NBER WORKING PAPER SERIES

EXPANDING PATERNITY LEAVE: EFFECTS ON BELIEFS, NORMS, AND GENDER GAPS

Henrik Kleven
Camille Landais
Anne Sophie S. Lassen
Philip Rosenbaum
Herdis Steingrimsdottir
Jakob Egholt Søgaard

Working Paper 34862
http://www.nber.org/papers/w34862

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2026, Revised June 2026

We thank seminar and conference participants at the Copenhagen Business School, Columbia University, London School of Economics, the NBER Public Economics Meeting, Princeton University, and the University of Chicago for helpful comments and suggestions. We also thank Niklas Hein and Clara Ylander for outstanding research assistance. Funding from the Carlsberg Foundation (Infrastructure Grant #CF21-0171), the Danish Independent Research Fund (Grant #4750-301280), and the Danish National Research Foundation (Grant #DNRF134) is gratefully acknowledged. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed additional relationships of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w34862

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2026 by Henrik Kleven, Camille Landais, Anne Sophie S. Lassen, Philip Rosenbaum, Herdis Steingrimsdottir, and Jakob Egholt Søgaard. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Expanding Paternity Leave: Effects on Beliefs, Norms, and Gender Gaps
Henrik Kleven, Camille Landais, Anne Sophie S. Lassen, Philip Rosenbaum, Herdis Steingrimsdottir, and Jakob Egholt Søgaard
NBER Working Paper No. 34862
February 2026, Revised June 2026
JEL No. J13

ABSTRACT

We study whether policy can shift gendered beliefs, norms, and labor market outcomes by exploiting a major expansion of earmarked paternity leave in Denmark. The reform generated large first-stage effects, substantially reallocating leave from mothers to fathers. Using a regression discontinuity design combined with new survey data linked to administrative records, we show that the reform makes parents more supportive of paternity leave, shifts gender-role beliefs in a progressive direction, and reduces perceived differences in childcare ability. The reform also narrows gender gaps in earnings and hours worked. The earnings gap falls by 34pp in the first year following childbirth (during leave) and by 2.8pp in the second year (after leave). These results demonstrate that policy can meaningfully influence beliefs, norms, and gender inequality. On the other hand, earmarking restricts families' ability to allocate leave freely and lowers leave satisfaction, highlighting a central trade-off inherent in paternalistic policies.

Henrik Kleven
Princeton University
Department of Economics
and CEPR
and also NBER
kleven@princeton.edu

Camille Landais
London School of Economics and
Political Science (LSE)
Department of Economics
and CEPR
c.landais@lse.ac.uk

Anne Sophie S. Lassen
WZB Berlin Social Science Center
anne.sophie.lassen@wzb.eu

Philip Rosenbaum
Copenhagen Business School
pr.eco@cbs.dk

Herdis Steingrimsdottir
Copenhagen Business School
Department of Economics
hs.eco@cbs.dk

Jakob Egholt Søgaard
University of Copenhagen
and CEBI
jes@econ.ku.dk

1 Introduction

A growing literature highlights the importance of social norms and beliefs for understanding gender inequality in the labor market (Bertrand 2011). Because much of the remaining inequality arises at the onset of parenthood, the role of these factors within families is particularly critical (Bertrand 2020; Kleven *et al.* 2019; Kleven 2025). This raises a central policy question: can gendered beliefs be shifted, and if so, through which mechanisms? The existing evidence offers little guidance. Using quasi-experimental variation and novel data, this paper provides evidence that policy can meaningfully shape beliefs, norms, and downstream gender gaps.

Any study of the impact of policy on beliefs and norms faces two methodological challenges. First, we need policy variation that is both exogenous and sufficiently large to plausibly shift beliefs. Second, we need data that directly measure the beliefs most relevant for parental behavior and provide enough statistical power to detect meaningful changes. We address these challenges by exploiting a major expansion of earmarked paternity leave in Denmark and fielding a new survey on beliefs and norms regarding gender roles and childcare. The survey includes roughly 40,000 parents with newborn children and is linked to rich administrative data.

Earmarking parental leave to fathers—often referred to as "daddy months"—has emerged as a leading policy instrument for shifting norms around childcare and reducing gender gaps. Norway and Sweden were early adopters, introducing such programs in the mid-nineties (Dahl, Løken, and Mogstad 2014; Ekberg, Eriksson, and Friebel 2013), and similar policies have since been introduced in a range of other countries. A recent EU Directive requires member states to provide at least two months of paid leave specifically reserved for fathers. In this context, Denmark expanded earmarked paternity leave from two weeks to eleven weeks for families with children born after August 1, 2022.[1] The additional paternity leave was offset against leave previously available to either parent, thereby reducing the amount of leave available to mothers and inducing a substantial shift in parental leave shares. Because eligibility is determined by a sharp birth-date cutoff, we estimate causal effects using a regression discontinuity (RD) design.

[1]The paternity leave was job-protected and paid, with a replacement rate of 100%.

The reform generated strong first-stage effects on leave duration. Fathers increased their leave by 3.7 weeks on average, while mothers reduced their leave by more than 5 weeks on average.[2] Consequently, the share of leave taken by fathers rose from about 12% to about 21%. This represents substantially larger variation than in prior studies and is important for detecting potential effects on beliefs and norms.[3] We also characterize compliers—those induced by the reform to take at least eleven weeks of paternity leave—and show that they are very similar to the full population of parents with newborn children. This similarity enhances the external validity of our estimates and supports our counterfactual policy exercises.

Having established the first stage, we investigate the effects of the reform on the beliefs and norms elicited in our survey. Three features of the survey design are important for interpreting the results. First, the survey was fielded for a full year before the reform and a full year after, enabling estimation of the effects on survey outcomes using an RD design. To our knowledge, no prior work provides comparably direct, causal evidence on the impact of government policy on beliefs and norms.[4] Second, parents were interviewed twice—when their child was 4 months old and again at 18 months. Because virtually all families take maternity leave first and paternity leave second, the first interview took place before parents experienced the longer paternity leave. The first wave primarily serves to establish a baseline rather than to identify treatment effects, and we therefore focus mostly on the second wave.[5] Finally, the second wave was placed about six months after the completion of government-sponsored leave, allowing us to examine the persistence of treatment effects beyond the duration of the new leave scheme.

[2]These duration changes are smaller than the statutory change of nine weeks for two reasons. First, some fathers took more than two weeks of leave under the pre-reform regime by combining earmarked weeks with leave available to either parent. Second, some fathers take fewer than eleven weeks under the post-reform regime. Such non-takeup reduces total family leave because these weeks are earmarked for fathers and cannot be transferred to mothers.

[3]Prior work on paternity-leave expansions has relied on first stages of at most three weeks—typically less—without any simultaneous leave contraction for mothers (e.g., Ekberg, Eriksson, and Friebel 2013; Dahl, Løken, and Mogstad 2014; Farré and González 2019; Patnaik 2019; Olafsson and Steingrimsdottir 2020; Andresen and Nix 2026). The study by Olafsson and Steingrimsdottir (2020) considered an Icelandic paternity-leave increase almost as large as ours, but without any reduction in leave availability for mothers.

[4]A related literature uses randomized survey experiments to study how policy *information* affects beliefs (e.g., Kuziemko *et al.* 2015; Stantcheva 2021).

[5]In principle, parents could respond in the first wave based on their knowledge of the new policy regime—for example, if they interpret government policy as a signal about appropriate or expected childcare division. Empirically, however, we find limited effects on first-wave outcomes.

We begin by analyzing beliefs about the policy itself. Two main insights emerge. First, parents are generally skeptical about the desirability of earmarked paternity leave. At baseline, more parents oppose the policy than support it, and the survey responses reveal several concerns. Many respondents do not believe that such schemes are important for promoting gender equality or strengthening the bond between fathers and children. Almost all respondents state that parents should be allowed to divide leave freely, and many express concerns that earmarking may reduce the time available for breastfeeding. Taken together, these patterns indicate that parents view the scheme as constraining free choice while offering limited perceived benefits. Second, the reform makes parents more positive about earmarked paternity leave. The RD estimates show positive discontinuities in beliefs about the policy's benefits and negative discontinuities in beliefs about its costs. Treated parents also report that the scheme increases the social and workplace acceptability of paternity leave. Across all outcomes, the effects are sizable and precisely estimated.

In line with the aforementioned concerns, earmarked paternity leave reduces leave satisfaction among both mothers and fathers. Virtually all families were satisfied with their leave division before the reform, but satisfaction drops sharply afterward. Moreover, mothers and fathers report similar reasons for their dissatisfaction: both express that mothers should have more leave, rather than that fathers should have less. These views are consistent with the design of the new scheme. By earmarking part of the pre-existing parental leave to fathers, the reform reduces leave optionality for mothers. Overall, our findings regarding policy beliefs and leave satisfaction highlight a potential welfare cost of paternalistic policies: a reduction in consumer sovereignty. Such policies restrict the ability to choose arrangements that reflect idiosyncratic preferences and constraints, and may therefore reduce welfare, all else equal. These costs must be weighed against the potential benefits of greater gender equality, which we consider below.

We next consider beliefs about gender roles and childcare. The reform has clear and statistically significant effects along these dimensions as well. Consider a question commonly included in value surveys: "Do you agree that pre-school children suffer if their mothers work full time?" The reform reduces agreement with this statement. We find similar effects on other

gender-role questions, with parents shifting toward more gender-progressive views. Regarding childcare, most parents believe that mothers are better at childcare than fathers—though less so as the child ages—but the reform reduces the perceived gap in childcare ability at all ages. Parents also view breastfeeding as important for children's development, yet the reform reduces its perceived importance. These effects are strongest for children aged nine to twelve months, precisely when most fathers take paternity leave. Together, these findings indicate that expanding paternity leave shifts beliefs in ways that could reduce the childcare burden on women and promote gender equality.

Three related results are worth emphasizing. First, we investigate heterogeneity by reform exposure: the reform is non-binding for families who take more than eleven weeks of paternity leave regardless of treatment assignment. Consistent with this, the effects are driven entirely by exposed families, while the effects for unexposed families are precisely estimated zeros. Second, we examine heterogeneity by birth order. The effects are larger for first-time parents, perhaps because such families are less anchored in pre-existing gender roles and therefore more susceptible to shifting their beliefs. This implies that the steady-state effects—as all families eventually experience the new regime from their first child—could exceed the short-run effects we estimate. Third, we provide both reduced-form and IV estimates, using the latter to quantify the effects of counterfactual policy regimes. This exercise is inherently suggestive because treatment effects may not scale proportionally to policies far from those observed. Nevertheless, our results indicate that further extensions of earmarked paternity leave—for example, to a regime of fully equal leave for mothers and fathers—could generate substantial effects.

