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Parenthood and Productivity

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Abstract

Does parenthood impair workers' on-the-job productivity? We study this question and its implications for understanding the child penalties in employment observed for mothers. We focus on judges, a profession that helps overcome key empirical challenges: output can be measured precisely, it can be observed for all workers before and after childbirth because virtually no parent leaves the profession, and workloads are evenly distributed, limiting scope for selective task allocation. Using a difference-in-differences design, we find no evidence that mothers' – and fathers' – output declines during pregnancy or after they return from parental leave, and we can rule out moderate declines. We validate this result using a broad set of measures capturing both the quantity and quality of judicial work, and we document similar patterns for self-employed labor lawyers. Our findings show that motherhood need not reduce on-the-job productivity and suggest that, at least in some contexts, child penalties in employment may not be driven by lasting declines in on-the-job productivity.

JEL classification

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Keywords

child penalty, productivity, gender gap

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1 Introduction

Parenthood leads to sizable labor market penalties for mothers, but not for fathers. These child penalties have been documented in a large number of countries across the five continents and are a major factor explaining gender gaps in the labor market (Kleven and Landais, 2017; Kleven et al., 2025). Despite a large body of research, we still do not fully understand their drivers (Andresen and Nix, 2022; Kleven et al., 2021).

One possible explanation – about which we have little direct evidence – is that childbirth may reduce mothers' ability to remain productivity at work. This could reflect gender norms that place a large share of childcare responsibilities on mothers, making it harder for them to sustain on-the-job performance. It could also reflect physiological changes related to pregnancy, delivery, and breastfeeding, which previous literature has suggested may have lasting effects on mothers (Feldman et al., 2007; Hoekzema et al., 2017; Numan and Insel, 2003).[1] If such productivity losses exist and employers anticipate them, they could help explain the employment penalties mothers face and the slower career trajectories documented in the literature. Alternatively, if mothers can remain equally productive, the child penalty would more likely reflect demand-side factors unrelated to their actual output – e.g., discrimination, biased evaluations – , or social norms that affect labor supply independently of productivity.

In this paper, we provide evidence that, at least under some conditions, mothers can sustain their observed output at work after childbirth. We do so by studying the effects of parenthood in a setting based on the work of judges. This setting allows us to overcome three fundamental challenges that usually make this question hard to study. First, worker output is difficult to measure in most occupations, and available indicators often capture only a narrow subset of relevant performance. Second, observed output may depend on factors outside the worker's control that can respond endogenously to childbirth – for example, if employers expect mothers to become less productive, they may assign them fewer tasks or fewer supporting resources. Third, in most settings, productivity can only be observed for a selected group of mothers who return to their jobs after childbirth.

The judicial profession is unusually well suited to overcome these challenges. Judges' output can be measured using objective indicators that capture both the quantity and the quality of their work, such as the number of sentences they issue and the shares of sentences that are appealed or reversed in higher courts. Their workloads are also evenly distributed – cases are randomly assigned across judges working in the same types of courts and jurisdictions – which sharply limits scope for endogenous task allocation around childbirth. Finally, judging

[1] Relatedly, longer leaves taken by mothers largely related to delivery and breastfeeding could reduce mothers' productivity due to skill depreciation.

is a highly paid occupation with strong employment protection, and virtually all parents return to their jobs after childbirth. This allows us to observe productivity before and after childbirth without the usual selection problem driven by labor market exit.

Our analysis leverages comprehensive administrative data tracking rich measures of judge output and fertility spanning an entire large country – Brazil. To estimate the effects of childbirth, we follow recent literature leveraging variation in the timing of childbirth (Andresen and Nix, 2022; Kleven et al., 2019a). We compare first-degree judges having their first child in a given year with peers who do so a few years later and who are matched on gender, age, and courthouse – using a difference-in-differences design (DID). Our main outcome is the number of sentences they issue at the monthly level – promotions in the Brazilian judiciary are strongly tied to this metric, which is intrinsically related to the number of closed cases (Neto, 2009) and in line with recent literature (Dahis et al., 2020; Laneuville, 2024). We also assess effects on alternative margins tracking the quantity and quality of their work.

We find that judges maintain their recorded output after they return from statutory leave, both for mothers and fathers. The number of sentences issued do not change during pregnancy, decline during the statutory leave periods as expected, and return to pre-birth levels afterwards. For mothers in the post-leave period, we can rule out output declines larger than 6.5% relative to pre-treatment levels.

We validate these results in various ways. First, we show that similar patterns emerge for secondary output measures, namely the number of sentences ratifying settlement agreements and the number of hearings held by each judge. Second, using detailed case-level data available for the subset of labor judges, we find no evidence of adverse effects on additional output measures such as the average time to close a case or the shares of sentences appealed and reversed in higher courts – which proxy for the quality of judges' work. We also show that childbirth does not affect the characteristics of cases allocated to judges, consistent with the institutional rules on even case allocation, nor does it affect the distribution of ruling outcomes. We further document that promotion gaps do not emerge with childbirth for either mothers or fathers within our 2-year analysis period.

We then provide a discussion of factors that may help explain how judges remain equally productive after childbirth. Features of the judicial profession are likely important. Judges are high-skill workers with substantial autonomy over how they organize their work: they can schedule in-person hearings, work remotely, and manage clerks within their court unit. Using time-stamp data on sentence issuance, we show that childbirth reduces the number of daily hours during which mothers and fathers file sentences. Since overall output remains unchanged, this pattern suggests that they may work fewer hours but more intensively. Finally, we provide descriptive evidence that high-income households in Brazil, such as judges'

households, rely heavily on childcare services and domestic workers – among those with children below age two, 68% use childcare services and 76% employ a full-time domestic worker (POF-IBGE 2017-2018).

Even though extrapolation warrants caution, we provide additional suggestive evidence indicating that our findings may bear some external validity and contribute to our understanding of the child penalties. First, we replicate our main analysis for self-employed labor lawyers, who share a legal background with judges but face a markedly different work environment – notably, they enjoy no employment protection and are not entitled to parental leave. Similar to judges, we find no evidence of permanent declines in lawyers' output after childbirth. While self-employed lawyers still share some key characteristics with judges – notably the legal background and substantial autonomy over their work –, these results suggest that our main findings are not unique to judges or highly protected public sector occupations.

Second, we ask why virtually all judge mothers return to work – that is, why child penalties in employment are largely absent in the judicial profession. If this reflected highly specific traits of women who become judges, our results would be less informative about the broader drivers of the child penalty. Instead, we show that this pattern is more plausibly linked to institutional features of public sector jobs. Specifically, we show that women in permanent public sector positions in Brazil do not experience significant child penalties in employment or earnings, whereas comparable women in the private sector do – including other high-skill women and even judges' siblings and cousins, who share a similar family background. These patterns suggest that features of public sector jobs, especially strong employment protection, are a main reason why female judges do not experience employment penalties, rather than selective characteristics of judges themselves.

We conclude by providing evidence that, in the private sector, child penalties in employment are strongly driven by firm-initiated layoffs as opposed to voluntary job quits. This pattern holds across different groups of workers, including high-skill mothers, and suggests that employer-side factors may be relevant for understanding the child penalty. Taken together with our findings showing no persistent declines in observed output for some groups of high-skill mothers, this evidence is consistent with the possibility that lower firm demand for mothers may not be fully explained by actual productivity declines, at least in contexts similar to ours.

Our work contributes in different ways to a large literature studying gender gaps in the labor market (for a review, see Bertrand et al., 2011; Cortés and Pan, 2023; Olivetti and Petrongolo, 2017) and the drivers of the child penalty (e.g., Adda et al., 2017; Andresen and Nix, 2022; Bertrand et al., 2010; Bronson and Thoursie, 2019; Fiva and King, 2024; Kleven et al., 2019a,b; Kleven, 2022). First, it shows that parenthood need not reduce mothers' output

once they return to work. This suggests that, at least in some contexts, persistent declines in on-the-job output may not be a main driver of the child penalty in employment. More broadly, our findings shift attention toward mechanisms that operate through workplace responses and career structures – such as discriminatory promotion practices (Ginther and Kahn, 2004; Bronson and Thoursie, 2019; Di Giovan Paolo and Marcolin, 2025) and biased evaluations (Saygin and Knight, 2023; Anne, 2017), or social norms that shape career trajectories independent of worker output (Farré et al., 2023; Kleven, 2022).

Second, our results indicate that biological differences – e.g., related to pregnancy, delivery, and breastfeeding – do not necessarily drive gender gaps in worker productivity, especially in cognitive intensive occupations such as the one we study. They complement recent studies indicating a limited role for the biological hypothesis by analyzing child penalties in employment and earnings. Specifically, these studies show that comparable biological and non-biological mothers experience similar labor market penalties – while Kleven et al. (2021) focus on adoptive vs. non-adoptive mothers, Andresen and Nix (2022) compare biological and non-biological mothers in same-sex couples. We complement their findings by directly studying the effects of motherhood on worker productivity, leveraging an ideal setting.

Few previous works study the effects of parenthood on worker output using similar DID designs. Healy and Heissel (2022) study military mothers in the US Marine, while concurrent work by Cairo et al. (2025) and Branco et al. (2025) analyze researchers in academia and physicians working in Brazilian public hospitals. Differently from us, all of these studies find reductions in worker output.[2] A contribution of our work is showing that motherhood does not necessarily reduce worker output based on a setting where mothers have good conditions to remain productive – female judges have high autonomy over their work and the allocation of their workload is largely shielded from external factors. Our interpretation is that these conditions, jointly, may contribute to the ability of mothers to sustain output in our setting – and that the lessons from our findings and those in prior work may be complementary: together, they are consistent with occupational and institutional conditions being relevant to how parenthood affects worker output.

More generally, a distinctive feature of our setting is that it jointly overcome multiple identification challenges related to: (i) the selection into return to work, (ii) potential work environment biases affecting productivity, and (iii) productivity measurement. For instance, mothers in the US Marines face physically demanding tasks and supervisor-evaluated performance; academic researchers operate in a highly collaborative environment where peer

[2]Recent work by Bonney et al. (2025) and Rutigliano (2024) complement this evidence by showing that parenthood by female owners reduces firm performance – also documenting higher owner exit rates (Bonney et al., 2025) and lower firm survival (Rutigliano, 2024).

and coauthor interactions shape output; physicians can largely adjust their task composition and complexity making measurement challenging, and part of their overall medical output – such as outpatient or private-practice activity – may not be fully captured in administrative hospital records.

Our work also contributes to a stream of research studying gender differences in productivity and providing cross-sectional comparisons between women with and without children in different professions: corporate managers (Bertrand et al., 2010), lawyers (Azmat and Ferrer, 2017); academia (Krapf et al., 2017; Kim and Moser, 2025); politics (King, 2025); and Gallen (2024) based on a production function estimation with Danish population data.

Finally, this paper also contributes to the literature on public sector productivity (Dal Bó et al., 2013; Dahis et al., 2020; Fenizia, 2022; Fenizia and Kirchmaier, 2025). A related literature shows that mothers tend to face smaller labor market penalties in the public sector than in the private sector and that mothers tend to sort into public sector jobs (Biasi and De Paola, 2026; Cortés and Pan, 2023; Duvivier and Narcy, 2015; Kleven et al., 2019a; Nielsen et al., 2004; Simonsen and Skipper, 2006). These patterns are often linked to institutional differences such as stronger employment protection and more generous leave arrangements. One concern, however, is that these same protections may come at the cost of lower productivity after childbirth, with adverse consequences for the quality of public service provision. Our evidence from judges shows that this need not be the case: in a public sector occupation with strong employment protection and high worker autonomy, productivity does not decline after childbirth.

The paper is structured as follows. Section 2 presents the institutional background, and Section 3 describes the data. Section 4 outlines the empirical strategy, presents the results, and discusses potential mechanisms. Section 5 provides a discussion relating our findings with the child penalty literature, followed by Section 6 which concludes.

2 Institutional Background

2.1 The Brazilian Judiciary

The Brazilian judiciary comprises 27 state courts and specialized labor, federal, and electoral courts. As of 2017, roughly 15,000 judges handle about 80 million outstanding lawsuits nationwide. Judges are career public servants selected through highly competitive national examinations and granted lifetime tenure upon appointment (Dahis et al., 2020).

Judicial careers are among the most prestigious in the Brazilian public sector. Judges are in the top percentile of the national income distribution and enjoy strong employment

protection, with dismissals being extremely rare: between 2006 and 2020, 118 disciplinary cases resulted in only five dismissals. As a result, employment and earnings are highly stable over the life cycle, and exits around childbirth are virtually nonexistent. In our main sample, all female judges remained employed during the analysis period, while only one male judge left his position because of retirement. Maternity leave lasts 120 days and can be extended by 60 days; paternity leave ranges from five to twenty days, both with full wage replacement.

Judges work in a context of large and growing case backlogs – according to the National Council of Justice, it would take three years to clear the existing backlog if no new cases were filed (Conselho Nacional de Justiça, 2018). A central goal of their work is to resolve disputes quickly while ensuring impartiality and legal due process. Judges spend much of their day reading case files and legal documents, analyzing facts and evidence, and issuing decisions that resolve disputes according to the law (Kilimnik, 2006). They also preside over hearings, participate in conciliation efforts to help parties reach settlements, hear arguments from lawyers and witnesses, rule on procedural requests, and manage the work of clerks in their court unit. Overall, judging is a cognitively intensive occupation that offers substantial autonomy and flexibility. Judges can set the schedule of hearings and meetings, and a large share of their work can be done remotely – for example, reviewing cases and issuing decisions through the online court system.

2.2 Output Measurement and Career Incentives

Judges' performance is constitutionally required to be evaluated using objective criteria.[3] Although the Constitution does not specify the exact metrics, a single measure has become dominant in practice: the number of sentences issued. This is a proxy for the number of cases closed and directly related to the ability of the judicial system to process its growing backlog of cases. This metric is widely recognized in the legal literature and central to merit-based evaluations and promotion decisions (Neto, 2009); it is also systematically recorded by the National Council of Justice. Other indicators, related to settlements, hearings, time to decision, or participation in training programs, may be considered but play a secondary role.

Judges' compensation is largely fixed and does not vary mechanically with short-run output. They receive a stable monthly salary that is not directly tied to hours worked or to the number of cases resolved in a given period. In particular, judges are not paid by case and do not receive overtime compensation if they take longer to decide a case. At the same time, they face incentives to sustain productivity through formal promotion and assignment rules. Judges enter the career as substitute judges (*juiz substituto*) and may be promoted to

[3]This requirement is established by Constitutional Amendment No. 45, December 30, 2004.

senior judges (*juiz titular*), transferred to preferred jurisdictions, or elevated to higher courts. By law, allocation to one out of every two of these positions must be based exclusively on productivity, while the remaining positions are assigned according to seniority. In addition, some courts may grant productivity-related bonuses or awards based on output, further reinforcing these incentives. Thus, despite fixed short-run remuneration, judges face clear incentives to sustain high output over time.

2.3 Workload Allocation and Task Assignment

A relevant feature of the judicial career is the limited scope for uneven workload allocation, which is key to our analysis. Within jurisdictions, cases are randomly assigned across court units (*vara*) of the same type. Each court unit is staffed by a senior judge and, in some cases, an additional substitute judge. When more than one judge operates within the same unit, cases are typically divided mechanically – e.g., by assigning even-numbered cases to one judge and odd-numbered cases to another. This structure prevents systematic selection into easier or more demanding cases and substantially limits scope for workload adjustments around parenthood. Accordingly, changes in output around childbirth are unlikely to reflect shifts in task composition, a claim we test directly in the empirical analysis.

