service, exact rank, or active/reserve status. Once we have the treated and comparison groups, we run the following model:

$$\begin{aligned}
Y_{igt} = & \alpha_g + \theta_0 \text{PregnancyDrop}_{igt}^{all} + \theta_1 \text{PregnancyTrend}_{igt}^{all} + \theta_2 \text{BirthDrop}_{igt}^{all} + \\
& \theta_3 \text{Recovery}_{igt}^{all} + \theta_4 \Delta \text{Recovery}_{igt}^{parents} + \beta_0 \text{PregnancyDrop}_{igt}^{parents} + \\
& \beta_1 \text{PregnancyTrend}_{igt}^{parents} + \beta_2 \text{BirthDrop}_{igt}^{parents} + \beta_3 \text{Recovery}_{igt}^{parents} + \\
& \beta_4 \Delta \text{Recovery}_{igt}^{parents} + \varepsilon_{igt}
\end{aligned} \tag{B.2}$$

⁴⁰In a stacked approach, regression estimates from each treatment-time cohort are combined using variance weighting to recover a single estimate of the impact across cohorts. Recently proposed alternative estimators, for example by Callaway and Sant'Anna (2021) and Sun and Abraham (2021), use other approaches to weighting each cohort-specific treatment estimate.

The key difference from Eq. 2 is that it excludes ϕ_t , the month-year fixed effect, because the time relative to birth and month-year is exactly the same within match groups. It also does not contain binned endpoints, because we only include $r = [-24, 24]$. Otherwise, the interpretation is the same as the main model. The potential problem for this model is that the treated and control group might differ on characteristics we know determine the outcome (i.e., months of service, rank, and active/reserve status).

The third row of each table segment shows results from an approach similar to our preferred model, but it only includes data from $r = [-24, 24]$ and removes the binned endpoints at $r = -25$ and $r = 25$. The binned endpoints were necessary for modeling both time relative to the match and month-year fixed effects. The model in Row (3) removes the month-year fixed effects, which requires the assumption that the relative time trends in Eq. (2) sufficiently capture counterfactual time trends. We use the same set of individuals as in the preferred specification. The model is identical to the Stacked TWFE model; the difference is that the stacked TWFE model exact matched on month-year while Row (3) exact matches on months of service, rank, active/reserve status, and year.

The fourth row of each table segment shows results from our preferred model provided for reference. It includes binned endpoints, month-year fixed effects, and exact matching on months of service, rank, active/reserve status, and calendar year. It is synonymous with Eq. (2).

B.2 Alternative Specification Results

The top panels of Tables B.1 and B.2 show a series of F -tests assessing the pretrends in $r = [-24, -10]$ by outcome for the different models. This is analagous to the F -test displayed in Figures 2 and 3. If the untreated group is a good counterfactual, we expect these estimates to be zero. There is some evidence of pretrends for both placebo estimates in education, with a p -value of 0.030 for the model with no time fixed effects and 0.025 for the preferred model with time fixed effects. Given the number of outcomes we examine, this could happen by chance, but for

this reason we take the women's education outcomes with a grain of salt. The other exact match pretrends are null for the mothers. The standard TWFE models is more precisely significant for the pretrends in education ($p = 0.000$), while the stacked TWFE model is significant for physical performance ($p = 0.009$), marksmanship ($p = 0.023$), and training ($p = 0.000$). The fathers show some evidence of pretrends in 25% of the standard TWFE outcomes, 67% of the stacked TWFE outcomes, and none of the two exact-matched placebo outcomes. This highlights the importance of exact matching in our setting.

The middle and bottom panels of the tables show the predicted value for the given outcome at $r = 12$ and $r = 24$, respectively. The broad take-aways are generally consistent across all models for women: there are large drops in physical performance that never return to prepregnancy levels, while training, education, and promotions remain below expectations at $r = 24$. However, the size of these predictions differ by model and highlight the importance of choosing the best comparison group. The parallel trends assumption means that the nonparents must represent a good counterfactual to the parents in the postperiod. Parallel pretrends offers support for this assumption, but parallel pretrends do not guarantee parallel posttreatment counterfactual trends. In our case, parents' (unobserved) counterfactual postbirth trajectory may differ from nonparents' (observed) postbirth trajectory. As an example, women in combat roles may have better expected physical fitness trajectories in the long run but are also less likely to become mothers. These women in combat jobs would not be a good counterfactual to the average mothers. For this reason, we prefer the conditional parallel trends assumption required in the exact matching strategies. Both exact-match placebo birth strategies (with and without time fixed effects) produce almost identical results.

We know that promotion is mechanically tied to rank and time in service, so it is particularly important to ensure parents and nonparents match on these characteristics in the preperiod. We would not want to match a low-ranking officer to mid-ranking enlisted, even if they have similar rates of promotion in the preperiod, as their subsequent expected promotion trajectories differ even in the absence of a child. Indeed, when comparing the stacked fixed effect model to the preferred

exact-match model, the promotion gap is 55% larger for mothers and, for fathers, flips direction and becomes statistically significant.