Finally, we estimate the effects of the paternity-leave reform on gender gaps in earnings and hours worked. We consider gender gaps during the first and second years after childbirth. In year one, the reform reduces the gender gap in earnings by about 34pp and the gender gap in hours by about 33pp. These effects are large but mostly mechanical: they follow directly from the reallocation of leave from mothers to fathers during the child's first year. More importantly, we also find effects in the second year, after the completion of leave for both parents. In year two, the reform reduces the gender gap in earnings by 2.8pp and the gender gap in hours by 1.4pp.

To put these magnitudes in perspective, child penalties in Denmark amount to roughly 20% in earnings and 10% in hours (Kleven, Landais, and Søgaard 2019). Hence, the reform reduces the size of child-related gender gaps by 14%.

Because the scheme does not directly affect incentives or constraints in the second year of the child's life, the year-two effects must operate through other longer-lasting consequences of the reform. A natural candidate is the shift in beliefs about gender roles and childcare we document. Consistent with this mechanism, we provide evidence that the reform shifted the actual division of childcare from mothers to fathers. A mediation analysis suggests that almost all of the gender-gap effects can be explained by the reform-induced changes in beliefs and childcare allocation.

To summarize, this paper studies an increasingly salient policy proposal for addressing gender inequality: earmarking parental leave to fathers. Using novel survey data combined with a quasi-experimental research design, the analysis highlights an important welfare trade-off. On the one hand, we find that earmarked paternity leave meaningfully shifts gendered beliefs and norms, and that it reduces gender gaps in labor market outcomes. Our estimates suggest that such policies, if further expanded, could play a major role in reducing the remaining gender gaps. On the other hand, parents express concerns about earmarking—especially that it restricts their ability to choose—and the policy sharply reduces their leave satisfaction, at least in the short run. As discussed, these effects may be interpreted as a consumer-sovereignty cost of paternalistic policies. Whether the lower leave satisfaction persists over time or attenuates as families adapt to the new regime is an important question for future research.

Our paper contributes to a large literature on gender inequality in the labor market, reviewed by Altonji and Blank (1999), Bertrand (2011), and Olivetti, Pan, and Petrongolo (2024). We build on recent work highlighting the central role of parenthood in shaping gender gaps (Bertrand, Goldin, and Katz 2010; Kleven, Landais, and Søgaard 2019; Kleven *et al.* 2019; Andresen and Nix 2022; Kleven, Landais, and Leite-Mariante 2025; Gallen *et al.* 2025), and on research showing that gender norms and culture influence the effects of parenthood (Bertrand 2020; Boelmann, Raute, and Schönberg 2025; Kleven 2025). While this literature provides important evidence on

the mechanisms behind gender inequality, identifying policy solutions that meaningfully shift outcomes has proven difficult.

A growing literature examines the effects of family policies on labor market outcomes. This includes studies of maternity leave and parental leave available to either parent (e.g., Lalive and Zweimüller 2009; Schönberg and Ludsteck 2014; Dahl *et al.* 2016; Lassen 2023; Kleven *et al.* 2024), as well as a smaller set of studies on paternity leave (Ekberg, Eriksson, and Friebel 2013; Dahl, Løken, and Mogstad 2014; Farré and González 2019; Patnaik 2019; González and Zoabi 2025; Albrecht *et al.* 2026; Andresen and Nix 2026). Research on paternity-leave schemes—some of them earmarked, others not—has consistently found strong effects on fathers' leave take-up, while the results on labor market outcomes are more mixed. Relative to this literature, we study a much larger reform and, most importantly, field a new beliefs survey linked to administrative data to provide causal evidence on both beliefs and labor market outcomes.

We also contribute to the literature on how beliefs, norms, and culture shape gender inequality, as reviewed by Bertrand (2020). Existing research provides evidence on how these factors influence female labor supply (Fernández, Fogli, and Olivetti 2004; Fernández and Fogli 2009) and child penalties (Boelmann, Raute, and Schönberg 2025; Kleven 2025). It offers limited guidance, however, on whether policymakers can shift norms and on the trade-offs involved in doing so.[6] Finally, a burgeoning literature uses surveys to elicit policy beliefs and conducts online experiments to shift those beliefs (Kuziemko *et al.* 2015; Alesina, Stantcheva, and Teso 2018; Stantcheva 2021; Settele 2022; Alesina, Ferroni, and Stantcheva 2024; Dechezleprêtre *et al.* 2025). While informational treatments have been shown to shift policy beliefs in online settings, concerns remain that these effects are sensitive to framing and may not be long-lasting. Our study complements this work by documenting persistent belief changes in response to an actual policy implemented at the national scale.

The paper proceeds as follows. Section 2 provides institutional background, and Section 3

[6]Broadly related, we have evidence suggesting that government legislation and school interventions can shift attitudinal outcomes in other contexts. This includes evidence from a headscarf ban in France (Abdelgadir and Fouka 2020), a textbook reform in China (Cantoni *et al.* 2017), classroom interventions in India (Dhar, Jain, and Jayachandran 2022), language restrictions in US elementary schools (Fouka 2020), and the legalization of same-sex marriages and domestic partnerships in European countries (Aksoy *et al.* 2020).

outlines our survey design and data. Section 4 presents the empirical design and the first-stage results. Section 5 reports the main findings on the effects of paternity leave on beliefs and labor market outcomes. Section 6 concludes.

2 Institutional Background

2.1 Danish Parental Leave System

Danish parents are eligible for one year of birth-related leave, divided into 4 weeks of pregnancy leave and 48 weeks of post-birth leave. These leave durations have been constant since 2002. Prior to the 2022 reform studied here, the post-birth leave included 14 weeks of earmarked maternity leave, 2 weeks of earmarked paternity leave, and 32 weeks of transferable parental leave.[7] The transferable leave can be allocated freely between parents and may be taken concurrently. All leave is paid and job-protected. The 2022 reform did not change the total amount of available leave, nor the rules governing benefits or job protection, but instead earmarked a substantial share of the transferable parental leave to fathers.[8] We describe the details of this reform below.

Eligibility for parental leave depends on labor market attachment, defined as having an active employment contract at the start of leave and having worked at least 40 hours per month in the preceding four months.[9] Leave benefits consist of a combination of public transfers and employer-provided wage top-ups. Public transfers replace 100% of pre-birth earnings up to a modest cap, implying an average replacement rate of roughly 50%. Employers cover the gap between these transfers and the worker's former wage during part of the leave period, with the duration of top-ups varying across firms and sectors.[10] As a result, parents typically face a replacement rate of 100% during the first part of leave—around six months for mothers and up

[7] Same-sex couples and adoptive parents are subject to the same rules, with the co-mother assuming the legal role of the father in same-sex couples and legal roles assigned by the parents themselves in adoptive families.

[8] The parental leave rules before and after the 2022 reform can be found at Borger.dk (2022*a*,*b*).

[9] These are the rules for employees, but similar rules apply to the self-employed.

[10] Administratively, when covered by firm-based parental leave benefits, workers receive their full wage directly from the employer, who is reimbursed for the value of the public leave benefits. In addition to these government reimbursements, firms often receive further compensation from employer organizations, leaving their net wage bill largely unaffected by having workers on parental leave (Brenøe *et al.* 2024).

to three months for fathers—and an average replacement rate of about 50% during the remainder of leave.

Finally, it is worth noting that this system—because mothers tend to take much longer leave than fathers—implies that many families leave money on the table: the marginal week of leave for mothers is typically compensated at the public benefit rate, whereas the marginal week for fathers is paid at their full wage (Jørgensen and Søgaard 2024). By itself, this pattern suggests that leave-allocation decisions are shaped by gendered preferences or norms around childcare. By forcing parents to reallocate leave from mothers to fathers through earmarking, the 2022 reform effectively increased total leave benefits for many Danish families.

2.2 EU Paternity Leave Directive and Danish Reform

To promote work-life balance and gender equality in caregiving, the European Union adopted Directive 2019/1158 on June 20, 2019 (European Parliament and the Council of the European Union 2019). In addition to granting fathers at least 10 working days of leave immediately after childbirth, the directive established an individual right for each parent to four months of parental leave, of which at least two months must be earmarked (i.e., non-transferable). Member states were required to transpose these provisions into national law by August 2, 2022, unless their existing rules were already compliant.

The directive was binding in the Danish context because, as described above, the existing rules provided only two weeks of earmarked leave for fathers. To comply, Danish political parties agreed on an amendment to the Parental Leave Act (Beskæftigelsesministeriet 2022). The main contours of the amendment were agreed upon in October 2021, and the reform was signed into law in March 2022 (Beskæftigelsesministeriet 2021).

The reform granted each parent 24 weeks of post-birth leave, of which 11 weeks are earmarked and 13 weeks are transferable between parents. The new regime can therefore be summarized as providing 11 weeks of earmarked maternity leave, 11 weeks of earmarked paternity leave, and 26 weeks of transferable parental leave. Relative to the pre-existing system,

the reform added nine weeks of earmarked leave for fathers while simultaneously reducing leave optionality for mothers by nine weeks (from 46 to 37). All other elements of the parental leave system—including eligibility and benefit rules—remained unchanged.[11]

The new rules applied to children born after August 1, 2022, which allows us to estimate the effects of the reform using a regression discontinuity design. Scope for manipulation of birth timing was limited by the short interval between political agreement and implementation—less than ten months—although parents may have anticipated some form of paternity-leave expansion following the 2019 EU Directive. We find no evidence of birth-timing manipulation in the data.

Two features of the reform are particularly noteworthy. First, relative to reforms studied in the existing literature, the Danish reform introduced a substantially larger expansion of earmarked paternity leave. Prior studies of paternity-leave reforms in Norway, Sweden, Spain, and Quebec rely on first-stage increases of two to three weeks or less (Ekberg, Eriksson, and Friebel 2013; Dahl, Løken, and Mogstad 2014; Farré and González 2019; Patnaik 2019; Andresen and Nix 2026). Second, because the additional paternity leave was offset against previously transferable parental leave, the reform reduced leave optionality for mothers. This design feature induced a substantial reallocation of leave from mothers to fathers, generating an unusually large experimental shock to the gender division of childcare.