Also relevant to our analysis, case assignment follows an institutional principle whereby judges should not work together on the same case – a single judge is expected to follow the entire case since its initial assignment. As an exception, during leave periods, courts typically provide temporary coverage of the judge's docket through substitute judges, so as to avoid delays in case processing and the accumulation of work upon the judge's return.[4] Judges on leave may still issue sentences in cases that had already been submitted for decision before the leave period, absent a medical contraindication (LC 35/1979, art. 73, §2º).

Using data similar to ours, Dahis et al. (2020) show that the number of sentences issued varies substantially across Brazilian judges, and that judge fixed effects account for 23% of this variation. They also document declines in sentence output when judges are reassigned across court units. Taken together, these findings indicate that judicial output is not mechanically constrained by the even allocation of workload. Instead, judges retain substantial scope to adjust output, and performance varies meaningfully across individuals. Moreover, the large

[4]See, for example, CNJ rules on judicial substitution during leave and vacation periods: https://www.cnj.jus.br/definidas-regras-para-substitui-de-membros-dos-tribunais/; CNJ rules on the temporary substitution of judges in cases of judicial absence: https://atos.cnj.jus.br/files/compilado144929202306076480 98f96b66e.pdf; and the TJMG program providing cooperating judges to perform hearings, issue decisions, and carry out other judicial acts during maternity leave: https://www.tjmg.jus.br/portal-tjmg/noticias/juizas-do-tjmg-sao-atendidas-por-programa-de-apoio-a-magistrada-gestante-e-lactante.htm.

backlog of pending cases in the Brazilian judiciary gives judges ample room to increase output when they choose to do so.

3 Data

Our empirical analysis combines administrative data from multiple sources to track judges' productivity over time and link it to the timing of childbirth. Data construction proceeds in two main steps.

First, we identify judges in the universe of Brazilian formal workers using administrative employment records from RAIS (*Relação Anual de Informações Sociais*) for 2002–2019. RAIS allows us to identify the population of Brazilian judges by name and unique tax identifier (CPF) and provides detailed information on job spells, including contract start and end dates, earnings, reason for termination, and demographics such as birthdate, race, and education. We then link judges to monthly productivity data (*Painel de produtividade*) maintained by the National Council of Justice (CNJ) for 2015–2019. This is the official dataset used for performance evaluation in the judiciary. It includes our main measure tracking the number of sentences issued and is complemented with the number of cases settled and hearings held by each judge.[5]

Second, we identify judges who become parents using family links data draw from multiple administrative data sources, following the methodology by Britto et al. (2025a). The primary source for judges is dependent claims reported in personal income tax declarations filed with the Brazilian tax authority (2006–2020). Because of their high-income, tax filing is mandatory for judges, and dependent claims allow us to identify children and their dates of birth with high precision. This source constitutes the primary basis for identifying childbirth events among judges. Additional data sources used to construct parent–child links play a limited role for judges, given near-universal tax filing – over 99% of parent-child links for judges are based on dependent claims. However, these other sources are useful for identifying children for other worker groups used in supplementary analyses and validation exercises – see Appendix A.1 for a detailed description of this procedure.[6]

We further use detailed case-level data – PJe – covering the universe of cases filed in

[5]The link between these datasets is based on judges' full names – 89.5% of judges working in our analysis period (2015–2020) have unique names among their peers, allowing for exact matching; the remaining judges are dropped from the analysis.

[6]While coverage of parent-child links is nearly universal for judges, we can link 71% and 51% of children born in 2015-2020 to their mothers and fathers in the general population, respectively. Previous work shows that individuals who can be linked to their children using these data are observationally similar to the general population along a wide range of characteristics, alleviating concerns about selective coverage (Britto et al., 2025a).

Brazilian labor courts for 2015-2019 to construct additional productivity measures, including average time to decision and the share of sentences that are appealed in higher courts. They also enable a detailed characterization of judges' workloads, including the number of legal issues under dispute, case value, and other indicators of case complexity, as well as judges' ruling behavior. These data allow for supplementary analyses using rich measures of productivity and work allocation for nearly 3.5 thousand labor judges, who are a subset of our main sample.

Together, these datasets allow us to observe judges' productivity at the monthly level and assess both quantity and quality dimensions of performance. They also allow us to examine whether workload composition and judicial behavior change around childbirth, providing direct evidence on the stability of task allocation and decision-making after parenthood.

4 Effects of Parenthood on Productivity

4.1 Empirical Strategy

Following recent literature, we leverage variation in the timing of childbirth across individuals to estimate the effects of childbirth on judges' productivity (e.g., Andresen and Nix, 2022; Britto et al., 2024; Kleven et al., 2019a; Massenkoff and Rose, 2024). Although the decision to have a child is endogenous, the precise timing of conception and childbirth is influenced by biological and idiosyncratic factors. We exploit this variation using a difference-in-differences (DID) design that compares judges who have their first child earlier, during 2016-2017, to judges who will have their first child a few years later, during 2018-2020.

To enhance comparability, each treated judge is matched to one control judge of the same gender, working in the same courthouse, and closest in age among eligible matches. We further require that the control judge's child conception occurs sufficiently later to ensure that it does not overlap with the treated judge's analysis window.[7] The resulting matched samples have 584 men and 392 women. Both are fairly balanced across a range of characteristics, including pre-treatment productivity outcomes (Appendix Table B.1).

Once treated and control judges are matched, we adopt a stacked DID design. Each treated–control pair forms a single difference-in-differences comparison and time is normalized relative to the month of conception – control judges are assigned a placebo conception date equal to that of the treated judge. We then stack all matched pairs into a panel that follows judges from 6 months before to 28 months after conception.

[7]We infer child conception to take place nine calendar months before childbirth.

We estimate the following event-study difference-in-differences specification:

$$Y_{it} = \sum_{\tau \neq -1} \beta_\tau \left(Treat_i \times \mathbb{1}\left[t = \tau \right] \right) + \delta_i + \lambda_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} denotes the productivity of judge i in month t, $Treat_i$ is a dummy for the treatment group, and event time τ is defined relative to the month of conception. The omitted category $\tau = -1$ indicates the month preceding conception. Judge fixed effects δ_i absorb time-invariant individual heterogeneity, while relative-time fixed effects λ_t capture time shocks common to all judges.[8] Standard errors are clustered at the control judge level, allowing arbitrary correlation in the error terms across treated judges matched to the same control.[9] The coefficients β_τ estimate childbirth productivity effects for $\tau \geq 0$ and pre-trends for $\tau < 0$.

To summarize effects across economically meaningful phases, we estimate a specification that aggregates event time into pregnancy, the potential maternity leave period, and post-leave periods:

$$\begin{aligned} Y_{it} = & \beta_1 \, Treat_i \times Pregnancy_{it} + \beta_2 \, Treat_i \times MaternityLeave_{it} + \\ & \beta_3 \, Treat_i \times PostLeave_{it} + \delta_i + \lambda_t + \epsilon_{it} \end{aligned} \tag{2}$$

where $Pregnancy_{it}$ equals one during months $t = \{0, 1, ..., 8\}$, $MaternityLeave_{it}$ equals one for $t = \{9, 10, ..., 15\}$, and $PostLeave_{it}$ equals one for $t \geq 16$. The coefficients β_1, β_2, and β_3 capture the average effect of treatment during the pregnancy, maternity leave, and post-leave periods, respectively.

Our stacked difference-in-differences design ensures that control judges remain not yet treated throughout the analysis window, including the conception period, and avoids contamination or negative weighting issues that can arise in staggered treatment settings (Baker et al., 2022; Dube et al., 2023).[10] The key identifying assumption is that, absent childbirth, treated and control judges would have followed parallel productivity trends. We assess the plausibility of this assumption by examining productivity dynamics prior to conception. We further show that the estimated effects are stable across alternative definitions of the control group based on different distances in childbirth timing, suggesting that the results are not driven by differential treatment timing.

[8] Calendar months fixed-effects are absorbed by relative-time fixed effects in this design, adding the former to our specification produces virtually no changes in the estimates – see robustness Section 4.2.3.

[9] This clustering choice follows the recommendation of De Chaisemartin and Ramirez-Cuellar (2024), who show that in matched-pair and small-strata settings, clustering at the unit level can underestimate standard errors—even when treatment effects are constant. Clustering at the level of the match—here, the control judge—correctly accounts for correlation introduced by the matching procedure.

[10] Baker et al. (2022) provides an extensive discussion of the *stacked* difference-in-differences design as a solution to concerns raised by the two-way fixed effects literature and shows that it closely approximates alternative estimators.

4.2 Main Results

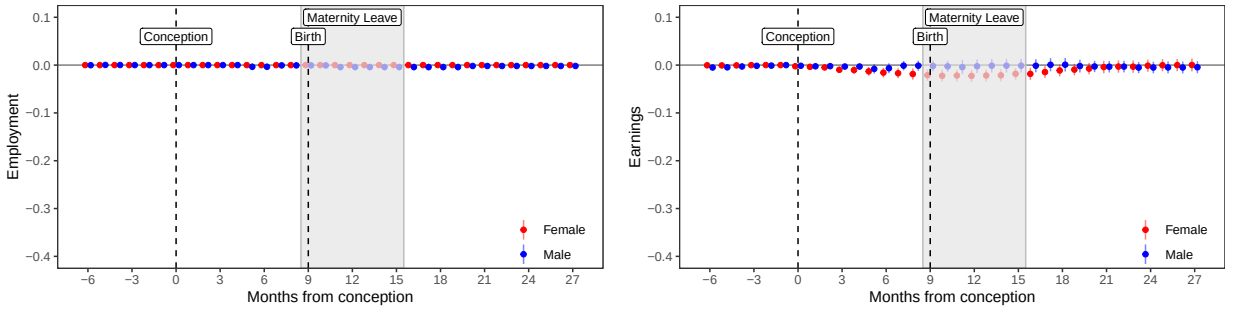
We turn to our main analysis of the effects of childbirth on judges' productivity, following the DID design described in Section 4.1. Our core outcome is the number of sentences issued, which is the main performance metric used in judges' official evaluations. We also analyze effects on the number of settlement agreements ratified by the judge and hearings held, which are secondary measures tracked in the official CNJ productivity data. All estimates are rescaled by the average outcome in the control group in the month before child conception, so that coefficients can be interpreted as percentage changes relative to the baseline. In all plots, we highlight the potential period when mothers take their statutory maternity leave – they can vary slightly across mothers since they may choose to start the leave up to one month before the expected birth date.

In Figure 1, we first document that childbirth does not affect the probability that judges remain in the profession and their earnings. Effects on the probability of employment as judges are virtually zero and statistically insignificant, both for mother and fathers. This is consistent with the fact that only one male judge in the treatment group leaves the profession due to retirement during our analysis period. In turn, we find that earnings remain essentially unchanged, with any responses being small and short-lived.

Figure 1: Effect of parenthood on judge's career

(a) Employed as judge

(b) Earnings as judge



Notes: This figure shows the effect of parenthood on: (a) employment as judges and (b) the associated earnings; based on RAIS data, as estimated from the difference-in-difference Equation 1 - along with 95% confidence intervals. Earnings are measured in Brazilian Reais at 2015 price levels. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

We next turn to our main analysis studying childbirth effects on our main measure of judge output: the number of sentence issued. Figure 2a shows the raw average for the treatment and control groups, while Figure 2b shows the estimated effects. During pregnancy,

mothers' productivity remains stable, and we find no evidence of differential pre-trends prior to the inferred conception month. As expected, the number of sentences issued by mothers declines sharply during the maternity leave period. This decline begins in the calendar month prior to birth, reflecting the fact that mothers can choose to start their leaves from the eighth pregnancy month. Productivity begins to recover in the sixth *calendar* month after birth (month 15 in the plot), when most mothers gradually return to work over the course of the month, as maternity leave typically lasts six months. These patterns are consistent with the timing of maternity leave, as well as the take-up any other leaves, reported in Appendix Figure B.1.

Our core result concerns the post-leave period. Mothers' productivity rebounds quickly once they resume full-time work: the effect on the number of sentences issued is close to zero and statistically insignificant starting from the second month after the potential leave period, highlighted in the plots. Notably, the average effect during the post-leave period is not statistically significant and has a positive sign. Hence, any productivity decline seems only temporary, lasting for a single month after judges return to work. Such pattern may be related to the disruption caused by the reallocation of cases to and from substitute judges during the leave period. The 95% confidence interval allows us to rule out moderate productivity losses for mothers. The lower bound of the overall post-leave period is -10.9%, and increases to -6.5% once we drop the first transition month (number 16 in the plot) when mothers return to work.

Results for fathers' productivity are qualitatively similar. Average effects on the number of sentences issued during pregnancy and in the post-leave period are statistically insignificant. Temporary declines in fathers' productivity are observed around childbirth – when most fathers take their paternity leave lasting up to 20 days.

In Appendix Figure B.2, we extend our main analysis to additional productivity measures from the official CNJ judge productivity data – the number of settlement agreements and hearings held. For both measures, we find no evidence that childbirth leads to persistent changes in output during pregnancy or once parents return to work.

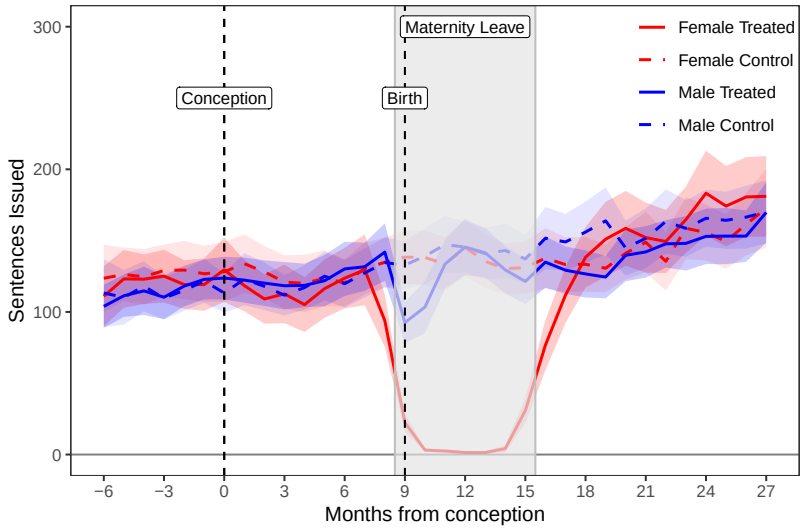
4.2.1 Additional productivity measures based on labor cases data

We next analyze additional key productivity measures that are not tracked in the official judge productivity data covering all judges in the country. This allows us to assess whether judges maintain sentence output after childbirth by adjusting other margins of behavior, e.g., by decreasing sentence quality.