Our approach that prioritizes defining an appropriate counterfactual while still considering how to minimize TWFE bias may be useful in cases where cells sizes are too small to isolate distinct cohorts of cases treated at the same time and maintain a sufficient number of comparison cases, especially in settings where other variables (e.g., months of service) are particularly important to consider for creating a counterfactual group. For instance, with yearly state-level data where total observations are limited by the total number of U.S. states, researchers might consider grouping proximate years rather than defining groups of states with the same precise treatment year.

Table B.1: Specification Checks for Alternative Approaches for Women

| | Physical Performance (sd) (1) | Job Perfor- mance (sd) (2) | Marks- manship (sd) (3) | Training (months) (4) | Education (years) (5) | Promotions (count) (6) |
|---|--|-------------------------------------|----------------------------------|-----------------------------|-----------------------------|------------------------------|
| A. F-test (prepregnancy effects=0), <i>p</i> -value | | | | | | |
| Standard TWFE | 0.352 | 0.736 | 0.127 | – | 0.000 | – |
| Stacked TWFE | 0.009 | 0.669 | 0.023 | 0.000 | 0.134 | 0.210 |
| Placebo event, no time FE | 0.096 | 0.778 | 0.256 | 0.148 | 0.031 | 0.683 |
| Placebo event, TWFE (preferred) | 0.152 | 0.794 | 0.222 | 0.113 | 0.025 | 0.617 |
| B. 12-month effect | | | | | | |
| Standard TWFE | -0.186*** [0.000] | -0.070 [0.052] | -0.087* [0.041] | – – | -0.100*** [0.000] | – – |
| Stacked TWFE | -0.290*** [0.000] | -0.083* [0.045] | -0.123* [0.013] | -0.775*** [0.000] | -0.023 [0.073] | -0.105*** [0.000] |
| Placebo event, no time FE | -0.290*** [0.000] | -0.171*** [0.000] | -0.117* [0.017] | -0.711*** [0.000] | -0.026* [0.013] | -0.073*** [0.000] |
| Placebo event, TWFE (preferred) | -0.289*** [0.000] | -0.181*** [0.000] | -0.132** [0.006] | -0.712*** [0.000] | -0.027* [0.012] | -0.074*** [0.000] |
| C. 24-month effect | | | | | | |
| Standard TWFE | -0.129*** [0.000] | -0.178*** [0.000] | -0.027 [0.605] | – – | -0.130*** [0.000] | – – |
| Stacked TWFE | -0.174*** [0.000] | -0.015 [0.756] | -0.018 [0.758] | -0.895*** [0.000] | -0.023 [0.148] | -0.136*** [0.000] |
| Placebo event, no time FE | -0.179*** [0.000] | -0.052 [0.246] | -0.060 [0.298] | -0.829*** [0.000] | -0.027* [0.042] | -0.088*** [0.000] |
| Placebo event, TWFE (preferred) | -0.184*** [0.000] | -0.071 [0.107] | -0.065 [0.254] | -0.831*** [0.000] | -0.028* [0.040] | -0.088*** [0.000] |

Notes: Table displays tests and predicted outcomes for alternative specifications for various outcomes. Standard TWFE model is a traditional event study where the comparison group is all same-gender Marines who do not have a baby and remain in the Marine Corps for at least three years; the comparison units are not matched at a particular point in time and do not have estimates for time relative to a placebo birth. Stacked TWFE model uses exact year-month matching with the five nearest neighbors, where time relative to birth and calendar date are synonymous within groups. This model does not exact match on months of service, rank, or active/reserve status; the underlying matching within year-month does include these variables and is identical to the preferred matching model. The model includes estimates for the placebos, then tests whether the parents differ from those patterns. Exact Match, no time FE model is the same matching process as the preferred model, but does only includes $r = [-24, 24]$ rather than binning $r < -24$ and $r > 24$. The model does not include exact month-year fixed effects. The model includes estimates for the placebos, then tests whether the parents differ from those patterns. Exact Match, time FE model is the preferred model that bins $r < -24$ and $r > 24$. The model includes exact month-year fixed effects. The model includes estimates for the placebos, then tests whether the parents differ from those patterns. Details included in Data Appendix. Training and promotion outcomes excluded from the standard TWFE model because it requires a starting point for the count; parents' count starts at $r = -10$. The first panel tests for pretrends with the *p*-value of an *F*-test of whether the points estimates for $r = [-24, -11]$ statistically differ from zero. The second and third panel predicts the effect for parents at $r = 12$ and $r = 24$, respectively. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table B.2: Specification Checks for Alternative Approaches for Men