3 Data

3.1 Beliefs and Norms Survey

A key component of our research design is a large-scale survey of parents with newborn children in which we elicit rich information on beliefs, norms, and choices related to childcare and careers. The survey was fielded through Statistics Denmark's Survey Division. By exploiting continuously updated population records with parent-child links, we were able to target the

[11] Self-employed parents and single parents were exempt from the new rules and remained under the old regime.

near-universe of parents with children born around the cutoff date of the Danish paternity-leave reform. The survey was designed after the adoption of the EU directive described above, and it began running before the announcement and enactment of the Danish reform (see Section 2.2). This timing allowed us to collect survey responses for a full year prior to—and a full year after—the reform cutoff, providing a unique opportunity to study its effects on beliefs and norms using a regression discontinuity design.

The sampling frame was restricted to parents who (i) are registered as parents in the population records, (ii) are at least 18 years old, (iii) reside at the same address as the child, and (iv) have a child who is alive at the time of the survey. Under these criteria, survey invitations were sent to approximately 99% of all mothers and 90% of all fathers with children born between August 1, 2021 and July 31, 2023.[12]

Parents were interviewed twice—when their child was 4 months old and again at 18 months. This timing was chosen deliberately: because virtually all families take maternity leave first and paternity leave second, the two survey waves capture beliefs and norms before and after families experience the longer paternity leave. To ensure consistency in children's ages at each interview, parents were sampled and interviewed on a monthly basis.

Survey invitations were administered through a secure government email system, *E-Boks*, which is used by public-sector agencies to communicate with residents on a wide range of matters, including taxes, transfers, pensions, and health services. Invitations were sent separately to mothers and fathers using an identical questionnaire. To increase response rates, we sent two reminders following the initial invitation and incentivized participation through a DKK 1,000 lottery, with one winner drawn per survey wave and month. As shown in Appendix Figure A.1, response rates were approximately 30% for mothers and 15-20% for fathers, corresponding to almost 40,000 parents per wave. The characteristics of these respondents and selection into the survey are analyzed below.[13]

[12]The lower sampling rate for fathers reflects two factors. First, unmarried fathers must actively register their parental status in the population records. Second, separated fathers are less likely to reside with their infant child. Mothers, by contrast, are automatically linked to their children in the population records, and they almost always reside with their infant child following a separation.

[13]The lottery incentive was introduced during the first wave for parents with children born in February 2022

In terms of content, the survey collects detailed information on beliefs and behaviors across four main modules. The first module focuses on parents' understanding of the Danish parental leave system. The second module elicits information on leave allocation, support for earmarked paternity leave, and leave satisfaction. The third module covers parental behavior during leave, with a focus on the division of childcare and household chores as well as breastfeeding duration. The final module elicits beliefs about gender roles, childcare, and careers. It includes both canonical questions from value surveys and a set of alternative questions tailored to our setting.

3.2 Administrative Data

We link the survey data to full-population administrative records from Statistics Denmark using personal identification numbers. This linkage provides a rich set of covariates for parents and their children—regardless of survey participation—allowing us to analyze selection into the survey. It also enables us to study the effects of the paternity-leave reform on leave-taking behavior (the first stage) and on labor market outcomes, for both survey respondents and the full population.

Two administrative datasets are central to our analysis. First, leave-spell records provide detailed information on each parent's parental-leave spells, including duration, leave type, benefit amounts, and whether benefits were paid directly to the parent or to the employer.[14] Second, matched employer-employee data provide information on earnings, hours worked, and other labor market outcomes at a monthly frequency for all wage earners. The monthly frequency is valuable for aligning outcomes precisely with the timing of childbirth, allowing us to measure labor market outcomes in the first year after birth, the second year after birth, and so on, without the measurement error that arises in annual data from misalignment between calendar time and the child's age. The registers cover the period from January 2008 to June 2025, providing a long pre-reform window and, at present, up to two years of post-birth outcomes.

or later. Appendix Figure A.1 shows that the lottery increased response rates by about 6pp (4.5pp) for mothers (fathers). In the second wave—which forms the basis for most of our analyses—the lottery was in place for all sampled parents.

[14]This allows us to distinguish leave periods covered by employer-provided benefits (with a replacement rate of 100%) from those covered exclusively by public benefits (with an average replacement rate of roughly 50%).

3.3 Descriptive Statistics

Our analysis focuses on families exposed to the reform—specifically, families in which both parents are eligible for parental leave and the father is not self-employed. This yields a full analysis sample of 174,466 parents over the study period (August 2021 to July 2023), of whom 38,494 responded to the second wave of the survey.[15]

Table 1 reports descriptive statistics—covering demographics and pre-birth labor market outcomes—for the full population of parents and for survey respondents. Comparing the first two columns, survey respondents are more likely to be married and to be first-time parents. They are also more highly educated and have higher pre-birth household earnings.[16] With the exception of education, however, the differences between respondents and the full population are relatively modest.

To address selection into the survey, we reweight respondents using inverse probability weights estimated from a logistic regression of survey participation on observable characteristics. We include two sets of covariates. The first set consists of demographic indicators for marital status, first-time parent status, age at birth, education level (7 categories), and region of residence (6 categories). The second set augments these with pre-birth earnings, pre-birth employment, and fathers' parental-leave duration (in weekly bins capped at 24 weeks). Reweighting on paternity-leave duration addresses a potentially important dimension of selection and ensures that first-stage effects are comparable between survey respondents and the full population. The model is estimated separately by survey wave, gender, and month, allowing selection patterns to vary flexibly over time.

Our reweighting procedure ensures balance on observables between survey respondents and the full population, as shown in the last column of Table 1. While our baseline results are based on the reweighted sample, we also show that the findings are highly robust to alternative

[15] A slightly larger number of parents responded to the first survey wave (see Appendix Figure A.1). Unless otherwise specified, we focus on outcomes from the second wave throughout the paper—that is, after both mothers and fathers have experienced parental leave.

[16] We also note that the female share of household earnings is slightly higher among survey respondents, and that the duration of paternity leave (not shown) tends to be longer. These patterns are consistent with survey respondents being positively selected on gender-progressive attitudes.

weighting schemes, including using no weights at all. The reason is that selection into the survey is relatively stable over time and therefore does not generate discontinuities in outcomes around the reform cutoff. As a result, reweighting primarily supports the external validity of our RD estimates, while internal validity does not hinge on the weighting procedure.

As an additional check, we assess the scope for selection on unobservables by exploiting the lottery incentive for survey participation, which was introduced during the first survey wave for parents with children born after February 2022. Using the lottery cutoff, Appendix Figure A.2 presents RD evidence on whether lottery-induced respondents differ from infra-marginal respondents across a range of survey outcomes. The left column reports estimates for the unweighted sample, while the right column reports estimates for the reweighted sample. The observed discontinuities in unweighted estimates reflect selection driven by either observables or unobservables. The fact that these discontinuities disappear once reweighting is applied—all effects in the right column are small and statistically insignificant—suggests that our reweighting procedure effectively addresses selection into the survey along both observed and unobserved dimensions. Our test for selection on unobservables is similar in spirit to the test proposed by Dutz *et al.* (2026).[17]

Finally, we examine whether the gender beliefs elicited in our survey are predictive of actual parental behavior. Figure 1 shows the pre-reform correlation between fathers' share of parental leave and agreement with the statement "A pre-school child is likely to suffer if their mother works full time." The correlation is strongly negative: in families where parents strongly agree with this statement, fathers take less than half as much leave as in families where parents strongly disagree. While this evidence is purely correlational, it suggests that elicited beliefs capture relevant determinants of behavior. This motivates our central research question: can such beliefs be shifted by policy, with meaningful consequences for parental outcomes?

[17] A difference between the tests is that Dutz *et al.* (2026) rely on randomized participation incentives, whereas we exploit a discontinuity in participation incentives at a birth-date cutoff.

4 Empirical Design

4.1 RD Specification

We exploit the sharp discontinuity in paternity-leave rules generated by the August 1, 2022 birth-date cutoff. Families with children born after August 1 are eligible for eleven weeks of earmarked paternity leave, whereas those with earlier birth dates are subject to the previous two-week regime. Let d_i denote the running variable—the date of childbirth relative to the cutoff—and let $T_i \equiv \mathbf{1}[d_i \geq 0]$ denote the treatment indicator. The key identifying assumption is that potential outcomes evolve smoothly at the cutoff in the absence of the reform. This assumption is plausible in our setting because the reform was announced roughly nine months prior to implementation, leaving little scope for families to manipulate the timing of birth.

We estimate the reduced-form effect on an outcome Y_i using the following RD specification:

$$Y_i = \alpha + \beta \cdot T_i + \gamma \cdot d_i \cdot \mathbf{1}[d_i < 0] + \delta \cdot d_i \cdot \mathbf{1}[d_i \geq 0] + \nu_i, \tag{1}$$

where β captures the effect of the reform, and γ and δ denote local linear trends on each side of the cutoff. Our baseline specification uses a bandwidth of twelve months on each side of the cutoff. We show that the results are robust to alternative bandwidth choices, donut specifications, and higher-order polynomial trends.

As described above, we reweight the sample of survey respondents to ensure that they are representative of the full population of parents who give birth within one year of the reform. To validate the empirical design, Appendix Figure A.3 provides RD evidence on the effect of the reform on demographic characteristics in the raw and weighted samples. We examine four outcomes: the fraction married, the fraction with a college degree, the fraction who are first-time parents, and age at birth. All outcomes are standardized to have mean zero and standard deviation one in the full population of parents, so that positive values indicate that a characteristic is overrepresented among survey respondents. Two patterns emerge. First, the outcomes are generally above zero in the unweighted sample but extremely close to zero once

weighted, confirming that the estimation sample is representative on observables. Second, none of the outcomes display discontinuities at the cutoff. This is consistent with no manipulation of birth timing and supports the identifying assumption of the RD design.

In addition to the reduced-form estimates, we provide IV estimates of the effects of fathers' share of parental leave. Let S_i denote the father's leave share in family i. We estimate the following specification:

$$Y_i = \alpha + \beta \cdot S_i + \gamma \cdot d_i \cdot \mathbf{1}\left[d_i < 0\right] + \delta \cdot d_i \cdot \mathbf{1}\left[d_i \geq 0\right] + \nu_i, \tag{2}$$

using the treatment indicator T_i as an instrument for S_i. Specifically, equation (2) is estimated by 2SLS, where the first stage corresponds to equation (1) with $Y_i = S_i$. The resulting estimator, β_{IV}, identifies the effect of increasing fathers' leave share from 0 to 1. This parameter can be used to quantify the impacts of counterfactual policy regimes that induce larger shifts in leave-taking than the shift generated by the observed reform.