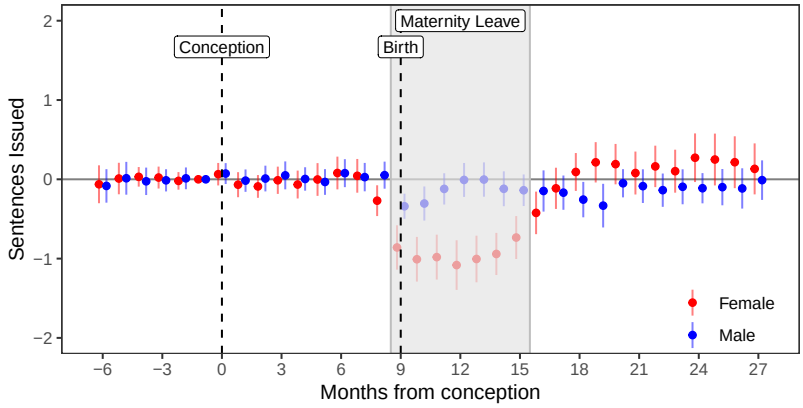
To do so, we use detailed case-level data from labor courts, which employ nearly 3,500 labor judges and allow us to construct granular output measures for the subset of labor

Figure 2: Effect of parenthood on judge output: sentences issued

(a) Raw trends



(b) Effects



Female: Baseline = 126.73
Pregnancy Coef. = −4.28 (6.27) , Maternity Leave Coef. = −119.46 (16.41) , Post Leave Coef. = 12.57 (13.30)
Avg. Pregnancy Coef. = −3.38 % , Avg. Maternity Leave Coef. = −94.26 % , Avg. Post Leave Coef. = 9.92 %

Male: Baseline = 121.66
Pregnancy Coef. = 4.98 (6.64) , Maternity Leave Coef. = −16.25 (9.78) , Post Leave Coef. = −14.60 (11.33)
Avg. Pregnancy Coef. = 4.09 % , Avg. Maternity Leave Coef. = −13.36 % , Avg. Post Leave Coef. = −12.00 %

Notes: Panel (a) shows the average number of sentences issued – CNJ data – by judges in the treatment and control groups around childbirth, along with 95% confidence intervals. Panel (b) shows the effect of parenthood on the same outcome, as estimated from the difference-in-difference Equation 1 - along with 95% confidence intervals. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

judges in our main analysis sample. We focus on two dimensions of decision quality. First, we analyze the average time to decision for cases handled by each judge – a measure of timeliness that captures whether judges process cases more slowly after childbirth. Second, we study the share of sentences that are appealed and the share that are reversed in higher courts. While appeals are an imperfect proxy for quality – as parties may appeal for reasons unrelated to decision quality, such as case stakes or litigation costs – reversals provide a cleaner signal,

as they reflect disagreement with the decision by a higher court. Together, these measures allow us to assess whether judges sustain output after childbirth by issuing lower-quality decisions. One limitation of this analysis is the smaller final sample size: there are 68 treated and control units in the mothers' analysis and only 24 in the fathers' analysis. Nevertheless, our main interest is in the mothers' analysis, for which the sample size is still reasonable – as suggested, for example, by Angrist and Pischke (2009).[11] We further address inference concerns with a simulation-based assessments following Ferman (2019). We find only mild over-rejection: tests at the 5% significance level indicate 6.2% and 6.5% rejection rates in the mothers and fathers' samples, respectively – see Appendix B.2.1 for a detailed description of this procedure.

Figure 3 reports the results. Panel (a) shows no evidence of delays following childbirth. Rather, it shows a statistically significant 18% reduction in the average duration of cases sentenced for mothers, while the same coefficient is not significant for fathers (Figure 3a). Panels (b) and (c) investigate effects on decision quality based on appeals data. We find statistically significant *reductions* in the share of sentences appealed – they decrease by 15% and 23% for mothers and fathers in the post-leave period (Figure 3b). Similar results emerge for the share of sentences reversed in appeals for mothers – they decrease by 32% for mothers in the post-leave period. Instead, effects for fathers on this outcome are not statistically significant (Figure 3c).

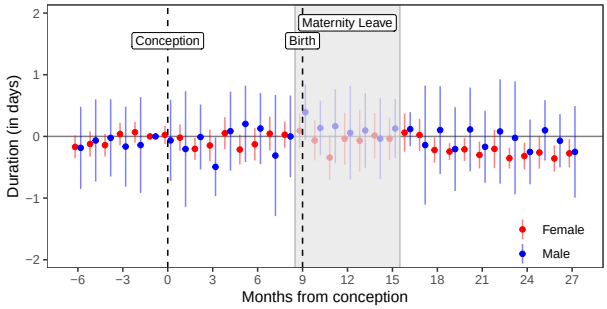
We further use the labor court data to study other dimensions of judicial behavior at the decision stage – see Table 1. We focus on the post-leave period when mothers have returned to work. Childbirth does not affect the share of cases in which judges rule in favor or against the plaintiff, or the share of cases resolved through settlement agreements. We also find no statistically significant changes in the average amount awarded in sentences. These results indicate that childbirth does not lead to economically meaningful adjustments in observable ruling behavior once judges return to work.

Taken together, these results indicate that judicial productivity does not decline after childbirth once judges return to work. This conclusion is supported by evidence on both the quantity of output and observable dimensions of decision quality. If anything, our evidence suggests improvements along some margins for mothers: their decision are less likely to be appealed and reversed after childbirth, and average case duration decreases.

[11] In Appendix Figure B.3, we show that our main results for the number of sentences issued and settlement agreements continue to hold in this sample.

Figure 3: Effect of parenthood on alternative measures of judicial productivity – labor judges

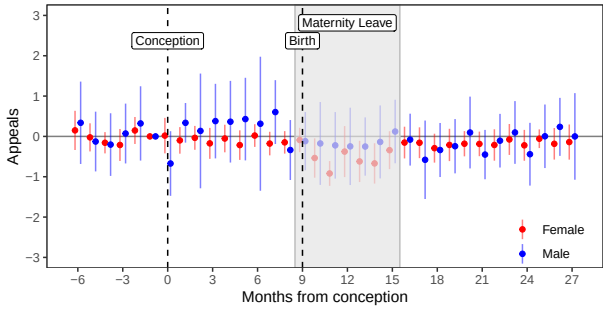
(a) Time to sentence



Female: Baseline = 291.04
Pregnancy Coef. = −4.65 (21.06) , Maternity Leave Coef. = −5.42 (37.26) , Post Leave Coef. = −51.27 (19.84)
Avg. Pregnancy Coef. = −1.60 % , Avg. Maternity Leave Coef. = −1.86 % , Avg. Post Leave Coef. = −17.62 %

Male: Baseline = 181.45
Pregnancy Coef. = 3.10 (15.72) , Maternity Leave Coef. = 41.94 (26.17) , Post Leave Coef. = 8.53 (19.04)
Avg. Pregnancy Coef. = 1.71 % , Avg. Maternity Leave Coef. = 23.11 % , Avg. Post Leave Coef. = 4.70 %

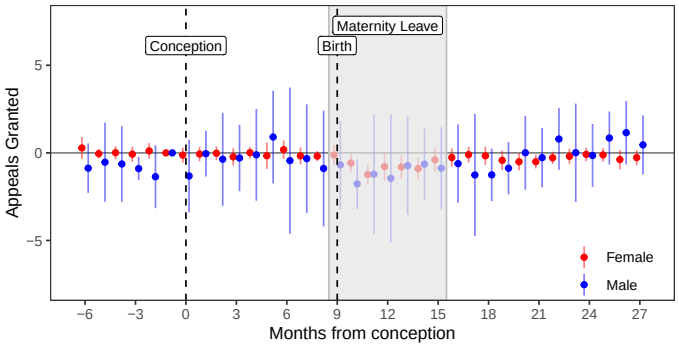
(b) Share of sentences appealed



Female: Baseline = 0.47
Pregnancy Coef. = −0.04 (0.04) , Maternity Leave Coef. = −0.23 (0.08) , Post Leave Coef. = −0.07 (0.04)
Avg. Pregnancy Coef. = −7.65 % , Avg. Maternity Leave Coef. = −48.27 % , Avg. Post Leave Coef. = −15.19 %

Male: Baseline = 0.33
Pregnancy Coef. = 0.03 (0.09) , Maternity Leave Coef. = −0.07 (0.06) , Post Leave Coef. = −0.07 (0.03)
Avg. Pregnancy Coef. = 9.96 % , Avg. Maternity Leave Coef. = −21.48 % , Avg. Post Leave Coef. = −22.67 %

(c) Share of sentences reversed



Female: Baseline = 0.20
Pregnancy Coef. = −0.03 (0.02) , Maternity Leave Coef. = −0.14 (0.04) , Post Leave Coef. = −0.06 (0.03)
Avg. Pregnancy Coef. = −12.70 % , Avg. Maternity Leave Coef. = −71.91 % , Avg. Post Leave Coef. = −31.61 %

Male: Baseline = 0.08
Pregnancy Coef. = 0.03 (0.06) , Maternity Leave Coef. = −0.03 (0.07) , Post Leave Coef. = 0.05 (0.04)
Avg. Pregnancy Coef. = 39.64 % , Avg. Maternity Leave Coef. = −31.96 % , Avg. Post Leave Coef. = 60.51 %

Notes: This figure presents the effect of parenthood on: (a) average time to sentence, (b) share of sentences appealed in higher courts, and (c) the share of appeals reverting the judge's sentence, using the PJe dataset and the difference-in-differences specification in Equation 1, along with 95% confidence intervals. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The baselines, the average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), as well as the relative effects during these periods, are also reported at the bottom of each graph.

4.2.2 Case allocation measures based on labor cases data

Next we leverage again case-level data from labor justice to study whether childbirth affects work allocation – focusing on the post-leave period. Consistent with the institutional setting in which cases are evenly allocated, we find no evidence of systematic changes in the characteristics of cases sentenced after childbirth. As shown in Table 2, childbirth does not affect the average value claimed in a case, the number of legal issues discussed, the number of defendants involved, either for mothers or fathers. In addition, judges are neither more nor less likely to handle cases involving different topics following childbirth.

Table 1: Effect of parenthood on case outcomes

	(1)	(2)	(3)	(4)	(5)
	Win	Partial win	Settlement	Lose	Sentence amount
			Panel A: Female		
Treat x Post Leave	-0.0053	0.0078	0.0322	0.0172	1,416
	(0.0067)	(0.0382)	(0.0357)	(0.0200)	(3,286)
Baseline	0.0183	0.5412	0.1829	0.1865	34684
Observations	2,015	2,015	2,015	2,015	1,988
			Panel B: Male		
Treat x Post Leave	0.0031	-0.1818	0.1228	0.0005	-4,447
	(0.0195)	(0.1193)	(0.1271)	(0.0638)	(7,213)
Baseline	0.0053	0.2746	0.2699	0.2906	13312
Observations	721	721	721	721	715

Notes: This table reports the effect of parenthood on various case outcomes using the PJe dataset, estimated from the difference-in-differences model described in Equation 2. Each column corresponds to a different outcome: probability of a full win for the plaintiff (Column 1), partial win (Column 2), settlement (Column 3), and full loss (Column 4). Column 5 presents the total amount that defendants are ordered to pay as part of the sentence. Panels A and B present results for female and male judges, respectively. Standard errors are reported in parentheses and are clustered at the control judge level. The row labeled Baseline reports the average value of the outcome in the control group at $t = -1$. The number of observations is reported at the bottom of each panel. Significance levels: *** p≤0.01, ** p≤0.05, * p≤0.1.

Table 2: Effect of parenthood on case workload characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Initial value	# defendants	# legal issues	Worker registration	Social contrib.	Working hours	Mandated benefits	Severance payments	Unemployment insurance	Other sep. issues	Civil prosecution
						Panel A: Female					
Treat x Post Leave	425	0.1362	0.2965	0.0432	0.0644	0.0013	0.0024	0.0457	0.0047	0.0161	0.0043
	(6,328)	(0.1740)	(0.3800)	(0.0410)	(0.0380)	(0.0392)	(0.0290)	(0.0274)	(0.0326)	(0.0183)	(0.0377)
Baseline	62768	1.8437	8.9370	0.2184	0.3305	0.6097	0.5624	0.6398	0.1625	0.1641	0.4728
Observations	2,015	2,015	2,015	2,015	2,015	2,015	2,015	2,015	2,015	2,015	2,015
						Panel B: Male					
Treat x Post Leave	-3,557	-0.0547	0.0503	-0.0034	-0.0175	0.0187	-0.0607	0.0354	-0.0298	-0.0131	0.0020
	(7,663)	(0.1194)	(0.7379)	(0.0759)	(0.0394)	(0.0428)	(0.0789)	(0.0585)	(0.0352)	(0.0370)	(0.0584)
Baseline	34467	1.3763	4.1056	0.1737	0.1236	0.2837	0.3528	0.4698	0.0717	0.1702	0.2589
Observations	721	721	721	721	721	721	721	721	721	721	721

Notes: This table reports the effect of parenthood on selected case characteristics and on the probability that the main issue in a case falls into one of eight aggregated topic categories, using the PJe dataset and the classification in Britto et al. (2023). Estimates are obtained from the difference-in-differences model in Equation 2. Columns 1 to 3 report effects on the initial monetary value claimed by the plaintiff, the number of defendants, and the number of legal issues in the case. Columns 4 to 11 report effects on the probability that the main issue in a case concerns worker registration status, social contributions, working hours, mandated benefits, severance payments, unemployment insurance, other separation-related issues, or civil prosecution. Panels A and B present results for female and male judges, respectively. Standard errors are reported in parentheses and are clustered at the control judge level. The row labeled Baseline reports the average value of the outcome in the control group at $t = -1$. The number of observations is reported at the bottom of each panel. Significance levels: *** p≤0.01, ** p≤0.05, * p≤0.1.

4.2.3 Robustness

We next provide additional tests on the robustness of our main findings. First, we assess whether varying the distance in birth timing between treated and control judges affects our core result of no post-leave effects on our main outcome – the number of sentences issued.

Shorter timing distances improve comparability between groups, strengthening the plausibility of the common-trend assumption, though they also shorten the analysis window because control judges eventually reach their own conception period (Fadlon and Nielsen, 2021). Appendix B.3.1 shows that varying the timing distance from two years down to six months does not meaningfully affect our estimates of the number of sentences issued, supporting the robustness of our main findings. Second, we add calendar month fixed effects to our main specification, alongside the relative event-time dummies. As shown in Appendix B.3.2, the estimates and standard errors remain virtually unchanged, indicating that our results are not driven by unobserved calendar-time shocks or by the particular way time is absorbed in the model. Additional robustness checks addressing potential inference concerns are reported in Appendix B.3.3.

4.3 Career dynamics

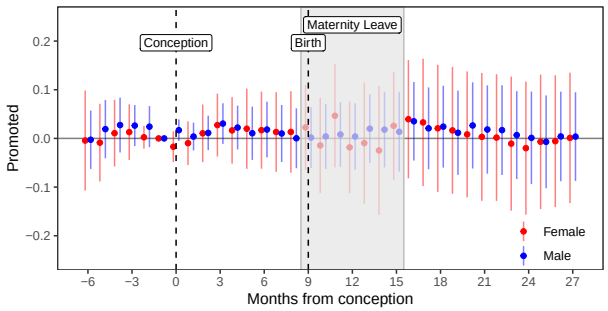
We next study the effects of childbirth on judges' career dynamics. We begin by assessing impacts on promotions, focusing on whether parenthood slows down career progression even in the absence of productivity losses. Figure 4a reports effects on the probability that judges are promoted – including progression to senior judge status and appointments to appellate courts. The figure shows that childbirth does not affect promotion probabilities for either fathers or mothers. This result suggests that parenthood does not hinder career advancement in the judiciary. Also, given the null effects on output, this result is also consistent with hte institutional setting in which promotions are formally tied to objective productivity measures rather than discretionary evaluations (in addition to seniority).