| | Physical Performance (sd) (1) | Job Perfor- mance (sd) (2) | Marks- manship (sd) (3) | Training (months) (4) | Education (years) (5) | Promotions (count) (6) |
|---|--|-------------------------------------|----------------------------------|-----------------------------|-----------------------------|------------------------------|
| A. F-test (prepregnancy effects=0), <i>p</i> -value | | | | | | |
| Standard TWFE | 0.804 | 0.323 | 0.988 | – | 0.000 | – |
| Stacked TWFE | 0.000 | 0.949 | 0.988 | 0.000 | 0.000 | 0.000 |
| Placebo event, no time FE | 0.819 | 0.119 | 0.466 | 0.252 | 0.332 | 0.399 |
| Placebo event, TWFE (preferred) | 0.859 | 0.163 | 0.355 | 0.125 | 0.349 | 0.274 |
| B. 12-month effect | | | | | | |
| Standard TWFE | 0.011* [0.029] | 0.059*** [0.000] | 0.058*** [0.000] | – – | -0.018*** [0.000] | – – |
| Stacked TWFE | -0.033*** [0.000] | 0.022 [0.061] | 0.048*** [0.000] | -0.035 [0.282] | 0.015*** [0.000] | -0.043*** [0.000] |
| Placebo event, no time FE | -0.045*** [0.000] | -0.003 [0.844] | 0.060** [0.001] | -0.032 [0.371] | 0.008 [0.073] | -0.002 [0.742] |
| Placebo event, TWFE (preferred) | -0.046*** [0.000] | -0.008 [0.586] | 0.052** [0.004] | -0.034 [0.343] | 0.008 [0.075] | -0.003 [0.618] |
| C. 24-month effect | | | | | | |
| Standard TWFE | -0.002 [0.756] | 0.007 [0.543] | 0.040** [0.004] | – – | -0.018*** [0.000] | – – |
| Stacked TWFE | -0.000 [0.978] | 0.074*** [0.000] | 0.045** [0.005] | 0.057 [0.157] | 0.025*** [0.000] | -0.046*** [0.000] |
| Placebo event, no time FE | -0.005 [0.643] | 0.041* [0.016] | 0.019 [0.352] | 0.038 [0.399] | 0.015** [0.006] | 0.013 [0.070] |
| Placebo event, TWFE (preferred) | -0.008 [0.468] | 0.030 [0.076] | 0.009 [0.666] | 0.036 [0.427] | 0.014** [0.007] | 0.012 [0.085] |

Notes: Table displays tests and predicted outcomes for alternative specifications for various outcomes. Standard TWFE model is a traditional event study where the comparison group is all same-gender Marines who do not have a baby and remain in the Marine Corps for at least three years; the comparison units are not matched at a particular point in time and do not have estimates for time relative to a placebo birth. Stacked TWFE model uses exact year-month matching with the five nearest neighbors, where time relative to birth and calendar date are synonymous within groups. This model does not exact match on months of service, rank, or active/reserve status; the underlying matching within year-month does include these variables and is identical to the preferred matching model. The model includes estimates for the placebos, then tests whether the parents differ from those patterns. Exact Match, no time FE model is the same matching process as the preferred model, but does only includes $r = [-24, 24]$ rather than binning $r < -24$ and $r > 24$. The model does not include exact month-year fixed effects. The model includes estimates for the placebos, then tests whether the parents differ from those patterns. Exact Match, time FE model is the preferred model that bins $r < -24$ and $r > 24$. The model includes exact month-year fixed effects. The model includes estimates for the placebos, then tests whether the parents differ from those patterns. Details included in Data Appendix. Training and promotion outcomes excluded from the standard TWFE model because it requires a starting point for the count; parents' count starts at $r = -10$. The first panel tests for pretrends with the *p*-value of an *F*-test of whether the points estimates for $r = [-24, -11]$ statistically differ from zero. The second and third panel predicts the effect for parents at $r = 12$ and $r = 24$, respectively. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

B.3 Connecting O*NET Data to NACE Categories

To create our graphs and tables for civilian tables, we start with occupations categorized by Standard Occupational Classification (SOC). We use an O*NET crosswalk to translate 8-digit O*NET-SOC codes to 6-digit SOC codes.⁴¹ We take the mean of the O*NET-SOC physicality levels for the 6-digit code. The Women’s Bureau of the Department of Labor provides Employment and Earnings by Occupation data, which contains the employment numbers and median wages for men, women, and overall by occupation.⁴² The occupation categories are sometimes aggregated to the 2-5 digit SOC codes for less-common occupations. We manually translate these aggregated categories from SOC to the DOL data and use the mean of the O*NET-SOC physicality levels in the group. We then link the collapsed O*NET physicality data to the DOL data. For the regressions using the data from the Women’s Bureau, we weight by number of individuals in the occupational category.