In the following sections, we consider a range of outcomes from the survey and administrative data. In the survey, most outcomes are categorical. Participants are presented with a statement and asked whether they strongly disagree, disagree, neither agree nor disagree, agree, or strongly agree. We code these five response categories as 0.00, 0.25, 0.50, 0.75, and 1.00, respectively. An alternative would be to use an indicator for agreeing or strongly agreeing; the results are similar under this approach. Our preferred measure, however, exploits all available variation: it can respond to changes in the share agreeing versus disagreeing as well as to changes in the intensity of respondents' views.

4.2 First Stage

Figure 2 presents the first-stage effects of the reform on leave duration. Panel A plots leave duration for mothers (red) and fathers (blue) by month of childbirth around the August 1, 2022 cutoff. Before the reform, mothers took about 40 weeks of leave on average, while fathers took about 5 weeks. At the cutoff, mothers' leave duration drops by more than 5 weeks and fathers'

leave increases by 3.7 weeks.[18] Panel B shows the resulting increase in fathers' leave share. This measure rises by 8.1 percentage points—from 12.4% before the reform to 20.5% afterward.

Figure 3 provides additional evidence on the first-stage effects by plotting the cumulative distributions of leave duration before and after the reform for mothers (Panel A) and fathers (Panel B). The reform shifts the distribution to the left for mothers and to the right for fathers. For mothers, the distribution exhibits a discontinuity at 46 weeks before the reform and at 37 weeks afterward, corresponding to the maximum amount of paid maternity leave under the pre- and post-reform rules. For fathers, the distribution displays a discontinuity at 2 weeks before the reform and at 11 weeks afterward, reflecting the amount of earmarked paternity leave under the two regimes. Notably, even before the reform, about 20% of fathers took at least eleven weeks of leave. These fathers are always-takers and are effectively unexposed to the reform.

As discussed above, leave is reallocated from mothers to fathers—rather than merely expanded for fathers—because the additional weeks of earmarked paternity leave were drawn from preexisting parental leave available to either parent. As a result, the reform reduced leave optionality for mothers. This feature of the policy design is likely important and may help explain why we find stronger effects than previous studies of paternity leave. It also links our analysis to the literature on maternity leave. To our knowledge, that literature has examined only expansions of maternity leave, not contractions.

4.3 Complier Analysis

The effects of the reform are driven by compliers—families induced to increase paternity leave due to the reform. We focus on a strong definition of compliers: those whose paternity leave is shifted to at least eleven weeks, corresponding to full take-up of the earmarked leave. We can characterize the average properties of this complier population using a simple decomposition. For any outcome Y, we have

$$Y_{\geq 11} = \lambda \cdot Y_C + (1 - \lambda) \cdot Y_A, \tag{3}$$

[18] The first-stage effect on total household leave is presented in Appendix Figure A.4. The reform reduces total leave by 1.4 weeks.

where the average value of Y among families who take at least eleven weeks of paternity leave after the reform, $Y_{\geq 11}$, is expressed as a weighted average of the outcomes for compliers (Y_C) and always-takers (Y_A). The quantity $Y_{\geq 11}$ is directly observed in the post-reform data, and Y_A can be obtained from families who took at least eleven weeks of leave before the reform. The share of compliers, λ, can be calculated based on the reform-induced increase in the number of families who take at least eleven weeks of paternity leave. With these values, Y_C can be recovered from equation (3).

The results of the decomposition exercise are presented in Table 2, which reports outcomes from both the administrative and survey data. Comparing the full population of parents who give birth within one year of the reform (first column) with the compliers (last column), we find that the two groups are remarkably similar. The marriage rate is 52% among compliers, compared to 51% in the full population, and the college share is 61% versus 62%. Labor market outcomes are similarly aligned: compliers have average household income of 849,000 DKK (roughly USD 132,000), and mothers earn 41% of that income—virtually identical to the corresponding values for the full population. Turning to the survey measures, the two groups again exhibit close alignment. For example, 39% of compliers agree that pre-school children suffer when mothers work full time, compared to 36% in the population. Overall, the complier population is highly representative, which supports the external validity of our results and lends credibility to the counterfactual policy exercises presented below.

It is worth noting that compliers are highly representative despite our strong definition of compliance—families induced to increase paternity leave to the full earmarked amount of eleven weeks. In principle, however, all families who increase paternity leave in response to the reform qualify as compliers. Moreover, because the reform reduces leave optionality for mothers, any family that reduces maternity leave as a result of the reform is also a complier under the standard LATE definition. Using this broader definition, virtually all families are compliers, in which case representativeness is guaranteed by construction. The fact that compliers remain highly representative even under the strong definition is informative for the external validity of our LATE estimates.

5 Empirical Results

5.1 Beliefs About Paternity Leave Policy

In this section, we examine beliefs about the policy itself. Survey participants were presented with a range of statements—capturing both positive and negative aspects of the reform—and asked whether they agreed or disagreed. As described in Section 4.1, responses are coded from zero (strongly disagree) to one (strongly agree), allowing the outcomes to reflect both the share agreeing versus disagreeing and the intensity of those views. Values above (below) 0.5 indicate that respondents tend to agree (disagree) on average. We begin by presenting RD evidence for mothers and later compare responses for mothers and fathers.

Figure 4 shows how mothers evaluate the policy overall. The survey question asks whether "earmarked paternity leave is a good idea." Two results stand out. First, mothers are generally opposed to the policy: the average response is less than 0.4 just below the cutoff. Second, the reform makes them less opposed. The RD estimate shows a sharp and precisely estimated increase of 0.07 at the cutoff. Nonetheless, even after accounting for this positive treatment effect, mothers remain slightly below neutral in their assessment of the policy.

To explore the underlying sources of this opposition, Figure 5 reports responses to a range of questions on different aspects of the policy. Panels A-D focus on perceived benefits, while Panels E-F focus on perceived costs. The pattern is consistent across outcomes: respondents are skeptical of the policy at baseline—agreeing more readily with its costs than its benefits—but treatment substantially reduces this skepticism. The strongest treatment effects—above 0.06 and precisely estimated—appear for three outcomes: that the policy makes paternity leave more acceptable in the workplace, that it makes paternity leave more acceptable in society, and that it is important for the bond between fathers and children. These points were also among the most central in the Danish policy debate and in discussions leading up to the EU Directive.

The results in the bottom panels are informative for understanding why many people oppose the policy. These panels report elicited views on two constraining features of earmarked leave: that it prevents parents from dividing leave freely, and that it limits the time available for

breastfeeding. Respondents express strong agreement with these concerns, with baseline values of about 0.9 for the first question and about 0.75 for the second. In other words, almost all parents strongly believe that leave should be freely divisible.

Respondents express similar sentiments in written comments made in open-text boxes included in the survey. Illustrative examples include:

- "It's important that the leave is 100% free to share, so that each family can take leave in a way that suits them."
- "Forced leave for fathers is the most idiotic thing that has been passed in this country in a long time."
- "Let the parents decide!"

To examine these sentiments systematically, Appendix Figure A.5 uses a large language model to classify the open-text comments into different themes. Panel A considers all comments received, while Panel B considers comments among those against the reform—i.e., those who disagree with the statement that "earmarked paternity leave is a good idea." The figure shows that roughly 55% of all written comments revolve around the right to choose leave allocation freely. The corresponding share among those opposed to the reform is even higher, more than 60%. Roughly 15% of comments focus on the survey itself, including feedback on the relevance and phrasing of questions. All other topics—such as father-child bond, financial concerns, daycare, biology and breastfeeding—constitute small shares of the comments received.

Our findings highlight a potential cost of paternalistic policies: although such policies aim to shift behavior in socially valued directions, they do so by restricting choice sets and reducing families' ability to optimize based on idiosyncratic circumstances. This can be interpreted as a loss of consumer sovereignty. While limiting choice is the very mechanism through which paternalistic policies operate, it nonetheless imposes a welfare cost that must be weighed against the potential benefits. Families may have allocated most leave to mothers for good reasons—say, because the father's job requires frequent travel—and may be forced to reduce total leave when part of it is earmarked for fathers.

Figure 6 summarizes the RD estimates presented above and compares the effects for mothers (red) and fathers (blue). The findings are highly consistent across outcomes and between parents: treatment increases the perceived benefits of earmarked leave and reduces the perceived costs. Effects are slightly larger for fathers, but the differences are not statistically significant. To provide a summary measure of overall policy support, the figure also reports effects on a *policy-support index*. This index is constructed as the average across all outcomes in this category, reversing the coding of the cost measures to ensure comparability with the benefit measures.[19] The average effects are sizable—about 0.05 for mothers and 0.06 for fathers—and precisely estimated. The corresponding RD plots for the policy-support index are provided in Appendix Figure A.6.

We assess the robustness of our findings in Appendix Figure A.7, which reports effects on the policy-support index under a wide range of specifications. The first panel presents the baseline results for mothers (red), fathers (blue), and both parents pooled (purple). For the pooled sample, the subsequent panels consider alternative choices of sample weights, bandwidths, donut holes, and polynomial orders of the trend functions. The results are highly robust: the estimates are consistently positive and strongly statistically significant. The most notable pattern is that donut specifications—omitting either one or two months around the cutoff—yield larger treatment effects than the baseline estimates discussed above.

5.1.1 Leave Satisfaction

Given many families express concerns about the constraints imposed by earmarking, it is natural to ask whether the reform reduces satisfaction with the allocation of leave. Figure 7 provides evidence on this point by plotting survey respondents' overall leave satisfaction (yellow dots) as

[19] For example, if respondents strongly agree with a potential cost (benefit) of the policy, this is coded as zero (one). After recoding the outcomes in this manner, we stack survey items $k = 1, ..., K$ within this category and estimate the following RD specification:

$$Y_{ik} = \alpha + \beta \cdot T_i + \gamma \cdot d_i \cdot \mathbf{1}[d_i < 0] + \delta \cdot d_i \cdot \mathbf{1}[d_i \geq 0] + \nu_{ik}, \tag{4}$$

where Y_{ik} denotes outcome k for individual i. To account for within-individual correlation across outcomes, standard errors are clustered at the individual level.

well as their preferences for reallocating leave toward mothers (red dots) or toward fathers (blue dots).[20] Panel A reports mothers' responses and Panel B reports fathers' responses.