We next analyze effects on judges' geographic mobility by studying assignments to courts located in metropolitan areas – positions that are typically subject to high demand and competition among peers. Figure 4b shows that childbirth does not affect the probability that judges work in metropolitan courts for either fathers or mothers.[12] There is only mild evidence that female judges are more likely to move to metropolitan courts toward the end of the pregnancy period. However, this pattern appears to be driven by the anticipation of such transitions, as the estimates become smaller and statistically insignificant a few months after the end of the maternity leave. In turn, there is also mild evidence that fathers are less likely to work in metropolitan courts but, similarly to mothers, average effects are not statistically significant in any period.

[12]Differently from our analysis, the figure reports absolute rather than relative estimates.

Figure 4: Effect of parenthood on career advancement

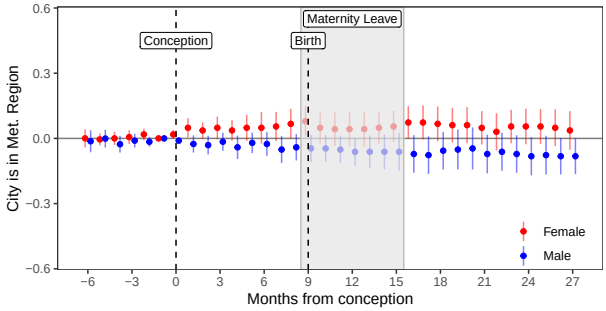
(a) Ever promoted



Female: Baseline = 0.25
Pregnancy Coef. = 0.01 (0.05) , Maternity Leave Coef. = 0.00 (0.06) , Post Leave Coef. = 0.00 (0.06)
Avg. Pregnancy Coef. = 3.04 % , Avg. Maternity Leave Coef. = 1.79 % , Avg. Post Leave Coef. = 1.48 %

Male: Baseline = 0.24
Pregnancy Coef. = −0.00 (0.03) , Maternity Leave Coef. = −0.01 (0.04) , Post Leave Coef. = −0.00 (0.05)
Avg. Pregnancy Coef. = −1.03 % , Avg. Maternity Leave Coef. = −2.59 % , Avg. Post Leave Coef. = −1.10 %

(b) Working in a metropolitan court



Female: Baseline = 0.71
Pregnancy Coef. = 0.04 (0.02) , Maternity Leave Coef. = 0.05 (0.04) , Post Leave Coef. = 0.05 (0.04)
Avg. Pregnancy Coef. = 5.85 % , Avg. Maternity Leave Coef. = 6.69 % , Avg. Post Leave Coef. = 7.25 %

Male: Baseline = 0.55
Pregnancy Coef. = −0.02 (0.02) , Maternity Leave Coef. = −0.04 (0.03) , Post Leave Coef. = −0.06 (0.04)
Avg. Pregnancy Coef. = −3.30 % , Avg. Maternity Leave Coef. = −8.15 % , Avg. Post Leave Coef. = −10.62 %

Notes: This figure shows the *absolute* effect of parenthood on career advancement outcomes using the CNJ dataset, as estimated from the difference-in-difference specification Equation 1 - along with 95% confidence intervals. Panel (a) reports effects on an indicator for whether the judge has ever been promoted relative to their baseline position. This variable switches from 0 to 1 in the month the first promotion occurs and remains 1 thereafter, capturing changes in the cumulative likelihood of promotion. Panel (b) shows the effect on whether the court where the judge most frequently issues sentences is located in a metropolitan region, capturing mobility toward more prestigious judicial districts. The baselines, the average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), as well as the relative effects during these periods, are also reported at the bottom of each graph.

4.4 How do judges adjust to keep up with productivity?

Next we discuss how judges are able to have children while maintaining output levels. A first consideration is that judges, as highly paid workers, can afford the costs of childcare and domestic services. Among Brazilian parents with children below two in the top 1% of the income distribution, 76% employ a full-time domestic worker in their household and 68% rely on childcare services. It is therefore plausible that the use of such services contributes to judges' ability to keep up with their productivity after childbirth.

We next provide evidence on how judges adjust their work time. Using case-level data from labor courts, we exploit time-stamp information reporting the time of day when labor judges file sentences. While this is an imperfect proxy for judges' working time and only captures one aspect of their work, it may offer some indication on how judges adjust the working schedule. Using these data, we create four indicators: the number of active daily hours when judges issue sentences, the share of sentences issued in the busiest hour, the initial and last of hour of day when judges issue sentences.

We find evidence that judges concentrate their work issuing sentences once they become parents – see Table 3 for the results. Female and male judges adjust their sentence timing in slightly different ways. Female judges issue their sentences using 2.07 less daily hours, relative to 6.71 hours in the baseline, and they also concentrate a higher share of sentences in a single busiest hour. Instead, male judges reduce the overall span of daily hours when

they issue senteces: their earliest sentence issuance time increase by 2.91 hours, while their latest hour decreases by 1.62 hours. Their number of active hours also decline by .94 hours, thought this effects is not statistically significant.

Although these measures on sentence issuance time are imperfect proxies for total working hours – missing adjustments for other tasks –, the results offer some indication that childbirth might reduce total working time without affecting total output. We interpret this as indirect, suggestive evidence that judges may adjust to parenthood by increasing the intensity of their work, effectively raising output per hour.

Table 3: Effect of parenthood on the timing of sentence issuance

	(1)	(2)	(3)	(4)
	# Active daily hours	Share in busiest hour	First hour of day	Last hour of day
		Panel A: Female		
Treat x Post Leave	-2.0706**	0.0532*	0.1649	0.1718
	(0.8376)	(0.0290)	(0.4401)	(0.3913)
Baseline	6.7059	0.2763	7.0212	20.1558
Observations	2,286	2,177	2,177	2,177
		Panel B: Male		
Treat x Post Leave	-0.9390	0.0288	2.9148*	-1.6179*
	(0.7144)	(0.0404)	(1.3317)	(0.8231)
Baseline	6.0000	0.4368	11.2455	18.9833
Observations	792	749	749	749

Notes: This table reports the effect of parenthood on the daily work schedule of labor judges using the PJe dataset, estimated from the difference-in-differences model described in Equation 2. Each column corresponds to a different outcome: number of active daily hours during which judges issue sentences (Column 1), share of sentences issued in the busiest hour of the day (Column 2), first hour of day at which judges issue sentences (Column 3), and last hour of day at which judges issue sentences (Column 4). Panels A and B present results for female and male judges, respectively. Standard errors are reported in parentheses and are clustered at the control judge level. The row labeled Baseline reports the average value of the outcome in the control group at $t = -1$. The number of observations is reported at the bottom of each panel. Significance levels: *** p≤0.01, ** p≤0.05, * p≤0.1.

4.5 External validity: parenthood and lawyers' output

While our judge-setting is ideal to study the effects of parenthood on worker output, it is also specific. We next provide evidence that similar patterns emerge for a different group of workers – self-employed labor lawyers. Although they share the legal background with judges, they face a markedly different employment environment and set of incentives relative to judges. As self-employed professionals, they do not benefit from job security or formal parental leave arrangements. In addition, despite being high-skill professionals, their average income is well below that of judges – on average, judges earn 3.53 times more than lawyers (2010 Population Census).

Using case-level labor court data, we replicate the empirical strategy applied to judges and study the effects of childbirth on lawyers' output. We focus on near 300k self-employed lawyers

who act in labor cases during 2015-2019 and who are not registered as regular employees.[13] Specifically, we analyze lawyers who have their first child in 2016-2017 and match them to lawyers who have their first child in 2018-2020.[14] To measure lawyers' productivity, we focus on the number of cases they file – analogous to judges – and on the number of cases they win. The latter is a relevant measure of lawyer output since most labor lawyers operate under contingency-fee arrangements when representing workers – that is, they typically earn a share of the proceeds awarded to their client. Although these lawyers do not have formal parental leave, we report average effects over the same periods used in the main analysis, based on equation (2).

The results, shown in Figure 5, indicate that childbirth is not associated with persistent reductions in lawyers' output. Although we observe a temporary decline in workload around childbirth – particularly for the number of cases filed by mothers – these effects are short-lived. Because self-employed lawyers are not entitled to statutory maternity leave, these temporary reductions in output reflect private behavioral responses rather than institutional leave rules. Within a few months of childbirth, lawyers' activity levels recover, and we find no evidence of lasting effects on either the number of cases filed or the number of cases won. In Appendix Table B.3, we further show that childbirth has no effects on several other case outcomes, including sentence amounts, case duration, and the probabilities of appeal and reversal. Overall, the results do not indicate significant changes in any of these outcomes for either mothers or fathers.[15]

Although still tied to a specific setting, this evidence shows that the absence of permanent productivity losses is not unique to judges or to highly protected public sector jobs. It indicates that childbirth does not generate a persistent productivity penalty in two markedly different occupations within the legal profession.

4.6 Comparative evidence: explaining the lack of child employment penalties for judges

Next, we aim to better understand the fact that virtually all judge mothers return to their jobs – i.e., the absence of child penalties in employment in the judicial profession. If this pattern were driven by highly specific judge characteristics, the documented effects of childbirth on

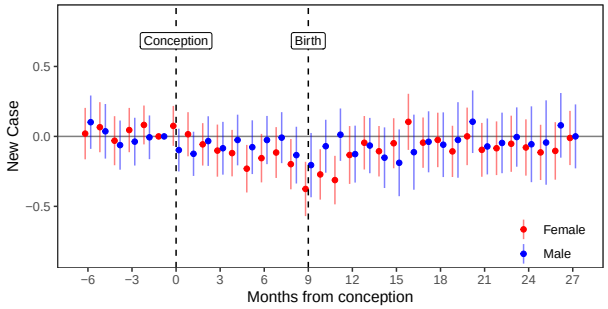
[13] Out of 500k lawyers acting in labor cases during 2015-2019, only 40.7% appear as regular employees in our employment data – for example, hired by law firms.

[14] Treated and control lawyers are matched on the following characteristics: (i) gender, (ii) municipality of residence, and (iii) closest age among eligible matches. We then estimate the difference-in-differences models described in Equation 1 and Equation 2.

[15] In Appendix Table B.4, we also show that the characteristics of cases handled by lawyers do not change – even though, unlike for judges, case allocation is not random.

Figure 5: Effect of parenthood on self-employed lawyer's productivity

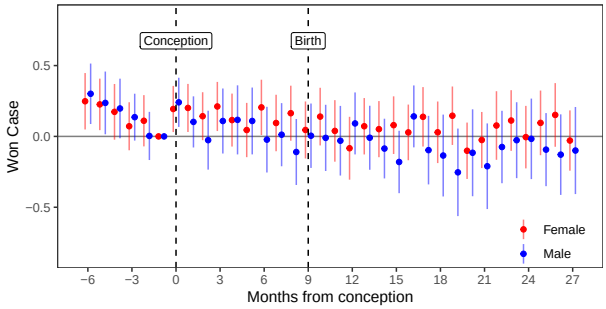
(a) Number of new cases



Female: Baseline = 2.14
Pregnancy Coef. = −0.27 (0.10) , Maternity Leave Coef. = −0.46 (0.13) , Post Leave Coef. = −0.17 (0.13)
Avg. Pregnancy Coef. = −12.82 % , Avg. Maternity Leave Coef. = −21.47 % , Avg. Post Leave Coef. = −8.09 %

Male: Baseline = 3.49
Pregnancy Coef. = −0.24 (0.18) , Maternity Leave Coef. = −0.40 (0.26) , Post Leave Coef. = −0.09 (0.34)
Avg. Pregnancy Coef. = −6.95 % , Avg. Maternity Leave Coef. = −11.44 % , Avg. Post Leave Coef. = −2.44 %

(b) Number of won cases



Female: Baseline = 1.13
Pregnancy Coef. = 0.02 (0.07) , Maternity Leave Coef. = −0.10 (0.08) , Post Leave Coef. = −0.09 (0.08)
Avg. Pregnancy Coef. = 1.96 % , Avg. Maternity Leave Coef. = −8.40 % , Avg. Post Leave Coef. = −8.14 %

Male: Baseline = 1.77
Pregnancy Coef. = −0.14 (0.11) , Maternity Leave Coef. = −0.29 (0.15) , Post Leave Coef. = −0.40 (0.19)
Avg. Pregnancy Coef. = −7.73 % , Avg. Maternity Leave Coef. = −16.55 % , Avg. Post Leave Coef. = −22.63 %

Notes: This figure shows the effect of parenthood on the productivity of self-employed lawyers, using outcomes based on the PJe data, as estimated from the difference-in-difference Equation 1 - along with 95% confidence intervals. Panel (a) shows the effect on the number of new cases, and Panel (b) shows the effects on the number of won cases. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

productivity would be less informative about mothers' ability to sustain employment and productivity in the broader population, as well as about the underlying drivers of the child penalty. Instead, we show that the limited child employment penalties observed for judges are plausibly linked to institutional features of public sector employment.

4.6.1 Child employment penalties in the public sector

We begin by documenting child employment penalties for the broad population of permanent public sector workers in Brazil. Similar to judges, entry into permanent public sector positions is based on competitive civil service examinations and, once appointed, workers enjoy very strong employment protection – dismissal is possible only in cases of severe disciplinary misconduct.

We replicate the empirical strategy applied to judges to study the effects of childbirth on employment among public sector workers. Using our employment and family link data, we identify individuals who have their first child while holding a permanent public sector job. Our analysis focuses on public sector workers who have their first child during 2014-2016 and matches them to public sector workers who have their first child three calendar years later.[16]

Figure 6 presents the effects of childbirth on employment for the full sample of public

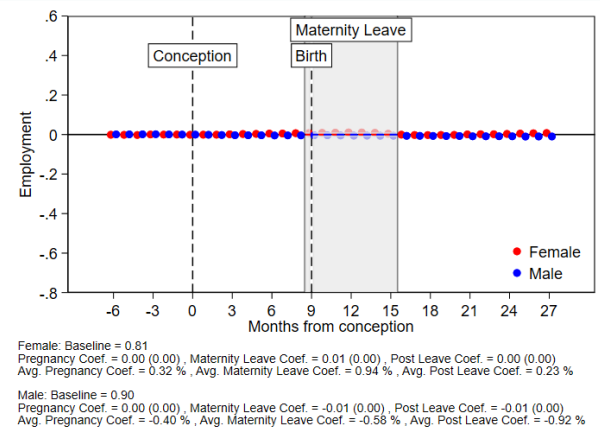
[16]Each treated individual is matched to a control based on six baseline characteristics: (i) gender, (ii) municipality of work, (iii) year of birth, (iv) presence in RAIS in the year before childbirth, (v) registration in CadÚnico in that same year, and (vi) whether they hold a college degree. We then estimate the difference-in-differences models described in Equation 1 and Equation 2.

sector workers and for top earners – defined as those earning above 25k BRL, similarly to judges. Panel (a) shows that public sector workers do not experience employment losses following the birth of their first child, for either fathers or mothers. Panel (b) shows that top earners instead experience small employment gains around childbirth – this is explained by less job quits rather than layoffs which are extremely rare for public workers. In Appendix Figure B.7, we replicate the analysis by income quartiles and show that childbirth does not generate child employment penalties for any group, regardless of baseline income or gender.