Fontenay et al. (2023) use Nomenclature statistique des activités économiques dans la Communauté européenne (NACE)-based industry categories. To get from occupation to industry, we use the industry-occupation matrix data from the U.S. Bureau of Labor Statistics.⁴³ For instance, within the Agriculture, forestry, fishing and hunting industry (2022 National Employment Matrix code 110000), there are 13,900 Chief executives (SOC code 11-1011) and 420,500 workers classified as Farmworkers and laborers, crop, nursery, and greenhouse (SOC code 45-2092). We collapse the relevant physicality for each occupation down to the broad categories used by Fontenay et al. (2023), weighted by number of workers by occupation and category. We map NACE to BLS industry categories as follows:

- NACE Agriculture, forestry, and fishing (called Section A in NACE) to 2022 National Employment Matrix code 110000 Agriculture, forestry, fishing and hunting
- NACE Manufacturing, mining, and quarrying (which included Section B Mining and quarry-

⁴¹See <https://www.onetcenter.org/taxonomy/2019/soc.html>.

⁴²See <https://www.dol.gov/agencies/wb/daddata/occupations>.

⁴³See <https://www.bls.gov/emp/tables/industry-occupation-matrix-industry.htm>.

ing; Section C Manufacturing; Section D Electricity, gas, steam, and air conditioning supply and Section E Water supply, sewerage, waste management and remediation activities) to 210000 Mining, quarrying, and oil and gas extraction; 220000 Utilities; 31-330 Manufacturing, and 562000 Waste management and remediation services.

- NACE Construction (Section F) to 230000 Construction.
- NACE Wholesale and retail trade (Section G) to BLS 420000 Wholesale trade and 44-450 Retail trade.
- NACE Hotels and restaurants (Section I) to 720000 Accommodation and food services.
- NACE Transport, storage, and communication (which included Section H Transporting and storage and Section J Information and communication) to BLS 48-490 Transportation and warehousing and 510000 Information.
- NACE Financial intermediation (which included Section K Financial and insurance activities) to 520000 Finance and insurance.
- NACE Real estate, renting, and business activities (which included Section L Real estate activities; Section M Professional, scientific and technical activities; and Section N Administrative and support service activities, which we condense to Business activities for labeling purposes) to 530000 Real estate and rental and leasing; 540000 Professional, scientific, and technical services; 550000 Management of companies and enterprises; and 561000 Administrative and support services.
- NACE Public administration and defence (Section O) to 900000 Government.
- NACE Education (Section P) to 610000 Educational services; state, local, and private.
- NACE Health and social work (Section Q) to 620000 Healthcare and social assistance.

- NACE Other services (which included Section R Arts, entertainment and recreation and Section S Other service activities) to 710000 Arts, entertainment, and recreation and 810000 Other services (except public administration).

For the regressions and graphs predicting the child penalty, we weight by number of women in the industry from BLS.⁴⁴

Appendix Table B.3 shows that as average strength (stamina) required by an occupation increases by 1 point, the proportion of females in the occupation decreases by 0.68 (0.51) percentage points. As average strength (stamina) required by an occupation increases by 1 point, the median of an occupation decreases by \$1,103 (\$957). As average strength (stamina) required by an industry increases by 1 point, the child penalty in that industry increases by 0.4 (0.5) percentage points. The final two columns are at the industry level, as the outcome comes from Fontenay et al. (2023) where industries are classified into 12 categories.

⁴⁴See <https://www.bls.gov/opub/reports/womens-databook/2020/>.

Table B.3: Civilian Women’s Outcomes by Occupational Characteristics

| | Percent female | | Median earnings | | Child penalty | |
|---------------|----------------------|----------------------|----------------------------|----------------------------|--------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Avg. strength | -0.680*** (0.166) | | -1102.835*** (167.320) | | 0.429 (0.279) | |
| Avg. stamina | | -0.514** (0.176) | | -957.467*** (166.158) | | 0.482 (0.245) |
| Constant | 56.204*** (3.329) | 54.379*** (3.113) | 78487.851*** (4569.677) | 77589.498*** (4773.090) | 18.156* (5.894) | 16.185* (5.589) |
| Observations | 317 | 317 | 317 | 317 | 12 | 12 |
| R^2 | 0.116 | 0.078 | 0.239 | 0.211 | 0.138 | 0.207 |

Notes: Columns (1)–(4) predict the outcome (percent women or median earnings) in an occupation by the given O*NET characteristic. Regressions are weighted by the number of total worked in the occupation. Outcomes and weights come from the Department of Labor (DOL) Employment and Earnings by Occupation table in 2021. Columns (5)–(6) predict the child penalty for an industry group from Fontenay et al. (2023) by the given O*NET characteristic. Categories are weighted by number of women in the industry. See data appendix for details. Heteroskedasticity robust standard errors in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.