The findings are striking. About 90% of both mothers and fathers were satisfied with their leave allocation prior to the reform, but this share falls to 50% for mothers and 60% for fathers after the reform. Moreover, both groups attribute their dissatisfaction to the same underlying issue: a preference for more leave to mothers, rather than less leave to fathers. These preferences align with the design of the reform and with the first-stage effects presented above. By reallocating freely divisible parental leave to earmarked paternity leave, the reform reduced leave optionality for mothers while leaving it unchanged for fathers. Although mothers and fathers express similar views overall, the decline in satisfaction is larger for mothers, as is the increase in their stated preference for more maternal leave.

These results reinforce the welfare trade-offs posed by earmarking, discussed above. The loss of leave satisfaction must be weighed against the potential benefits of shifting gender-role norms and reducing gender inequality. The following sections provide evidence on these potential benefits.

5.2 Beliefs About Gender Roles and Childcare

5.2.1 Reduced-Form Estimates

Figure 8 examines whether the paternity-leave expansion shifts beliefs about gender roles. We analyze six measures of gender-role attitudes, including canonical questions from value surveys as well as several alternative questions tailored to our setting. As above, responses are coded from zero (strongly disagree) to one (strongly agree), allowing the outcomes to capture both the share agreeing versus disagreeing and the intensity of those views. We first present effects for mothers and then compare effects for mothers and fathers.

We find that the reform shifts beliefs in a gender-progressive direction. Consider the canonical question in the first panel: whether a pre-school child suffers if their mother works full time. The

[20] These outcomes are taken from the first wave of the survey, whereas the preceding survey outcomes were drawn from the second wave. Questions on leave satisfaction were included only in the first wave.

average response is about 0.5 at baseline and drops by 0.031 (0.010) at the cutoff. We observe similar declines in the belief that mothers should take most of the leave and in the perception that mothers have stronger bonds with their children than fathers. The remaining questions ask whether mothers or fathers care more about their job than their family if they take less than a certain amount of leave. Here the effects are particularly pronounced for the statement that fathers who take less than two months of leave care more about their job than their family: the outcome increases by 0.038 (0.008) at the cutoff, indicating that the reform raises expectations regarding how much leave fathers ought to take.[21]

Figure 9 investigates whether the reform affects beliefs about childcare, again focusing on mothers' responses. Panel A considers perceived childcare abilities of mothers relative to fathers at three child ages: 0-6 months, 6-9 months, and 9-12 months. Values above (below) 0.5 indicate that mothers view themselves as better (worse) at childcare than fathers. We find that mothers rate themselves substantially better at childcare—though the perceived advantage narrows as the child ages—but the reform significantly reduces the perceived gap at all ages.

Panel B reports beliefs about the importance of breastfeeding, a task that only mothers can perform. Mothers view breastfeeding as important, particularly for children younger than six months but also at older ages. However, the reform reduces the perceived importance of breastfeeding. The effects are small for children younger than nine months, but larger and statistically significant for children aged 9-12 months. Because most families take maternity leave first and paternity leave second, these effects coincide with the period during which fathers take their expanded paternity leave.

Taken together, these results indicate that expanding paternity leave shifts beliefs about childcare abilities and breastfeeding in ways that could reduce the childcare burden on women.

Figure 10 summarizes the RD estimates for gender-role and childcare beliefs, comparing the effects for mothers (red) and fathers (blue). The findings are consistent across outcomes and

[21] In principle, this effect could reflect the change in scheme rules—earmarking eleven weeks of leave to fathers—rather than a shift in underlying preferences or norms. However, the questions about policy beliefs and the questions about gender-role beliefs were asked in separate sections of the survey. The items considered here were part of a section on attitudes toward gender roles, not views about the policy itself.

between mothers and fathers, although the effects for fathers are less precisely estimated due to their smaller sample size. As in the corresponding figure on policy beliefs, we construct a *gender-beliefs index* that captures the average effect across all outcomes in this category, estimated using equation (4). A negative effect on this index indicates a shift toward more gender-progressive norms. The estimated effects are similar for mothers and fathers, and they are precisely estimated: -0.023 (0.005) for mothers and -0.020 (0.006) for fathers. The corresponding RD plots are provided in Appendix Figure A.8.

The robustness of the effects on the gender-beliefs index is evaluated in Figure 11. Starting with the baseline estimates for mothers (red), fathers (blue), and both parents pooled (purple), the figure reports pooled estimates across a range of alternative specifications. These include different choices of sample weights, bandwidths, donut holes, and polynomial orders of the trend functions. The results are highly robust: the estimates are consistently negative and statistically significant, and their magnitudes remain stable across specifications. If anything, the estimated effects become larger when introducing donut holes or higher-order polynomials, suggesting that the baseline estimates may be conservative.

Finally, Figure 12 explores heterogeneity in the effects on gender beliefs. The top row compares families who take less than twelve weeks of paternity leave throughout the sample period with families who take more than twelve weeks. Because the reform expands earmarked leave to eleven weeks, those taking more than twelve weeks are always-takers and therefore unexposed to the new scheme. Consistent with this, we find that the effects are driven entirely by exposed families; the effect on unexposed families is a precisely estimated zero. This pattern provides an additional validation of our empirical design.

The bottom row examines heterogeneity by birth order, comparing families with first births to those with higher-parity births. The effects are much larger for first-time parents: the reform reduces their gender-beliefs index by 0.034 (0.007), which is roughly 2.5 times larger than for parents with later births. A natural interpretation is that first-time parents are less anchored in pre-existing gender-role models and therefore more susceptible to shifting their beliefs. Importantly, the effects for first-time parents are likely more informative about the steady-state

impact of the reform. Over time, as new cohorts form families and have children, everyone will experience the new regime starting with their first child.

5.2.2 IV Estimates and Counterfactual Policy Regimes

To assess the magnitudes of the effects, we relate the reduced-form estimates to the first stage—the leave reallocation induced by the reform. To this end, we estimate the IV specification described in Section 4.1. Specifically, we regress outcomes on fathers' share of parental leave, instrumented using the birth-date discontinuity in scheme rules. The specification is estimated by 2SLS.

The results are presented in Table 3. The top panel reports the first stage. The reform increases fathers' leave share by 0.081 from a baseline of 0.124, corresponding to a 65% increase. The first-stage F-statistic is 625.3.

The subsequent panels present second-stage estimates, first for mothers and then for fathers. Results are shown for the gender-beliefs index and for three of its underlying components. The results are highly consistent across parents and outcomes. The IV estimates for the gender-beliefs index are -0.286 (0.055) for mothers and -0.240 (0.075) for fathers. Because the outcomes are coded from zero to one—with one representing the most gender-conservative beliefs possible—these estimates imply belief shifts of about a quarter of their total range. While these changes are substantial, the policy variation they correspond to is also extreme: by construction, the IV estimates capture the effect of increasing fathers' leave share from zero to one.

An advantage of the IV estimates is that they lend themselves to analyzing the effects of counterfactual policy regimes. As noted above, the actual reform increases fathers' leave share by 0.081—from 0.124 before the reform to 0.205 afterward. It is informative to consider policies that induce smaller or larger shifts in fathers' leave-taking. Table 3 reports predicted gender beliefs under three alternative scenarios: fathers' leave shares of 0.00, 0.25, and 0.50. Under the equal-leave scenario (0.50), the gender-beliefs index—approximately 0.6 at baseline—would fall to about 0.5 for both mothers and fathers. Some of the subcomponents of the index fall to values well below 0.5. Given a value of 0.5 represents gender neutral views, this implies that

equal-leave policies have the potential to shift average beliefs from the conservative range into the progressive range.

To summarize, our findings suggest that earmarked paternity leave can generate substantial shifts in beliefs and norms about gender roles and childcare. The implemented reform generated sizable effects, and further expanding paternity leave toward an equal-leave scenario would produce even larger effects. Of course, the extrapolations to counterfactual policy regimes are inherently suggestive. They rely on the assumption that treatment effects scale proportionally as we move to policies far from those observed. In practice, the effects may be nonlinear, in which case further expansions could yield either smaller or larger changes than those implied by our IV estimates.

5.2.3 First Wave vs Second Wave

The preceding analyses focus on outcomes drawn from the second wave of our survey. These interviews were conducted when children were 18 months old—i.e., after the completion of government-sponsored leave for both parents. The first wave of our survey interviewed parents when their children were 4 months old. Because virtually all families take maternity leave first and paternity leave second, these interviews took place before experiencing the longer paternity leave. If beliefs are shaped partly or wholly by actual experience of leave, the reform will have smaller effects in the first wave. Any changes in first-wave beliefs would reflect anticipatory effects of parents' knowledge of the new policy regime—for example, if they interpret government policy as a signal about appropriate or expected childcare division.

To investigate this question, Appendix Figure A.9 compares RD estimates of the effects of the reform on gender-role beliefs. We consider two specific questions: whether respondents believe pre-school children will suffer if their mothers work full time, and whether they believe it is better if mothers take most of the leave. First-wave effects are depicted by gray bars, while second-wave effects are depicted by purple bars. The sample includes both mothers and fathers. The effects in the first wave are small and statistically insignificant. The effects in the second wave are three to four times larger and strongly significant. These results speak against any

effects of pure knowledge (without any personal experience) of the new paternity-leave regime.

5.3 Actual Childcare Division

Do the effects on gender-role and childcare beliefs translate into changes in the *actual* division of childcare? This is an important question, because childcare is one of the key mechanisms through which beliefs may influence realized labor market outcomes. Our survey elicits information on childcare tasks in four domains: childcare at night, drop-off at daycare, pick-up from daycare, and childcare when children are sick. Responses fall into five categories, coded as follows: "My partner does almost everything" (0); "My partner does most" (0.25); "My partner and I do about the same" (0.50); "I do most" (0.75); and "I do almost everything" (1). In this section, we examine the effects of the paternity-leave expansion on these outcomes.