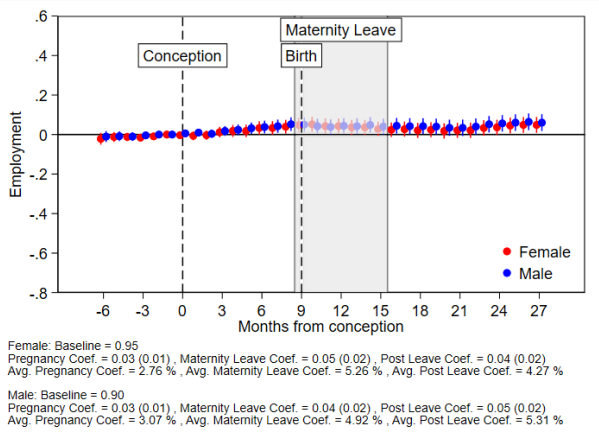
Overall, these results indicate that the absence of child employment penalties observed for judges is a broader feature of public sector employment. The fact that these results hold for low-income public workers suggests that employment stability – rather than high wage levels – may be the prime public sector feature explaining this result. More generally, we interpret these findings as evidence that the absence of child employment penalties for judges, especially for mothers, is unlikely to be driven by particularly selected individual traits and is more plausibly explained by the institutional characteristics of public sector jobs.

Figure 6: Effect of parenthood on public sector worker's employment

(a) All



(b) Top Earners



Notes: This figure shows the effect of parenthood on public sector workers' employment using the RAIS data, as estimated from the difference-in-difference Equation 1 - along with 95% confidence intervals. Panel (a) shows the effect on the probability of being employed for all matched public workers. Panel (b) displays the same outcome for top earners—defined as public workers earning at least R$25,000 (the initial wage of a judge in Brazil) in the year we selected them in RAIS. Earnings are measured in Brazilian Reais at 2015 price levels. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

4.6.2 Comparative evidence: child employment penalties for workers similar to judges working in the private sector

We proceed by studying child employment penalties for workers who are similar to judges but employed in the private sector. If employment penalties are present for judge-like workers in

the private sector, this would further support the idea that judges experience no employment losses because of their public employment status.

We focus on four comparison groups of individuals employed in the private sector prior to childbirth: workers with a college degree, salaried lawyers, top earners, and judges' siblings and cousins who share the same family background – identified using our family links data. As before, we replicate our main empirical strategy to study child employment penalties. The treatment group is composed of individuals who have their first child during 2014–2016 and who held a private sector job at the end of the previous calendar year, while the control group is defined by matching to similar individuals who have their first child three calendar years later.[17]

The results for each comparison group are presented in Figure 7. Strikingly, all groups of mothers who share similar traits with judges but are employed in the private sector experience significant child employment penalties. On average, employment in any job declines by 17 p.p. for judges' siblings and cousins, who share a similar family background with judges – e.g., gender norms and the social environment. Women in private sector jobs with a college degree, salaried lawyers, and other top earners experience employment declines of 8–10 p.p. in the post-maternity leave period. In contrast, we find no economically meaningful employment effects for most groups of fathers.[18] Overall, this evidence shows that multiple comparison groups sharing key characteristics with female judges along different dimensions experience substantial employment losses when working in the private sector.

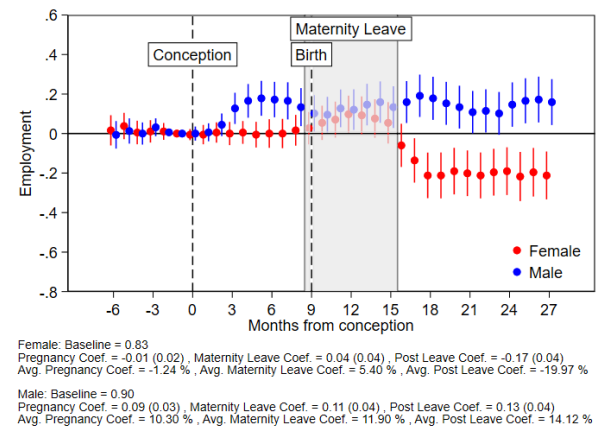
Taken together, these findings indicate that judges are not an unusually selected group with exceptional preferences or abilities. Workers who resemble judges experience sizable child employment penalties when employed in the private sector, while most public sector workers do not. The negligible employment penalties observed among judges therefore appear to be a consequence of the institutional features of their jobs – in particular the strong job security offered by permanent public sector positions – rather than unique preferences or individual characteristics.

[17]The analyses are conducted separately for each comparison group. Within each group, treated individuals are matched to controls based on six baseline characteristics: (i) gender, (ii) municipality of work, (iii) year of birth, (iv) presence in RAIS in the year before childbirth, (v) registration in CadÚnico in that same year, and (vi) whether they hold a college degree. As an exception, control units for judges' relatives are drawn from the population of private sector workers – rather than their own group – since their sample size is reduced. For this group, we also match on (vii) occupation. We then estimate the difference-in-differences models described in Equation 1 and Equation 2.

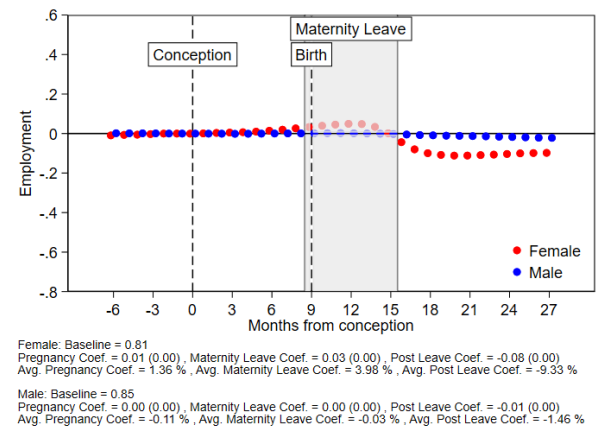
[18]Judges' male siblings and cousins are an exception and experience employment gains after childbirth.

Figure 7: Effect of parenthood on employment of judge-comparable private workers

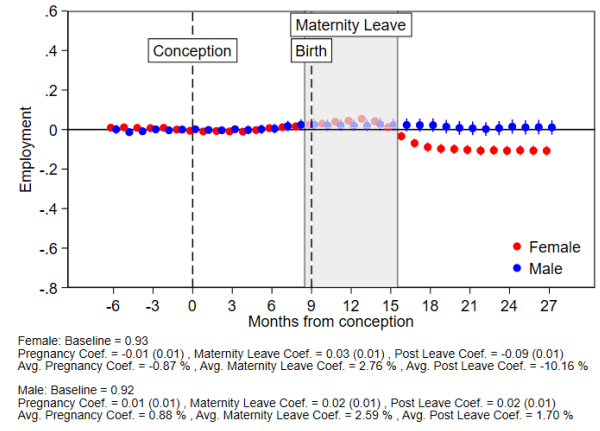
(a) Sibling and Cousin



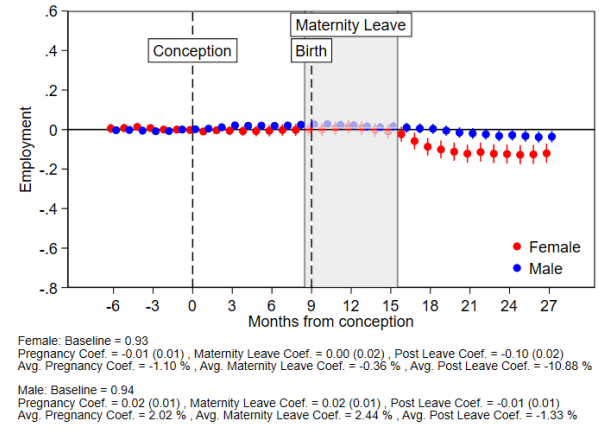
(b) Worker with College



(c) Lawyer



(d) Top Earner



Notes: This figure shows the effect of parenthood on employment among private sector workers comparable to judges, using RAIS data and the difference-in-differences model described in Equation 1 - along with 95% confidence intervals. Panel (a) shows the effects on the probability of being employed for private sector workers who are siblings or cousins of judges. Panel (b) presents estimates for workers with a college degree. Panel (c) focuses on lawyers in the private sector. Panel (d) reports effects for top earners—defined as private sector workers earning at least R$25,000 (the initial wage of a judge in Brazil) in the year we selected them in RAIS. Earnings are measured in Brazilian Reais at 2015 price levels. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

5 Discussion

We have shown that Brazilian judges do not experience significant reductions in output following the birth of their first child. If anything, some results point to improvements along certain margins for mothers, such as a reduction in average time to case resolution and a decline in the share of cases appealed and reversed in higher courts. In addition, we document that several other dimensions of work performance remain unchanged – for example, judges do not appear to issue systematically different rulings after childbirth.

A main contribution relative to recent work is the ability to isolate the effects of childbirth on worker output from selection and environmental factors. First, we analyze output for the same workers before and after childbirth, eliminating selection into continued employment. Second, we study output in an institutional context where external influences play a minimal role in shaping productivity. In particular, our estimates are not confounded by differential task assignment, career opportunities, or access to resources that could change with parenthood – for example, due to discretionary decisions by supervisors, firms, or peers. We now turn to broader implications of these findings.

A first takeaway is that on-the-job output declines may not be a necessary feature of mothers' experience after childbirth, especially in cognitively intensive occupations such as ours. It may be the case that some mothers who remain in the labor market may experience slower career progression not because they are inherently less productive, but because they are not given the assignments, flexibility, or support needed to sustain output. This interpretation is consistent with previous work documenting declines in output or performance among mothers in settings where task assignment and evaluation are more discretionary, such as low-skill jobs in the US military (Healy and Heissel, 2022) and physicians in Brazil (Branco et al., 2025), or dependent on peer interactions, such as researchers in academia (Cairo et al., 2025; Morgan et al., 2021). More broadly, these patterns shift attention toward mechanisms that operate through workplace responses and career structures – such as discriminatory promotion practices (Ginther and Kahn, 2004; Bronson and Thoursie, 2019; Di Giovan Paolo and Marcolin, 2025) and biased evaluations (Saygin and Knight, 2023; Anne, 2017), or social norms that shape career trajectories independent of worker output (Farré et al., 2023; Kleven, 2022).

A second implication of our findings is that biological factors associated with childbirth do not necessarily imply a sustained reduction in mothers' ability to remain productive. A large medical literature documents physiological changes following childbirth (Feldman et al., 2007; Hoekzema et al., 2017; Numan and Insel, 2003), and these changes have been proposed as potential drivers of the child penalty (Kleven et al., 2021). Our results show that, at least in some contexts, mothers are able to sustain output after childbirth despite these changes. This holds even in the presence of gender norms that likely lead to an unequal allocation of domestic and childcare responsibilities. Naturally, our evidence is most informative for contexts similar to ours – high-skill, cognitively intensive jobs characterized by substantial autonomy, flexibility, and a managerial component.

Our findings complement recent evidence suggesting that child penalties in *employment* outcomes cannot be explained by biological mechanisms alone. For example, Kleven et al. (2021) show that biological and non-biological mothers experience similar labor market

penalties at first birth, while Andresen and Nix (2022) document comparable penalties among same-sex female couples regardless of which partner gives birth. These studies highlight the role of gender norms in shaping labor market outcomes. A key difference in our contribution is that we directly examine mothers' work output. Our results imply that gender norms and institutional responses may generate child penalties even when mothers retain the capacity to remain productive. In other words, norms and workplace structures may lead mothers to reduce labor market attachment independent of their underlying productivity potential.

5.1 Layoffs versus job quits as drivers of employment child penalties

We conclude by providing evidence that demand factors may play an important role in explaining child penalties in mothers' labor market trajectories. We study how childbirth affects the probability of firm-initiated separations – layoffs – and voluntary job quits, as recorded in our employment data.[19] Specifically, we replicate the analysis studying child penalties in employment for women working in the private sectors – Section 4.6.2. If employment penalties were fully driven by mothers who choose to withdraw from the labor force – e.g., due to gender norms in the family –, one would expect to observe mothers voluntarily quitting their jobs upon childbirth. Instead, demand driven factors would be consistent with firm laying off mothers after childbirth.

The results in Figure 8 show that child penalties in employment are mainly driven by firm-initiated layoffs rather than voluntary job quits. First, layoffs for mothers decline during the pregnancy and maternity leave periods, which is explained by the fact that labor law prohibits firm-initiated separations during these period.[20] Second, the cumulative probability of layoffs increases in the post-leave period. On average, monthly layoffs probability increase by 2.47 p.p. in the full sample of private workers (Panel a) and by 1.21 p.p. for workers with a college degree (Panel c). At the same time, voluntary quits decline by 1.10 p.p. and 2.01 p.p. for the same groups, respectively. The positive effects on *cumulative* layoff probability indicates that firm-initiated separations largely increase in the post-leave period, to the extent that it more than compensates the law-driven reduction in layoffs during the pregnancy and maternity leave periods.

In Appendix Figure C.1, we complement this evidence by showing that there are large spikes in the distribution of layoff probabilities when mothers return to work after the maternity

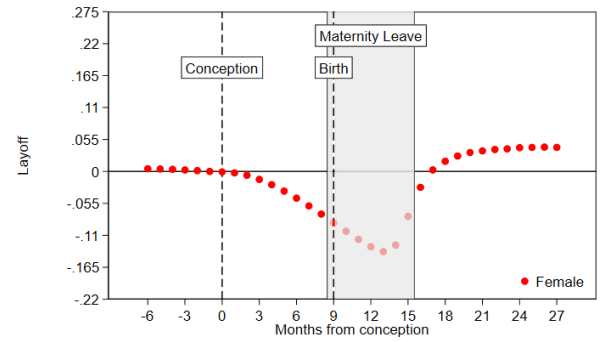
[19] Private firms in Brazil are free terminate employment without a just cause upon the payment of mandatory severance pay (about 1.36 monthly earnings per tenure year). Layoffs roughly account for two thirds of terminations and while the other one third are voluntary quits (Britto et al., 2022).

[20] More precisely, mothers cannot be displaced during pregnancy and up to five months after birth. The latter overlaps with the statutory four-month maternity leave, which can be extended by two months for firms enrolled in a leave-extension government program (Britto et al., 2025b).

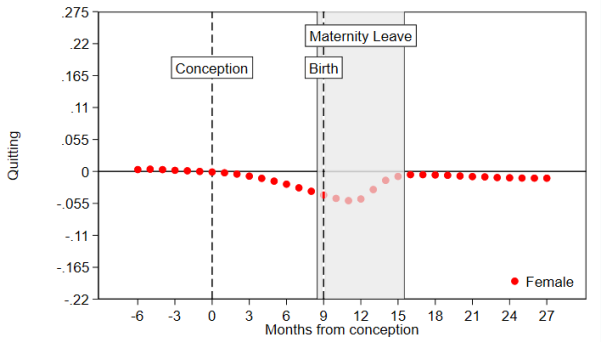
leave period – taking place four, five, and six months after childbirth.[21] We interpret these results as indication that demand factors likely play a relevant role in explaining child penalties in employment in the private sector.