Appendix Figures A.10-A.11 present our results. The first figure reports RD estimates for each of the aforementioned tasks, showing effects for mothers (red), fathers (blue), and fathers relative to mothers (purple). The direction of the effects is consistent with what we would expect—fathers do more and mothers do less—but the individual categories tend to be underpowered. To increase statistical precision, we estimate a *childcare index* that captures the average effect across all four outcomes. The effects on the index are statistically significant for fathers as well as for fathers relative to mothers. The second figure restricts attention to the index and evaluates robustness across a wide range of specifications, following the approach used in earlier sections. The findings are robust: point estimates are similar across specifications, and most of them are statistically significant.

In addition to childcare division, the bottom panel of Appendix Figure A.10 examines effects on perceived career constraints. We consider two questions: whether parents find it difficult to work the expected hours, and whether having children leaves them with little flexibility in life. The fact that the point estimates are positive for fathers and negative for mothers suggests that the reform shifts career concerns from mothers to fathers. The individual estimates are underpowered, however, as with the childcare outcomes. Focusing on the average effect across

the two outcomes—the career-constraints index—improves precision and yields estimates that are close to statistical significance at the 5 percent level.

To conclude, the paternity-leave expansion shifts beliefs about childcare (previous section) and the actual division of childcare (this section) in a gender-progressive direction. The reform also affects perceived career constraints, although these estimates are less precise. The key remaining question is whether the reform translates into changes in real labor market outcomes—specifically, whether it increases gender equality—through these or other mechanisms. We address this question in the next section.

5.4 Gender Gaps in Labor Market Outcomes

To examine effects on labor market outcomes, we use administrative records on monthly earnings and hours worked. The availability of monthly—rather than annual—data is a key advantage in this context, as it allows us to study post-birth dynamics without the measurement error that arises when calendar time and time since childbirth are misaligned for families giving birth throughout the year. We estimate effects on earnings and hours during the first year (months 1-12) and the second year (months 13-24) following childbirth. Because the reform is recent, it is not yet possible to analyze effects beyond this two-year horizon. Our main outcome variable is the log difference between male and female outcomes within the household. This implies that the estimated treatment effects can be interpreted (approximately) as percentage-point changes in gender gaps. The earnings measure used in the analysis excludes parental leave benefits—whether paid by employers or the government—though the data allow for including these components as well.

As a validation check, Figure 13 considers earnings and hours worked during the twelve months prior to childbirth. Because the reform does not affect pre-birth incentives or constraints, any estimated effects on these outcomes would indicate selection around the birth-date cutoff. The top panel presents an RD plot of the pre-birth gender gap in earnings and shows a precisely estimated zero effect. The bottom panel shows the same pattern for hours worked. These

placebo estimates confirm the absence of selection effects, complementing the earlier evidence on demographic variables in Appendix Figure A.3.

Figure 14 examines our main outcomes of interest—post-birth gender gaps in earnings and hours worked. The top row focuses on the twelve months immediately after birth. The effects are large and sharp: the reform reduces the earnings gap by about 34pp and the hours gap by about 33pp. Note that, while the first-year effects are very large, they are mostly mechanical: the reduction in gender gaps is a direct consequence of the forced reallocation of leave between mothers and fathers during the child's first year.

The bottom row considers the subsequent twelve months. We find non-trivial effects on gender gaps even in the second year. The earnings gap falls by 2.8pp and the hours gap falls by 1.4pp, although the hours estimates are underpowered.[22] Importantly, because government-sponsored parental leave—maternity, paternity, and general parental leave included—lasts at most 48 weeks in Denmark, second-year labor market outcomes are not directly affected by the change in leave rules.[23] The second-year effects must therefore operate through longer-lasting consequences of the reform. The changes in beliefs and norms documented above, along with the reallocation of actual childcare duties, are natural candidates for such mechanisms. We consider mechanisms in a mediation analysis presented below.

Appendix Table A.1 decomposes the effects on gender gaps into changes in male and female outcomes. We find that the effects arise from both higher female outcomes and lower male outcomes. For earnings, 78% of the first-year effect and 62% of the second-year effect are driven by mothers, with the remainder driven by fathers. There are two reasons why mothers account for a greater share of the effects: (i) their baseline earnings level is smaller—especially in the first year following childbirth—so that each dollar of earnings translates into a larger percentage effect; and (ii) their leave contraction is larger than the leave expansion for fathers (see Figure 2). In level terms, the reform-induced earnings changes are similar for mothers and fathers. As a

[22] As further validation, Appendix Figure A.12 provides placebo estimates of second-year effects based on the pre-reform years 2018-2021. Conducting RD analyses around the August 1st birth-date cutoff in these pre-reform years, the figure shows that placebo effects on gender gaps in both earnings and hours are precisely estimated zeros.

[23] Total family leave may be shorter than 48 weeks either because of incomplete take-up or because mothers and fathers take some of their leave concurrently.

result, the effect on total household earnings is small and statistically insignificant.

To summarize, the Danish paternity-leave reform changed not only gender-role and childcare beliefs, but also gender gaps in labor market outcomes. In the second year of the child's life—after both parents have completed their leave—the reform reduces the gender gap in earnings by 2.8pp and the gender gap in hours worked by 1.4pp. To put these magnitudes in perspective, child penalties in Denmark amount to roughly 20% in earnings and 10% in hours (Kleven, Landais, and Søgaard 2019). Hence, the reform's effects correspond to 14% of child-related gender inequality in both earnings and hours. These impacts are driven by an increase in fathers' leave share from 12.4% to 20.5% and, as discussed above, could be larger under further expansions of earmarked paternity leave toward an equal-leave regime. A crucial question, of course, is whether the two-year effects we document will persist over time. Given the recency of the reform, it is not yet possible to study longer-term consequences. Understanding the persistence of these effects is an important question for future work.

5.5 Share of Gender Gap Effects Explained by Beliefs and Childcare

In the preceding sections, we have seen that the Danish paternity-leave reform changed beliefs about gender roles and childcare, shifted the actual division of childcare toward fathers, and reduced gender gaps in labor market outcomes. A plausible interpretation is that these different treatment effects are linked. For example, changes in gender-role beliefs may impact decisions about childcare allocation, which then influences gender differences in labor market outcomes. Either beliefs or childcare may also have independent effects on labor market outcomes. In this section, we ask how much of the effect on gender gaps can be explained by the effects on beliefs and childcare. We have no quasi-experimental research design for connecting survey outcomes and gender gaps—only for connecting each of them to paternity-leave reform—but we may provide a mediation analysis by combining our RD estimates with pre-reform associations between the different variables.

Figure 15 examines the pre-reform relationship between gender gaps in labor market

outcomes—earnings and hours worked, respectively—and either beliefs or childcare division. Both gender gaps and survey outcomes are measured in the second year following childbirth, after the completion of government-sponsored leave. The different panels present binned scatterplots of gender gaps against the gender-beliefs index (top row), the policy-support index (middle row), and the childcare gap (bottom row). The childcare gap is measured as childcare for mothers relative to fathers across the different domains described in Section 5.3. It is worth noting that these relationships are estimated at the household level, which is possible in our setting due to the linkage between the survey and administrative data. Prior studies using value surveys have been restricted to estimating correlations at geographically aggregated levels such as states or countries.

We find strong and virtually linear relationships between gender gaps and survey outcomes. More progressive beliefs about gender roles and childcare—corresponding to a smaller value of the gender-beliefs index—as well as greater support for earmarked paternity leave are associated with smaller gender gaps in hours and earnings. Similarly, greater equality in the division of childcare is associated with smaller gender gaps. The relationship between gender gaps in labor market outcomes and the childcare gap is particularly strong, with slope coefficients of close to one.

Table 4 quantifies how much of the reform-induced reduction in the earnings gap can be explained by changes in beliefs and childcare allocation. The top panel reports pre-reform associations between the earnings gap and survey outcomes, including both raw correlations (as shown in Figure 15) and residual correlations after controlling for other determinants of labor market outcomes. The controls include education (6 bins), occupation (5 bins), industry (5 bins), and age (2nd-order polynomial) for each parent. The most saturated specification simultaneously includes the gender-beliefs index (views on gender roles generally), the policy-support index (views on earmarked paternity leave specifically), and the childcare gap between mothers and fathers. The coefficients on all three regressors—whether estimated separately or jointly—are sizable and strongly significant, even after controlling for the confounders just listed.

The bottom panel links these pre-reform associations with our RD estimates of reform-

induced changes in the earnings gap, beliefs, and childcare. The first line reports the RD effect on the earnings gap (previously shown in Figure 14), while the second line shows the part of the effect that operates through beliefs and/or childcare. The latter is calculated by multiplying the pre-reform correlations with the RD effects on beliefs indices and the childcare gap. From these numbers, we infer the share of the earnings gap effect explained by beliefs and childcare. Focusing on the most saturated specification, we find that changes in beliefs and childcare allocation jointly account for 96% of the reform effect on the earnings gap. Adding childcare to a specification with only beliefs, and vice versa, increases the fraction explained. This suggests that the two factors have partly independent effects on labor market outcomes, as opposed to a situation in which the entire effect of beliefs runs through childcare allocation.

To conclude, the mediation analysis presented here is consistent with a story in which paternity-leave expansions increase gender equality by shifting gender-role beliefs and childcare allocation in a progressive direction. In fact, our estimates suggest that almost all of the effect on gender gaps is mediated by beliefs and childcare. There is essentially nothing left to be explained by other mechanisms such as dynamic labor market effects of parental leave. Of course, this analysis is only suggestive: it relies partly on cross-sectional correlations during the pre-reform period, which may or may not be causal. To support a causal interpretation, we highlight that prior studies find sizable effects of beliefs and social norms on gender gaps using a variety of research designs (e.g., Boelmann, Raute, and Schönberg 2025; Kleven 2025). These studies argue that the correlation between beliefs/norms and gender gaps is at least partly causal.

6 Conclusion

This paper examines whether policy can shift gendered beliefs, norms, and labor market outcomes by exploiting a major expansion of earmarked paternity leave in Denmark. The reform increased fathers' earmarked leave from two to eleven weeks, offset against parental leave previously available to either parent. It generated strong first-stage effects, increasing fathers' share of leave from about 12% to about 21%. Using a regression discontinuity design

combined with new survey data, we find striking effects on measures of beliefs and norms. The reform makes parents more supportive of paternity leave, shifts gender-role beliefs in a progressive direction, and reduces perceived differences in childcare ability. It also reduces gender gaps in earnings and hours. These effects are very large in the first year following childbirth and remain sizable in the second year—after both parents have completed their leave and are no longer constrained by the scheme. Overall, the results show that policy can meaningfully influence beliefs, norms, and gender inequality.