Figure 8: Effect of parenthood on cumulative layoff and quit probabilities, private sector mothers

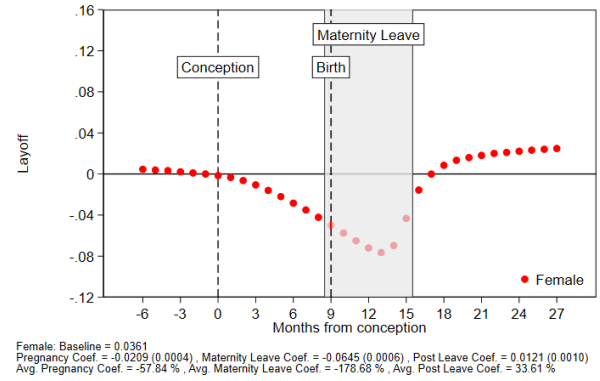
(a) Layoff, all workers



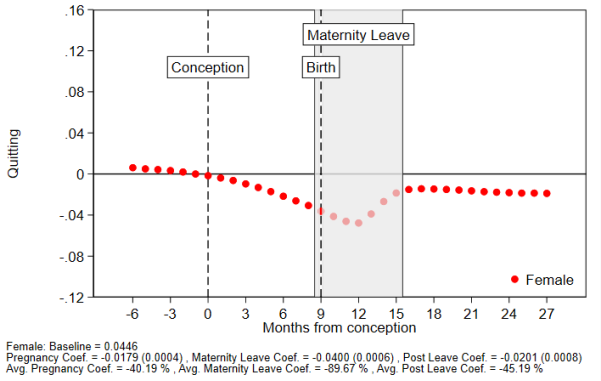
(b) Quit, all formal workers



(c) Layoff, Workers with College



(d) Quit, Workers with College



Notes: This figure shows the *absolute* effect of parenthood on the cumulative probability of firm-initiated layoffs and voluntary job quits for private sector workers, using RAIS data and the difference-in-differences model described in Equation 1 – along with 95% confidence intervals. Panels (a) and (b) show the effects on the cumulative probability of layoff and voluntary quit, respectively, for all formal private sector workers. Panels (c) and (d) report the same outcomes for workers with a college degree. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

6 Conclusion

This paper asks whether mothers can sustain their observed output at work after childbirth. We study Brazilian judges, a setting in which output is precisely measured, caseloads are evenly

[21] Returning dates vary because mothers in the private sector can choose to initiate the leave period up to one month before birth and because the leave duration varies between four to six months. Layoff patterns are also affected by a one-month advance layoff notice period, mandated by law.

allocated, and virtually all workers return to their jobs after childbirth. Using administrative data that link monthly output measures to the timing of first births, we find that mothers experience a short-lived decline in output during statutory leave, followed by a rapid return to pre-birth levels once they resume work. Fathers' output is also largely unchanged. These patterns extend beyond our main measure of sentences issued: other margins of performance remain stable, including settlements, hearings, and case-level indicators of timeliness and quality. Consistent with the institutional design of the judiciary, we also find no meaningful changes in employment, earnings, or promotion trajectories around childbirth.

Taken together, the evidence indicates that parenthood need not generate reductions in mothers' observed output after they return from statutory leave, at least in cognitively intensive occupations with strong employment protection, high autonomy over how work is organized, and near-universal retention after childbirth. Within similar settings, the results narrow the set of plausible explanations for the child penalties documented in the broader labor market, away from a mechanical decline in mothers' ability to produce output and toward forces that operate through job structure and labor market responses. Comparative evidence reinforces this interpretation: we find similar output dynamics for self-employed labor lawyers, and we show that the absence of employment penalties is a broader feature of protected public sector jobs, while women with judge-like traits experience sizable employment losses in the private sector. Our findings are consistent with the view that occupational and institutional conditions shape the extent to which parenthood translates into observed output losses, and they point to employment protection and workload allocation as dimensions worth examining in future work on how the child penalty emerges across different professional settings.

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Online Appendix to "Parenthood and Productivity"

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April 29, 2026

A Appendix to Section 3

A.1 Family Links

We construct parent–child links in multiple rounds, combining different datasets and linkage strategies to associate parents and children through their unique CPF identifiers.

We begin with mothers. In the first round, we use dependent claims in personal income tax records for 2006–2020, provided by the Brazilian Tax Authority, which directly link parents and children via CPF identifiers. In the second round, we exploit the mother's name available in the *Cadastro de Pessoas Físicas*, a person registry covering the entire population. We identify mothers' CPFs based on their names when these are unique in the population – a condition satisfied by roughly 50% of individuals, as Brazilians typically carry multiple surnames. Importantly, Britto et al. (2022, 2025a) show that uniquely named individuals do not strongly from the overall Brazilian population along several characteristics. In the third round, we extend this approach by identifying mothers whose names are unique within their postal code of residence, using address information from the same registry. In the fourth round, we use mothers' names from CadÚnico (2011–2019), a welfare registry covering about two-thirds of the population, again restricting to unique names. In the fifth round, we apply the same procedure using the 2014 School Census, which provides information on mothers' names for all students enrolled in Brazilian schools. Finally, we use household composition in CadÚnico to link remaining children to their mothers.

We follow an analogous procedure for fathers, except that we omit the steps based on the person registry, as fathers' names are not recorded in these data. Combining these strategies, we identify mothers and fathers for 70.61% and 50.97% of children born in Brazil between 2015 and 2020, respectively. This process yields a comprehensive dataset of family links that we use in our supplementary analyses.

B Appendix to Section 4

B.1 Descriptive statistics: treatment vs. control judges in the main sample

Table B.1: Descriptive statistics: main analysis sample

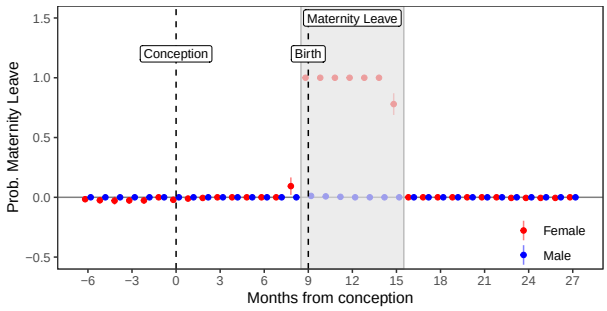
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Male					Female				
	Control		Treated			Control		Treated		
	Mean	SD	Mean	SD	Std Diff	Mean	SD	Mean	SD	Std Diff
Employed	1.00	0.00	1.00	0.00	0.00	1.00	0.00	1.00	0.00	0.00
Earnings	28972.44	2466.26	29104.40	2712.09	-0.05	28409.36	1996.34	28524.19	2174.71	-0.06
Wage	25902.19	5771.07	25709.35	6733.60	0.03	24951.96	6834.40	24755.05	7251.01	0.03
Sentences Issued	121.66	133.17	122.83	121.90	-0.01	126.73	140.11	119.17	130.33	0.06
Work in State Capital	0.96	0.20	0.98	0.16	-0.09	0.98	0.14	0.94	0.23	0.19
Work in Metro Region	0.97	0.17	0.97	0.18	0.01	1.00	0.00	1.00	0.00	0.00
Years of Schooling	16.22	0.76	16.16	0.61	0.10	16.07	0.38	16.06	0.35	0.03
Year of Birth	1976.94	5.39	1975.86	6.39	0.18	1980.16	3.12	1978.81	4.04	0.37
Year of Hiring	2007.10	5.73	2005.62	6.76	0.24	2009.37	3.60	2007.59	4.75	0.42
White	0.41	0.49	0.54	0.50	-0.26	0.31	0.46	0.41	0.49	-0.21
Observations	292		292			196		196		

Notes: This table reports descriptive statistics for the sample used in the main analysis, after applying the matching procedure. Columns show the mean, standard deviation, and standardized differences by treatment status and gender. All variables are measured at month $t = -1$, the month immediately preceding conception. All monetary variables are in Brazilian Reais at 2015 price levels.

B.2 Effect of parenthood on judge output: additional outcomes

Figure B.1: Effect of parenthood on work leaves

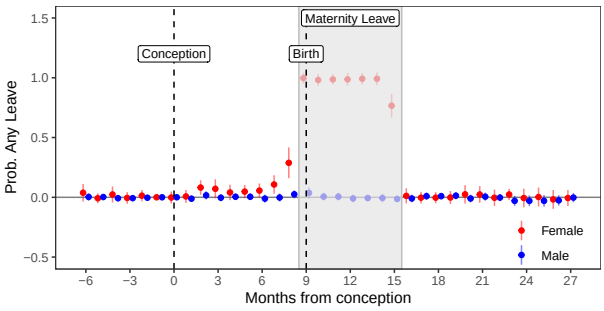
(a) Prob. of maternity leave



Female: Baseline = 0.00
Pregnancy Coef. = 0.03 (0.01) , Maternity Leave Coef. = 0.99 (0.01) , Post Leave Coef. = 0.02 (0.01)
Avg. Pregnancy Coef. = Inf % , Avg. Maternity Leave Coef. = Inf % , Avg. Post Leave Coef. = Inf %

Male: Baseline = 0.00
Pregnancy Coef. = 0.00 (0.00) , Maternity Leave Coef. = 0.00 (0.00) , Post Leave Coef. = 0.00 (0.00)
Avg. Pregnancy Coef. = Inf % , Avg. Maternity Leave Coef. = Inf % , Avg. Post Leave Coef. = Inf %

(b) Prob. of any leave



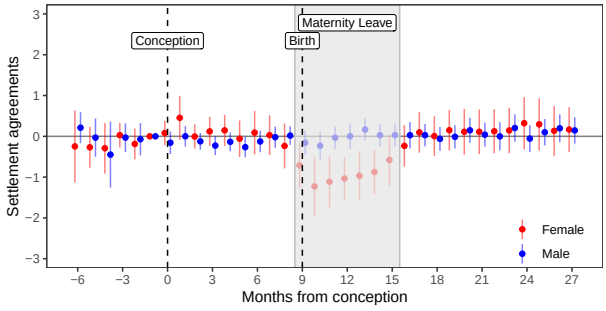
Female: Baseline = 0.04
Pregnancy Coef. = 0.07 (0.02) , Maternity Leave Coef. = 0.95 (0.02) , Post Leave Coef. = −0.01 (0.02)
Avg. Pregnancy Coef. = 184.90 % , Avg. Maternity Leave Coef. = 2584.84 % , Avg. Post Leave Coef. = −16.81 %

Male: Baseline = 0.02
Pregnancy Coef. = 0.00 (0.01) , Maternity Leave Coef. = 0.00 (0.01) , Post Leave Coef. = −0.01 (0.01)
Avg. Pregnancy Coef. = 20.58 % , Avg. Maternity Leave Coef. = 17.29 % , Avg. Post Leave Coef. = −24.98 %

Notes: This figure shows the effect of parenthood on work leaves based on RAIS data: panel (a) for maternity leave and (b) for any leaves. We exclude from the analysis mothers for whom no maternity leave period is recorded in the RAIS data – this likely reflects measurement error in the employment records, as maternity leave is mandatory by law. All effects are estimated using the difference-in-differences model in Equation 1, with 95% confidence intervals. The baselines, the average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), as well as the relative effects during these periods, are also reported at the bottom of each graph.

Figure B.2: Effect of parenthood on judge output: hearings and settlement agreements

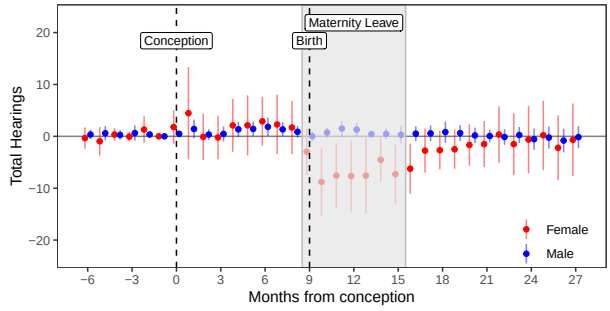
(a) Settlement agreements



Female: Baseline = 63.02
Pregnancy Coef. = 14.26 (11.30) , Maternity Leave Coef. = −46.39 (13.17) , Post Leave Coef. = 17.59 (14.08)
Avg. Pregnancy Coef. = 22.63 % , Avg. Maternity Leave Coef. = −73.61 % , Avg. Post Leave Coef. = 27.91 %

Male: Baseline = 72.74
Pregnancy Coef. = −3.35 (9.91) , Maternity Leave Coef. = 2.96 (15.09) , Post Leave Coef. = 9.58 (15.47)
Avg. Pregnancy Coef. = −4.60 % , Avg. Maternity Leave Coef. = 4.07 % , Avg. Post Leave Coef. = 13.17 %

(b) Hearings



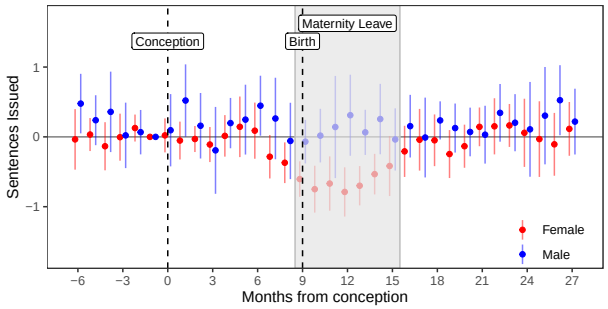
Female: Baseline = 5.78
Pregnancy Coef. = 10.56 (11.41) , Maternity Leave Coef. = −37.04 (14.14) , Post Leave Coef. = −10.64 (13.62)
Avg. Pregnancy Coef. = 182.77 % , Avg. Maternity Leave Coef. = −641.20 % , Avg. Post Leave Coef. = −184.15 %

Male: Baseline = 15.71
Pregnancy Coef. = 10.95 (7.50) , Maternity Leave Coef. = 5.03 (10.54) , Post Leave Coef. = −4.20 (15.06)
Avg. Pregnancy Coef. = 69.73 % , Avg. Maternity Leave Coef. = 31.99 % , Avg. Post Leave Coef. = −26.75 %

Notes: This figure shows the *absolute* effect of parenthood on additional judge output measures using CNJ data, estimated from the difference-in-differences model in Equation 1, with 95% confidence intervals. Panel (a) reports results for settlement agreements and (b) for hearings. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

Figure B.3: Effect of parenthood on productivity: PJe sample of labor judges

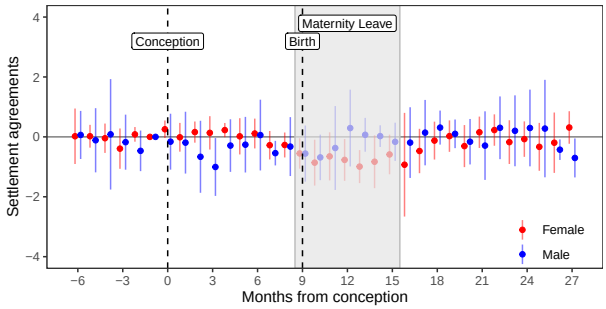
(a) Sentences Issued



Female: Baseline = 56.55
Pregnancy Coef. = −3.68 (4.00) , Maternity Leave Coef. = −36.02 (4.63) , Post Leave Coef. = −0.86 (5.75)
Avg. Pregnancy Coef. = −6.51 % , Avg. Maternity Leave Coef. = −63.70 % , Avg. Post Leave Coef. = −1.51 %

Male: Baseline = 62.18
Pregnancy Coef. = 1.26 (4.72) , Maternity Leave Coef. = −4.04 (4.26) , Post Leave Coef. = 1.69 (5.50)
Avg. Pregnancy Coef. = 2.03 % , Avg. Maternity Leave Coef. = −6.50 % , Avg. Post Leave Coef. = 2.72 %

(b) Settlement agreements



Female: Baseline = 34.56
Pregnancy Coef. = 3.28 (4.04) , Maternity Leave Coef. = −23.47 (8.62) , Post Leave Coef. = −3.20 (6.60)
Avg. Pregnancy Coef. = 9.48 % , Avg. Maternity Leave Coef. = −67.91 % , Avg. Post Leave Coef. = −9.25 %

Male: Baseline = 37.56
Pregnancy Coef. = −8.91 (4.13) , Maternity Leave Coef. = −2.45 (7.88) , Post Leave Coef. = 4.48 (8.27)
Avg. Pregnancy Coef. = −23.73 % , Avg. Maternity Leave Coef. = −6.52 % , Avg. Post Leave Coef. = 11.93 %

Notes: This figure shows the effect of parenthood on judge productivity using the PJe sample of labor judges. The outcomes are the number of sentences issued (Panel a), constructed from PJe time-stamp data capturing all case updates, and settlement agreements (Panel b), constructed from CNJ data. Estimates are obtained from the difference-in-differences model in Equation 1, with 95% confidence intervals. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, reported as the baseline. The average effects during the pregnancy, maternity leave, and post-leave periods—corresponding to coefficients β_1, β_2, and β_3 in Equation 2—as well as the associated relative effects, are reported at the bottom of the graph.