At the same time, our findings highlight a potential welfare cost: earmarking restricts parents' freedom to allocate leave according to their own preferences and constraints. Almost all parents report that leave should be freely divisible and many express concerns that earmarking reduces the time available for breastfeeding. Consistent with these views, the reform sharply reduces leave satisfaction. These findings point to a broader welfare trade-off associated with paternalistic policies: such policies may generate benefits by shifting norms and behaviors in socially valued directions—in this case, toward greater gender equality—but they may also impose costs by limiting consumer sovereignty.

Two questions remain regarding the dynamics of the effects. First, will the effects on beliefs and labor market outcomes persist over time? Given the recency of the reform, we are able to study its consequences for up to two years following childbirth. The presence of significant second-year effects—after both parents have completed their leave—indicates some degree of persistence, but it remains to be seen how long-lasting these effects are. Second, will the reduction in leave satisfaction persist, or will it attenuate as families adapt to the new regime? Other paternalistic policies—from mandatory seat belts to smoking bans in public spaces—were controversial at the time of implementation, yet few object to them today or view them as constraining. Future waves of our survey will allow us to test whether earmarked paternity leave exhibits a similar pattern of adaptation.

References

Abdelgadir, Aala, and Vasiliki Fouka. 2020. "Political Secularism and Muslim Integration in the West: Assessing the Effects of the French Headscarf Ban." *American Political Science Review*, 114(3): 707–723.

Aksoy, Cevat, Christopher Carpenter, Ralph De Haas, and Kevin Tran. 2020. "Do Laws Shape Attitudes? Evidence From Same-Sex Relationship Recognition Policies in Europe." *European Economic Review*, 124(1).

Albrecht, James, Per-Anders Edin, Raquel Fernández, Jiwon Lee, Peter Thoursie, and Susan Vroman. 2026. "Parental Leave: Economic Incentives and Cultural Change." Working Paper.

Alesina, Alberto, Matteo Ferroni, and Stefanie Stantcheva. 2024. "Perceptions of Racial Gaps, their Causes, and Ways to Reduce Them." NBER Working Paper No. 29245.

Alesina, Alberto, Stefanie Stantcheva, and Edoardo Teso. 2018. "Intergenerational Mobility and Preferences for Redistribution." *American Economic Review*, 108(2): 521–554.

Altonji, Joseph, and Rebecca Blank. 1999. "Race and Gender in the Labor Market." In *Handbook of Labor Economics*. Vol. 3, ed. Orley Ashenfelter and David Card, Chapter 48. Elsevier: Amsterdam.

Andresen, Martin, and Emily Nix. 2022. "What Causes the Child Penalty? Evidence from Adopting and Same Sex Couples." *Journal of Labor Economics*, 40(4): 971–1004.

Andresen, Martin, and Emily Nix. 2026. "You Cannot Force Me Into Caregiving: Paternity Leave and the Child Penalty." *Economic Journal*, 136(674): 780–797.

Bertrand, Marianne. 2011. "New Perspectives on Gender." In *Handbook of Labor Economics*. Vol. 4b, ed. Orley Ashenfelter and David Card, Chapter 17. Elsevier: Amsterdam.

Bertrand, Marianne. 2020. "Gender in the Twenty-First Century." *AEA Papers and Proceedings*, 110: 1–24.

Bertrand, Marianne, Claudia Goldin, and Lawrence Katz. 2010. "Dynamics of the Gender Gap for Young Professionals in the Financial and Corporate Sectors." *American Economic Journal: Applied Economics*, 2(3): 228–255.

Beskæftigelsesministeriet. 2021. "Aftale om implementering af EU's orlovsdirektiv og ligestilling af orlovsrettigheder mellem forældre og øremærket forældreorlov." October 26, 2021.

Beskæftigelsesministeriet. 2022. "Lov om ændring af barselsloven (Indførelse af øremærket orlov, ligedeling af retten til barselsdagpenge m.v.)." In *Lovtidende A*, Number 343, March 22, 2022.

Boelmann, Barbara, Anna Raute, and Uta Schönberg. 2025. "Wind of Change? Cultural Determinants of Maternal Labor Supply." *American Economic Journal: Applied Economics*, 17(2): 41–74.

Borger.dk. 2022*a*. "Barsel for lønmodtagere: Barn født den 2. august 2022 eller senere." At *Borger.dk—Parental Leave Rules for Wage Earners: Child Born on August 2, 2022 or Later*. Available at: https://www.borger.dk/familie-og-boern/barsel-oversigt/barsel-loenmodtagere/barsel-loenmodtagere-ny-orlovsmodel (Accessed: December 14, 2025).

Borger.dk. 2022*b*. "Barselsregler for lønmodtagere: Barn født før den 2. august 2022." At *Borger.dk—Parental Leave Rules for Wage Earners: Child Born Before August 2, 2022*. Available at: https://www.borger.dk/familie-og-boern/barsel-oversigt/barsel-loenmodtagere/Barsel-loenmodtagere (Accessed: December 14, 2025).

Brenøe, Anne, Serena Canaan, Nikolaj Harmon, and Heather Royer. 2024. "Is Parental Leave Costly for Firms and Coworkers?" *Journal of Labor Economics,*, 42(4): 1135–1174.

Cantoni, Davide, Yuyu Chen, David Yang, Noam Yuchtman, and Jane Zhang. 2017. "Curriculum and Ideology." *Journal of Political Economy*, 125(2): 338–392.

Dahl, Gordon, Katrine Løken, Magne Mogstad, and Kari Salvanes. 2016. "What is the Case for Paid Maternity Leave?" *Review of Economics and Statistics*, 98(4): 655–670.

Dahl, Gordon, Katrine V. Løken, and Magne Mogstad. 2014. "Peer Effects in Program Participation." *American Economic Review*, 104(7): 2049–2074.

Dechezleprêtre, Antoine, Adrien Fabre, Tobias Kruse, Bluebery Planterose, Ana Sanchez Chico, and Stefanie Stantcheva. 2025. "Fighting Climate Change: International Attitudes toward Climate Policies." *American Economic Review*, 115(4): 1258–1300.

Dhar, Diva, Tarun Jain, and Seema Jayachandran. 2022. "Reshaping Adolescents' Gender Attitudes: Evidence from a School-Based Experiment in India." *American Economic Review*, 112(3): 899–927.

Dutz, Deniz, Ingrid Huitfeldt, Santiago Lacouture, Magne Mogstad, Alexander Torgovitsky, and Winnie van Dijk. 2026. "Selection in Surveys: Using Randomized Incentives to Detect and Account for Nonresponse Bias." *Review of Economic Studies*, 93(2): 1–47.

Ekberg, John, Rickard Eriksson, and Guido Friebel. 2013. "Parental Leave—A Policy Evaluation of the Swedish "Daddy-Month" Reform." *Journal of Public Economics*, 97: 131–143.

European Parliament and the Council of the European Union. 2019. "Directive (EU) 2019/1158 of the European Parliament and of the Council." *Official Journal of the European Union*, L 188/79.

Farré, Lídia, and Libertad González. 2019. "Does Paternity Leave Reduce Fertility?" *Journal of Public Economics*, 172(1): 52–66.

Fernández, Raquel, Alessandra Fogli, and Claudia Olivetti. 2004. "Mothers and Sons: Preference Formation and Female Labor Force Dynamics." *Quarterly Journal of Economics*, 119(4): 1249–1299.

Fernández, Raquel, and Alessandra Fogli. 2009. "Culture: An Empirical Investigation of Beliefs, Work, and Fertility." *American Economic Journal: Macroeconomics*, 1(1): 146–177.

Fouka, Vasiliki. 2020. "Backlash: The Unintended Effects of Language Prohibition in U.S. Schools after World War I." *Review of Economic Studies*, 87(1): 204–239.

Gallen, Yana, Juanna Joensen, Eva Johansen, and Gregory Veramendi. 2025. "The Labor Market Returns to Delaying Pregnancy." Working Paper.

González, Libertad, and Hosny Zoabi. 2025. "Does Paternity Leave Promote Gender Equality Within Households?" *Journal of the European Economic Association*, forthcoming.

Jørgensen, Thomas, and Jakob Søgaard. 2024. "The Division of Parental Leave: Empirical Evidence and Policy Design." *Journal of Public Economics*, 238.

Kleven, Henrik. 2025. "The Geography of Child Penalties and Gender Norms: A Pseudo-Event Study Approach." *American Economic Journal: Applied Economics*, forthcoming.

Kleven, Henrik, Camille Landais, and Gabriel Leite-Mariante. 2025. "The Child Penalty Atlas." *Review of Economic Studies*, 92(5): 3174–3207.

Kleven, Henrik, Camille Landais, and Jakob Søgaard. 2019. "Children and Gender Inequality: Evidence from Denmark." *American Economic Journal: Applied Economics*, 11(4): 181–209.

Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. 2019. "Child Penalties Across Countries: Evidence and Explanations." *AEA Papers and Proceedings*, 109: 122–126.

Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. 2024. "Do Family Policies Reduce Gender Inequality? Evidence from 60 Years of Policy Experimentation." *American Economic Journal: Economic Policy*, 16: 110–149.

Kuziemko, Ilyana, Michael Norton, Emmanuel Saez, and Stefanie Stantcheva. 2015. "How Elastic Are Preferences for Redistribution? Evidence from Randomized Survey Experiments." *American Economic Review*, 105(4): 1478–1508.

Lalive, Rafael, and Josef Zweimüller. 2009. "How Does Parental Leave Affect Fertility and Return to Work? Evidence from Two Natural Experiments." *Quarterly Journal of Economics*, 124(3): 1363–1402.

Lassen, Anne. 2023. "Gender Norms and Specialization in Household Production: Evidence from a Danish Parental Leave Reform." Working Paper.

Olafsson, Arna, and Herdis Steingrimsdottir. 2020. "How Does Daddy at Home Affect Marital Stability?" *Economic Journal*, 130(629): 1471–1500.

Olivetti, Claudia, Jessica Pan, and Barbara Petrongolo. 2024. "The Evolution of Gender in the Labor Market." In *Handbook of Labor Economics*. Vol. 5, ed. Christian Dustmann and Thomas Lemieux, Chapter 8. Elsevier: Amsterdam.

Patnaik, Ankita. 2019. "Reserving Time for Daddy: The Consequences of Fathers' Quotas." *Journal of Labor Economics*, 37(4): 1009–1059.