B.2.1 Inference in the labor cases data

To assess whether our inference may be distorted in finite samples, we conduct a simulation-based inference assessment following Ferman (2019). In each gender-specific sample, we keep the original estimation sample, regression specification, fixed effects, and clustering structure unchanged, and replace the observed outcome with an i.i.d. standard normal variable generated at the observation level. We then re-estimate our baseline specification using this simulated outcome and test the null of no effect for the coefficient on the treatment $\times$ post-leave interaction. We repeat this exercise 10,000 times separately for mothers and fathers and compute the empirical rejection frequency at conventional significance levels. Appendix Table B.2 reports the results.

Table B.2: Simulation-based inference assessment

	(1)	(2)
	Empirical rejection rate (%)	
	Female	Male
10% significance level	11.43	12.16
5% significance level	6.20	6.55
1% significance level	1.51	1.50
Observations	2,286	792
Replications	10,000	10,000

Notes: This table reports empirical rejection frequencies from a simulation-based inference assessment conducted separately in the female and male samples. In each replication, we replace the original outcome with an i.i.d. standard normal variable at the observation level and re-estimate the baseline specification using the same fixed effects and clustered inference procedure as in the main analysis. The entries report the share of simulations in which the null hypothesis of no effect for the treatment $\times$ post-leave coefficient is rejected at the indicated nominal significance level.

At the 5% significance level, the empirical rejection rates are 6.20% in the mothers' sample and 6.55% in the fathers' sample. At the 1% level, the rejection rates are 1.51% and 1.50%, respectively. At the 10% level, the rejection rates are 11.43% for mothers and 12.16% for fathers. Overall, the results point to only mild over-rejection, suggesting that finite-sample inference distortions in our setting are limited in magnitude.

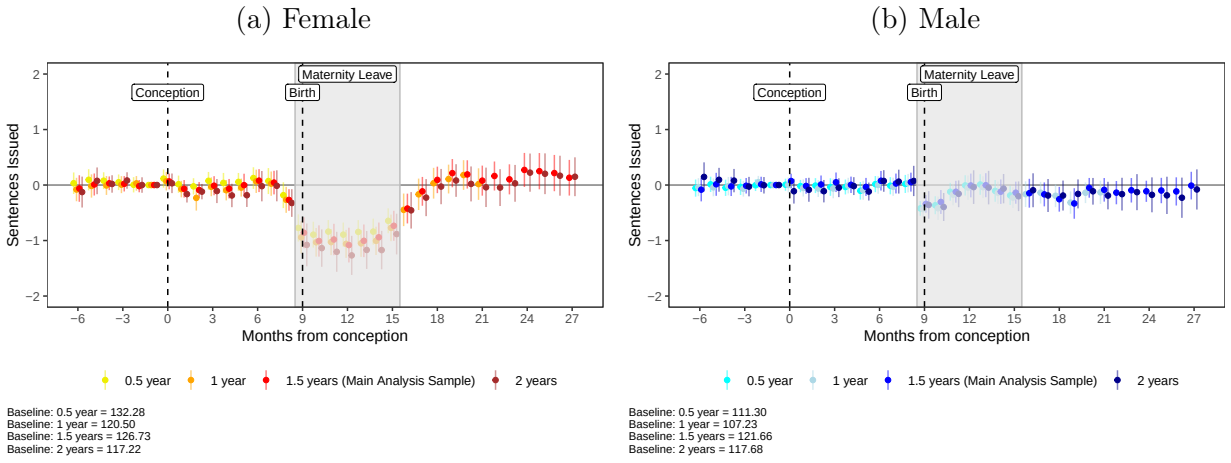
B.3 Robustness

B.3.1 Alternative control groups

A potential threat to identification in our difference-in-differences strategy is that the timing of conception may be correlated with unobserved determinants of productivity. If judges strategically plan fertility based on career considerations, or if underlying factors influence both childbearing and work effort, comparisons between treated and control judges may be biased.

To address this concern, our baseline specification matches treated judges to controls who conceived at least 1.5 years later, ensuring they are not pregnant or undergoing major life transitions during the event window. To test whether our findings hinge on this specific cutoff, we replicate the analysis using three alternative thresholds: 6 months, 1 year, and 2 years after the treated judge's childbirth.[22] These thresholds reflect a trade-off between internal validity and statistical power. Narrower windows improve alignment in life stage but shorten the analysis window, since control judges become parents sooner. Longer thresholds extend the estimation window but may increase the risk of selection on unobservables.

Figure B.4: Effect of parenthood on judge output: varying distances in birth timing between treated and control groups



Notes: This figure shows the effect of parenthood on productivity using the CNJ dataset, as estimated from the difference-in-difference Equation 1 - along with 95% confidence intervals. The treatment group comprises judges who gave birth in 2016 or 2017, while the control groups consist of judges who conceived at least 6 months, 1 year, or 2 years after the treated judge's childbirth. Our main specification—shown here for comparison—uses a threshold of 1.5 years. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline.

As shown in Appendix Figure B.4, the estimated effects remain consistent across all specifications: we continue to observe a temporary productivity dip around childbirth and no

[22]This strategy is also used by Britto et al. (2024) to study the effects of parenthood on criminal behavior, and by Fadlon and Nielsen (2021) in their analysis of health shocks.

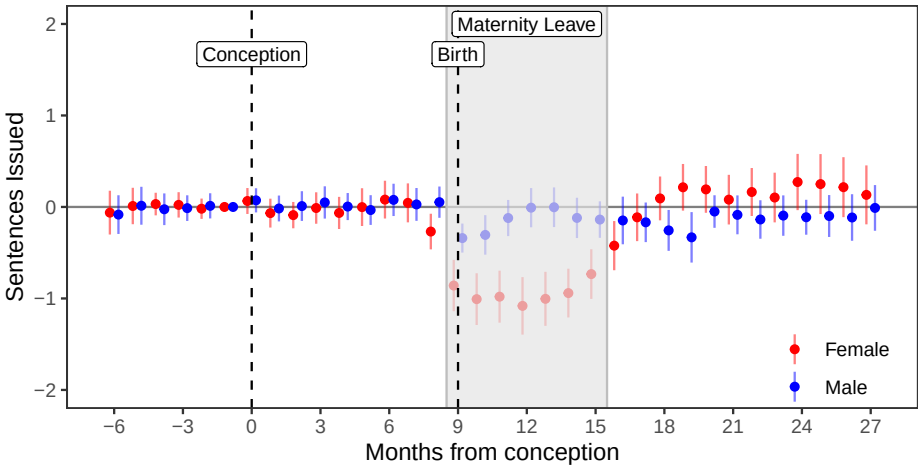
evidence of lasting effects. This robustness supports the view that our results are not driven by endogenous fertility timing. Moreover, this exercise complements our main findings in Section 4.2, where we find no evidence of anticipatory behavior in work outcomes prior to conception. Survey data from Brazil further show that roughly 50% of first-time mothers do not have their first child at their preferred time (Britto et al., 2024). Taken together, these results support treating conception as plausibly quasi-random within short windows and validate the comparability of our matched control group.

B.3.2 Calendar-month fixed effects

A potential concern with our stacked difference-in-differences design is that productivity may vary over calendar time for reasons unrelated to parenthood. For example, aggregate shocks to the judiciary, seasonal changes in court activity, or other time-varying institutional factors could affect judges' output during the sample period. Our baseline specification already includes relative-time fixed effects, which absorb shocks common to treated and control judges within each event month. Nevertheless, we further assess whether our results are sensitive to controlling directly for calendar-time variation.

Appendix Figure B.5 re-estimates our main specification for the number of sentences issued after adding calendar-month fixed effects. The estimated coefficients and standard errors are virtually unchanged relative to the baseline results. This indicates that our main findings are not driven by aggregate calendar-time shocks or by the way time is absorbed in the baseline specification. As expected, because relative-time fixed effects already absorb the main common time variation within each stacked comparison, adding calendar-month fixed effects to the specification produces virtually no changes in the estimates.

Figure B.5: Effect of parenthood on productivity with month calendar fixed effects



Female: Baseline = 126.73
Pregnancy Coef. = −4.27 (6.28) , Maternity Leave Coef. = −119.45 (16.44) , Post Leave Coef. = 12.58 (13.33)
Avg. Pregnancy Coef. = −3.37 % , Avg. Maternity Leave Coef. = −94.25 % , Avg. Post Leave Coef. = 9.93 %

Male: Baseline = 121.66
Pregnancy Coef. = 4.98 (6.65) , Maternity Leave Coef. = −16.26 (9.80) , Post Leave Coef. = −14.61 (11.35)
Avg. Pregnancy Coef. = 4.09 % , Avg. Maternity Leave Coef. = −13.36 % , Avg. Post Leave Coef. = −12.01 %

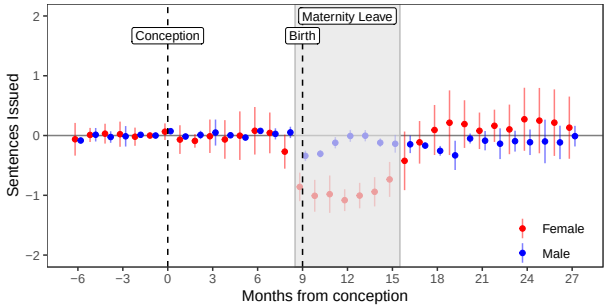
Notes: This figure shows the effect of parenthood on productivity using the CNJ data, as estimated from the difference-in-differences model in Equation 1, augmented with month calendar fixed effects, along with 95% confidence intervals. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2, which is also augmented with month calendar fixed effects), and the relative effects during such periods, are also reported at the bottom of each graph.

B.3.3 Inference

A central econometric concern in our setting is that multiple treated judges are matched to the same control judge, potentially inducing correlation in outcomes across observations that share the same counterfactual. If standard errors are clustered at the individual level, this correlation is ignored, leading to underestimated standard errors and overstated statistical significance. Following De Chaisemartin and Ramirez-Cuellar (2024), we address this concern by clustering at the control judge level, allowing for arbitrary correlation in residuals across all treated judges linked to the same control. To assess the robustness of our inference to alternative assumptions about the error structure, we compare our preferred clustering approach to four alternatives: (i) clustering at the judicial branch level (i.e., State Justice, Labor Justice, and Federal Justice); (ii) clustering at the courthouse level; (iii) clustering at the parent fixed effects level, which allows for serial correlation within each judge over time but not across matched units; and (iv) heteroskedasticity-robust standard errors. As shown in Appendix Figure B.6, confidence intervals differ only slightly across specifications, indicating that our conclusions are not driven by any particular choice of standard error correction.

Figure B.6: Effect of parenthood on productivity using alternative standard error specifications

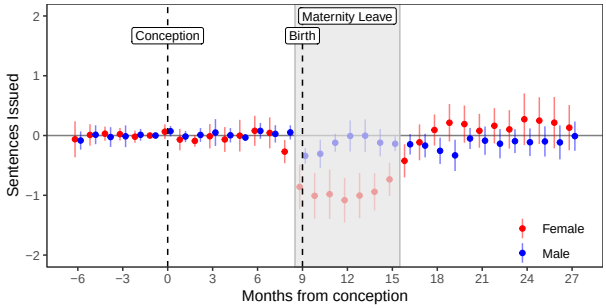
(a) Clustered at judicial branch level



Female: Baseline = 126.73
Pregnancy Coef. = −4.28 (10.46) , Maternity Leave Coef. = −119.46 (8.35) , Post Leave Coef. = 12.57 (19.70)
Avg. Pregnancy Coef. = −3.38 % , Avg. Maternity Leave Coef. = −94.26 % , Avg. Post Leave Coef. = 9.92 %

Male: Baseline = 121.66
Pregnancy Coef. = 4.98 (5.03) , Maternity Leave Coef. = −16.25 (3.87) , Post Leave Coef. = −14.60 (5.30)
Avg. Pregnancy Coef. = 4.09 % , Avg. Maternity Leave Coef. = −13.36 % , Avg. Post Leave Coef. = −12.00 %

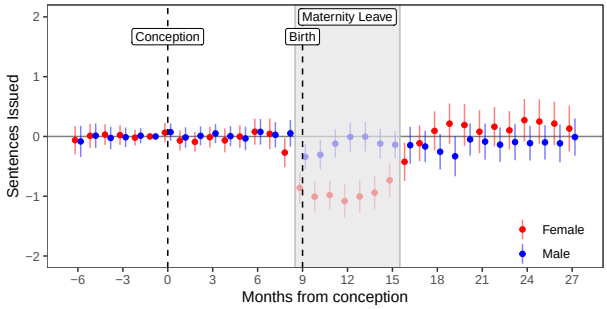
(b) Clustered at courthouse level



Female: Baseline = 126.73
Pregnancy Coef. = −4.28 (5.91) , Maternity Leave Coef. = −119.46 (22.90) , Post Leave Coef. = 12.57 (14.51)
Avg. Pregnancy Coef. = −3.38 % , Avg. Maternity Leave Coef. = −94.26 % , Avg. Post Leave Coef. = 9.92 %

Male: Baseline = 121.66
Pregnancy Coef. = 4.98 (7.64) , Maternity Leave Coef. = −16.25 (9.97) , Post Leave Coef. = −14.60 (12.12)
Avg. Pregnancy Coef. = 4.09 % , Avg. Maternity Leave Coef. = −13.36 % , Avg. Post Leave Coef. = −12.00 %

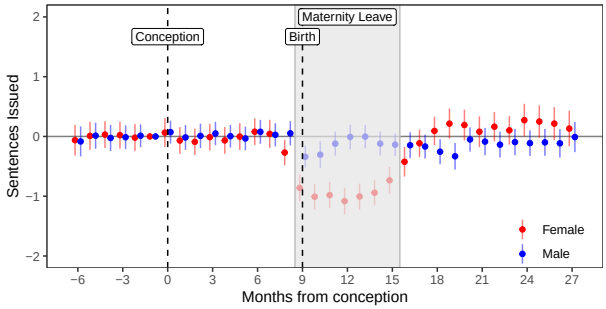
(c) Clustered at parent level



Female: Baseline = 126.73
Pregnancy Coef. = −4.28 (8.53) , Maternity Leave Coef. = −119.46 (15.32) , Post Leave Coef. = 12.57 (17.28)
Avg. Pregnancy Coef. = −3.38 % , Avg. Maternity Leave Coef. = −94.26 % , Avg. Post Leave Coef. = 9.92 %

Male: Baseline = 121.66
Pregnancy Coef. = 4.98 (8.75) , Maternity Leave Coef. = −16.25 (12.55) , Post Leave Coef. = −14.60 (15.65)
Avg. Pregnancy Coef. = 4.09 % , Avg. Maternity Leave Coef. = −13.36 % , Avg. Post Leave Coef. = −12.00 %

(d) Robust to heteroscedasticity



Female: Baseline = 126.73
Pregnancy Coef. = −4.28 (5.64) , Maternity Leave Coef. = −119.46 (5.79) , Post Leave Coef. = 12.57 (5.86)
Avg. Pregnancy Coef. = −3.38 % , Avg. Maternity Leave Coef. = −94.26 % , Avg. Post Leave Coef. = 9.92 %

Male: Baseline = 121.66
Pregnancy Coef. = 4.98 (5.04) , Maternity Leave Coef. = −16.25 (5.30) , Post Leave Coef. = −14.60 (5.14)
Avg. Pregnancy Coef. = 4.09 % , Avg. Maternity Leave Coef. = −13.36 % , Avg. Post Leave Coef. = −12.00 %

Notes: This figure shows the effect of parenthood on productivity using the CNJ dataset, as estimated from the difference-in-difference Equation 1 - along with 95% confidence intervals. Panel (a) clusters standard errors at the judicial branch level, assuming that errors may be correlated within each branch of the judiciary (i.e., State Justice, Labor Justice, and Federal Justice). Panel (b) clusters at the courthouse level, allowing errors to be correlated within each courthouse over time. Panel (c) clusters at the parent fixed effects level, assuming that errors are correlated within each parent cluster across time. Panel (d) reports standard errors robust to heteroskedasticity (White correction). All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

B.3.4 Additional results: parenthood and lawyers' output

Table B.3: Effect of parenthood on self-employed lawyers' productivity

	(1)	(2)	(3)	(4)	(5)	(6)
	New Case	Won Case	Sentence amount	Appeal	Appeal Granted	Duration
			Panel A: Female			
Treat x Post Leave	-0.1733	-0.0921	70.8982**	0.0084	-0.0004	-27.4678
	(0.1310)	(0.0826)	(35.0912)	(0.0163)	(0.0109)	(19.8574)
Baseline	2.1427	1.1322	482.2507	0.2807	0.1450	248.6779
Observations	51,078	51,078	34,811	38,308	38,308	38,308
			Panel B: Male			
Treat x Post Leave	-0.0852	-0.3996**	-4.8564	-0.0089	-0.0144	6.2872
	(0.3356)	(0.1875)	(60.7501)	(0.0219)	(0.0160)	(20.0462)
Baseline	3.4879	1.7657	585.9578	0.2545	0.1603	225.0023
Observations	32,979	32,979	23,454	25,645	25,645	25,641

Notes: This table reports the effect of parenthood on measures of self-employed lawyers' performance using the PJe dataset, estimated from the difference-in-differences model described in Equation 2. The outcomes include: number of new cases (Column 1), number of cases won (Column 2), sentence amount (Column 3), the likelihood that a sentence is subsequently reviewed by an appeals court (Column 4), probability that the appeal is granted (Column 5), and case duration (in days) until a case is resolved (Column 6). Panels A and B present results for female and male self-employed lawyers, respectively. Standard errors are reported in parentheses and are clustered at the control self-employed lawyer level. The row labeled Baseline reports the average value of the outcome in the control group at $t = -1$. The number of observations is reported at the bottom of each panel. Significance levels: *** p≤0.01, ** p≤0.05, * p≤0.1.

Table B.4: Effect of parenthood on case workload characteristics: self-employed lawyers

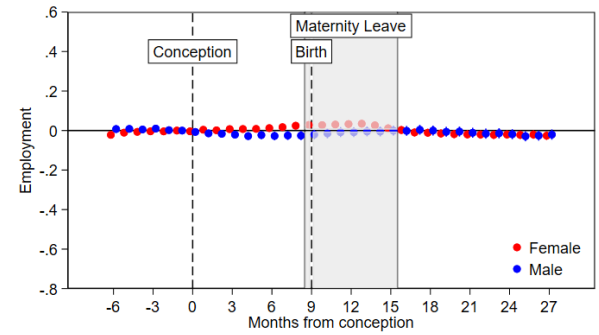
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Initial value	# defendants	# legal issues	Worker registration	Social contrib.	Working hours	Mandated benefits	Severance payments	Unemployment insurance	Other sep. issues	Civil prosecution
						Panel A: Female					
Treat x Post Leave	4,728	0.0210	0.0373	-0.0090	-0.0195	0.0300*	-0.0076	-0.0054	-0.0075	-0.0012	0.0004
	(3,103)	(0.0413)	(0.2357)	(0.0150)	(0.0157)	(0.0177)	(0.0180)	(0.0170)	(0.0123)	(0.0133)	(0.0182)
Baseline	56681	1.5148	6.8450	0.1998	0.2776	0.5425	0.5097	0.5909	0.1396	0.2071	0.3363
Observations	38,308	38,308	38,308	38,308	38,308	38,308	38,308	38,308	38,308	38,308	38,308
						Panel B: Male					
Treat x Post Leave	3,137	-0.0467	0.0302	0.0037	0.0131	-0.0036	0.0115	-0.0159	0.0132	0.0074	-0.0020
	(4,881)	(0.0549)	(0.2826)	(0.0156)	(0.0221)	(0.0253)	(0.0245)	(0.0253)	(0.0182)	(0.0175)	(0.0200)
Baseline	56623	1.4604	5.9276	0.1682	0.2280	0.5081	0.4535	0.5484	0.1254	0.1708	0.3060
Observations	25,645	25,645	25,645	25,645	25,645	25,645	25,645	25,645	25,645	25,645	25,645

Notes: This table reports the effect of parenthood on selected case characteristics and on the probability that the main issue in a case falls into one of eight aggregated topic categories, using the PJe dataset and the classification in Britto et al. (2023). Estimates are obtained from the difference-in-differences model in Equation 2. Columns 1 to 3 report effects on the initial monetary value claimed by the plaintiff, the number of defendants, and the number of legal issues in the case. Columns 4 to 11 report effects on the probability that the main issue in a case concerns worker registration status, social contributions, working hours, mandated benefits, severance payments, unemployment insurance, other separation-related issues, or civil prosecution. Panels A and B present results for female and male self-employed lawyers, respectively. Standard errors are reported in parentheses and are clustered at the control self-employed lawyer level. The row labeled Baseline reports the average value of the outcome in the control group at $t = -1$. The number of observations is reported at the bottom of each panel. Significance levels: *** p≤0.01, ** p≤0.05, * p≤0.1.

B.3.5 Public Sector Workers

Figure B.7: Effect of parenthood on public sector worker's employment by baseline earnings

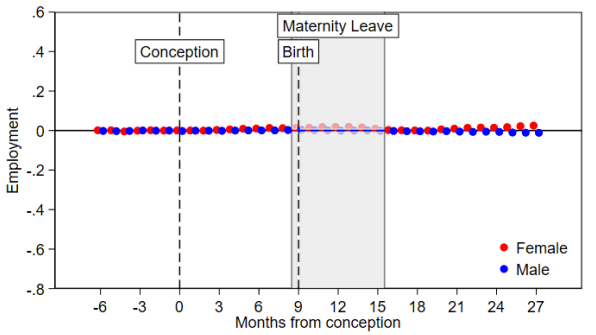
(a) 1st Quartile



Female: Baseline = 0.82
Pregnancy Coef. = 0.01 (0.01) , Maternity Leave Coef. = 0.03 (0.01) , Post Leave Coef. = -0.01 (0.01)
Avg. Pregnancy Coef. = 1.67 % , Avg. Maternity Leave Coef. = 3.53 % , Avg. Post Leave Coef. = -0.88 %

Male: Baseline = 0.85
Pregnancy Coef. = -0.02 (0.01) , Maternity Leave Coef. = -0.01 (0.01) , Post Leave Coef. = -0.01 (0.01)
Avg. Pregnancy Coef. = -2.64 % , Avg. Maternity Leave Coef. = -1.43 % , Avg. Post Leave Coef. = -1.76 %

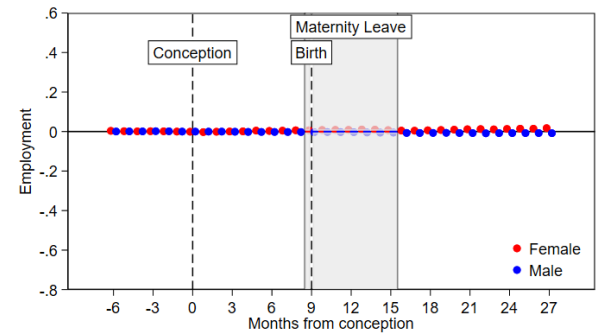
(b) 2nd Quartile



Female: Baseline = 0.72
Pregnancy Coef. = 0.00 (0.00) , Maternity Leave Coef. = 0.01 (0.00) , Post Leave Coef. = 0.01 (0.00)
Avg. Pregnancy Coef. = 0.61 % , Avg. Maternity Leave Coef. = 1.55 % , Avg. Post Leave Coef. = 1.09 %

Male: Baseline = 0.89
Pregnancy Coef. = 0.00 (0.00) , Maternity Leave Coef. = 0.00 (0.00) , Post Leave Coef. = 0.00 (0.00)
Avg. Pregnancy Coef. = 0.12 % , Avg. Maternity Leave Coef. = 0.22 % , Avg. Post Leave Coef. = -0.48 %

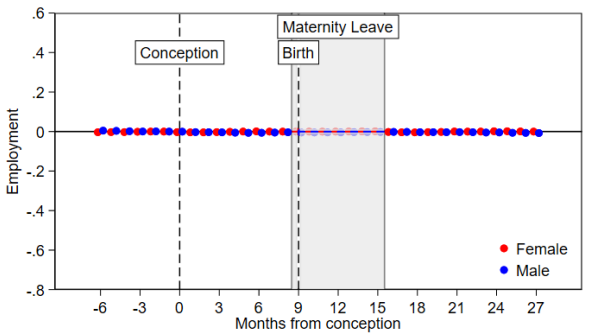
(c) 3rd Quartile



Female: Baseline = 0.80
Pregnancy Coef. = 0.00 (0.00) , Maternity Leave Coef. = 0.01 (0.00) , Post Leave Coef. = 0.01 (0.00)
Avg. Pregnancy Coef. = 0.02 % , Avg. Maternity Leave Coef. = 0.68 % , Avg. Post Leave Coef. = 0.90 %

Male: Baseline = 0.93
Pregnancy Coef. = 0.00 (0.00) , Maternity Leave Coef. = 0.00 (0.00) , Post Leave Coef. = -0.01 (0.00)
Avg. Pregnancy Coef. = -0.22 % , Avg. Maternity Leave Coef. = -0.46 % , Avg. Post Leave Coef. = -0.84 %

(d) 4th Quartile



Female: Baseline = 0.86
Pregnancy Coef. = 0.00 (0.00) , Maternity Leave Coef. = 0.00 (0.00) , Post Leave Coef. = 0.00 (0.00)
Avg. Pregnancy Coef. = 0.01 % , Avg. Maternity Leave Coef. = 0.18 % , Avg. Post Leave Coef. = 0.07 %

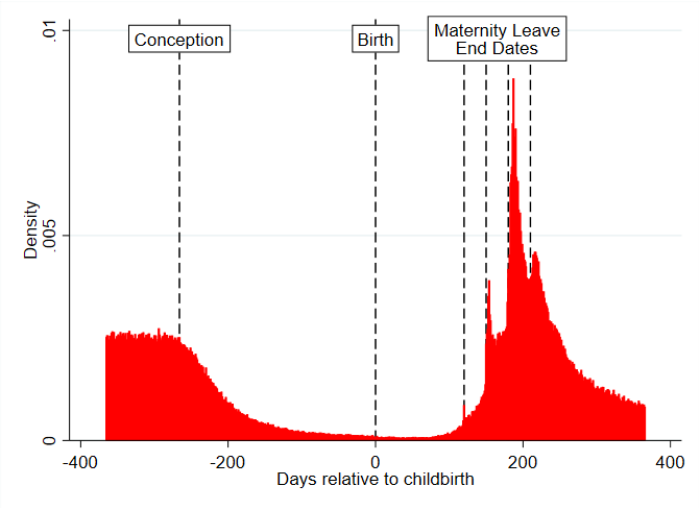
Male: Baseline = 0.89
Pregnancy Coef. = -0.01 (0.00) , Maternity Leave Coef. = 0.00 (0.00) , Post Leave Coef. = -0.01 (0.00)
Avg. Pregnancy Coef. = -0.60 % , Avg. Maternity Leave Coef. = -0.54 % , Avg. Post Leave Coef. = -0.59 %

Notes: This figure shows the effect of parenthood on public sector workers' employment using the RAIS data, as estimated from the difference-in-difference Equation 1 - along with 95% confidence intervals. Panels (a) to (d) present the effects on the probability of being employed, disaggregated by quartiles of the public workers' earnings distribution measured in the year we selected individuals in RAIS. Earnings are measured in Brazilian Reais at 2015 price levels. All coefficients are rescaled by the average value of the outcome in the control group at $t = -1$, which is also reported as Baseline. The average effects during the pregnancy, maternity leave, and post-leave periods (as estimated by coefficients β_1, β_2, and β_3 in Equation 2), and the relative effects during such periods are also reported at the bottom of each graph.

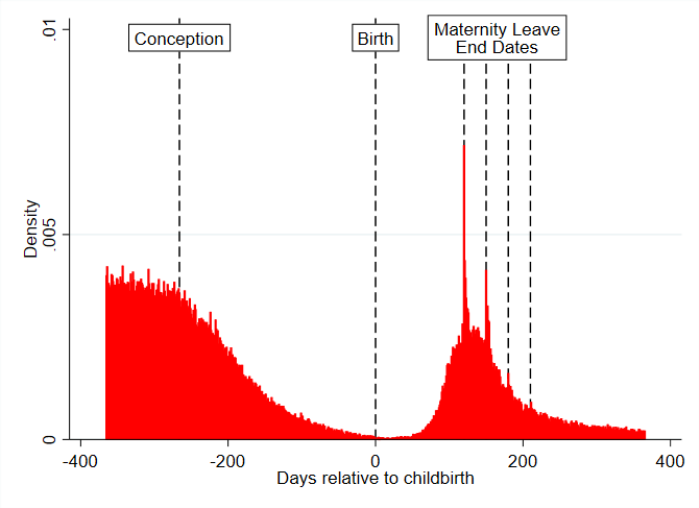
C Appendix to Section 5

Figure C.1: Job separations around childbirth for female workers in Brazil

(a) Layoff



(b) Quit



Notes: This figure shows the timing of job separations for formal-sector female workers around childbirth. We use RAIS data from 2014 to 2019 and restrict the sample to women with registered births. Panel (a) plots the density of firm-initiated separations without just cause (layoffs), while Panel (b) plots worker-initiated separations (quitting). The x-axis shows days relative to the childbirth date, with a bandwidth of 365 days. Vertical lines mark key events: the estimated conception date, childbirth, and the end of maternity leave (4 to 7 months after birth).