Schönberg, Uta, and Johannes Ludsteck. 2014. "Expansions in Maternity Leave Coverage and Mothers' Labor Market Outcomes after Childbirth." *Journal of Labor Economics*, 32(3): 469–505.

Settele, Sonja. 2022. "How Do Beliefs about the Gender Wage Gap Affect the Demand for Public Policy?" *American Economic Journal: Economic Policy*, 14(2): 475–508.

Stantcheva, Stefanie. 2021. "Understanding Tax Policy: How do People Reason?" *Quarterly Journal of Economics*, 136(4): 2309–2369.

TABLE 1: DESCRIPTIVE STATISTICS

Baseline Outcomes	Full Population of Parents	Survey Respondents	
		Unweighted	Weighted
Married	0.51	0.54	0.51
College Degree	0.62	0.73	0.62
High-School Degree or Less	0.14	0.09	0.13
Live in Copenhagen	0.28	0.27	0.28
Hours Worked	1682	1694	1680
Household Earnings	864K	887K	858K
Mom's Share of Household Earnings	0.41	0.43	0.42
First-Time Parent	0.44	0.47	0.44
Age at Birth	32.61	32.61	32.67
Number of Observations	174,466	38,494	38,494

Notes: This table reports descriptive statistics for the full population of parents (conditional on reform eligibility) and for survey respondents. It considers both the unweighted and weighted samples of survey respondents, where the weights have been estimated using the specification described in Section 3.3. Parents with children born within one year of the paternity-leave reform—between August 1, 2021 and July 31, 2023—are included in the samples. Demographics variables are measured on January 1 of the year the child is born, while labor market outcomes are measured over the 12 months preceding childbirth.

TABLE 2: WHO ARE THE COMPLIERS?

Baseline Outcomes	Population	Paternity Leave ≥ 11 Weeks		Compliers
		Before Reform	After Reform	
Married	0.51	0.54	0.53	0.52
College Degree	0.62	0.84	0.70	0.61
Live in Copenhagen	0.28	0.42	0.31	0.23
Household Earnings	864K	980K	901K	849K
Mom's Share of Household Earnings	0.41	0.43	0.42	0.41
Agree Child Suffers if Mom Works	0.36	0.23	0.33	0.39
Moms Are Better at Childcare (0-6m)	0.78	0.72	0.77	0.81
Moms Are Better at Childcare (6-9m)	0.66	0.58	0.64	0.68

Notes: This table compares average outcomes in the full population of parents (who had a child within one year of the reform) to average outcomes among compliers, defined as those induced to increase paternity leave to at least eleven weeks (full take-up) due to the reform. Compliers are characterized using the approach described in Section 4.3. The intuition behind the approach is that families who take at least 11 weeks of paternity leave before the reform are always-takers, while those who take at least 11 weeks of paternity leave after the reform are a mix of always-takers and compliers. Hence, any differences between these two groups must be driven by compliers, the outcomes of whom can then be isolated using equation (3). The table reports outcomes from both the administrative data and the survey data (first wave), reweighting the survey sample to match the full population as described above. The results show that compliers are highly representative of the general population of parents.

TABLE 3: IV ESTIMATES AND COUNTERFACTUAL POLICY REGIMES

	IV Estimate	Baseline	Counterfactuals		
			0.00	0.25	0.50
First Stage:					
Fathers' Share of Leave	0.081 (0.003)	0.124	0.00	0.25	0.50
F-Statistic	625.3				
Second Stage for Mothers:					
Gender-Beliefs Index	-0.286 (0.055)	0.615	0.650	0.579	0.507
A Pre-School Child Suffers If Mom Works Full Time	-0.375 (0.114)	0.465	0.512	0.418	0.324
Better If Mom Takes Most of the Leave	-0.477 (0.106)	0.636	0.695	0.576	0.457
Moms Have Stronger Bonds With Their Children	-0.194 (0.102)	0.581	0.605	0.556	0.508
Second Stage for Fathers:					
Gender-Beliefs Index	-0.240 (0.075)	0.599	0.628	0.568	0.508
A Pre-School Child Suffers If Mom Works Full Time	-0.278 (0.166)	0.389	0.423	0.354	0.285
Better If Mom Takes Most of the Leave	-0.560 (0.162)	0.567	0.636	0.496	0.356
Moms Have Stronger Bonds With Their Children	-0.243 (0.153)	0.535	0.565	0.505	0.444

Notes: This table reports IV estimates of the effect of fathers' parental-leave share on gender beliefs. Estimates are based on the specification described in Section 4.1, with standard errors clustered at the child level. The top panel presents the first-stage effect of the reform on fathers' leave share, which increases by 0.081 from a baseline of 0.124. The corresponding F-statistic is 625.3. The subsequent panels report second-stage estimates—first for mothers and then for fathers—on the gender-beliefs index and three of its underlying components. The effects are sizable and highly consistent across parents and outcomes. The final three columns use the IV estimates to predict outcomes under counterfactual policy regimes with fathers' leave shares of 0%, 25%, and 50%. Moving to an equal-leave scenario would reduce the gender-beliefs index from about 0.6 to about 0.5—the threshold demarcating gender-neutral views. Some of the underlying components of the index fall to values well below 0.5, corresponding to strongly gender-progressive views.

TABLE 4: MEDIATION ANALYSIS

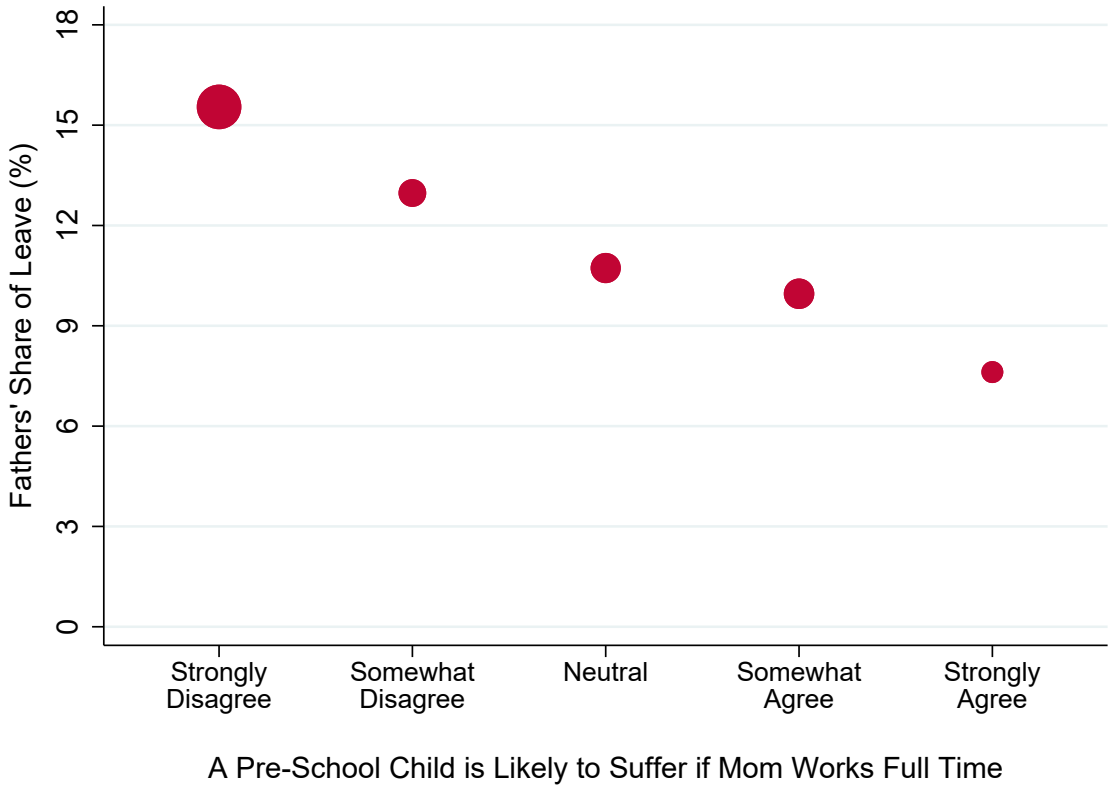
SHARE OF EARNINGS EFFECT EXPLAINED BY BELIEFS AND CHILDCARE

	Gender Gap in Earnings				
Pre-Reform Associations:					
Gender-Beliefs Index	0.565	0.435	0.353		0.225
	(0.048)	(0.047)	(0.052)		(0.051)
Policy-Support Index			-0.136		-0.096
			(0.032)		(0.032)
Childcare Gap				1.031	0.929
				(0.058)	(0.058)
Effects of Paternity-Leave Reform:					
Total Effect on Gender Gap	-0.028	-0.028	-0.028	-0.028	-0.028
Effect Through Beliefs and Childcare	-0.012	-0.009	-0.015	-0.018	-0.027
Share Explained by Beliefs and Childcare	44%	34%	54%	66%	96%
Controls:					
Education		×	×	×	×
Occupation		×	×	×	×
Industry		×	×	×	×
Age		×	×	×	×

Notes: This table examines the extent to which the reform-induced reduction in the earnings gap is mediated by changes in beliefs and childcare. We include beliefs about gender roles (gender-beliefs index), beliefs about earmarked paternity leave (policy-support index), and the actual childcare allocation for mothers relative to fathers (childcare gap). The first panel reports pre-reform associations between the earnings gap and the different beliefs and childcare variables, while the second panel links the pre-reform associations with our RD estimates of reform effects on the earnings gap, beliefs, and childcare. When estimating pre-reform associations, we consider specifications with and without controls for potentially confounding determinants of labor market outcomes. The controls include education (6 categories), occupation (5 categories), industry (5 categories), and age (2nd-order polynomial) for each parent. The first panel shows that a larger value of the gender-beliefs index (more conservative views) predicts a larger earnings gap, that a larger value of the policy-support index (more progressive views) predicts a smaller earnings gap, and that a larger childcare gap between mothers and fathers predicts a larger earnings gap. Using these correlations, the second panel shows how much of the reform-induced change in the earnings gap can be explained by the reform-induced changes in beliefs and childcare division. In the most saturated specification (last column), changes in beliefs and childcare account for 96% of the total effect of the reform on the gender gap in earnings.

FIGURE 1: FATHERS' LEAVE SHARE VS GENDER NORMS

PRE-REFORM BASELINE

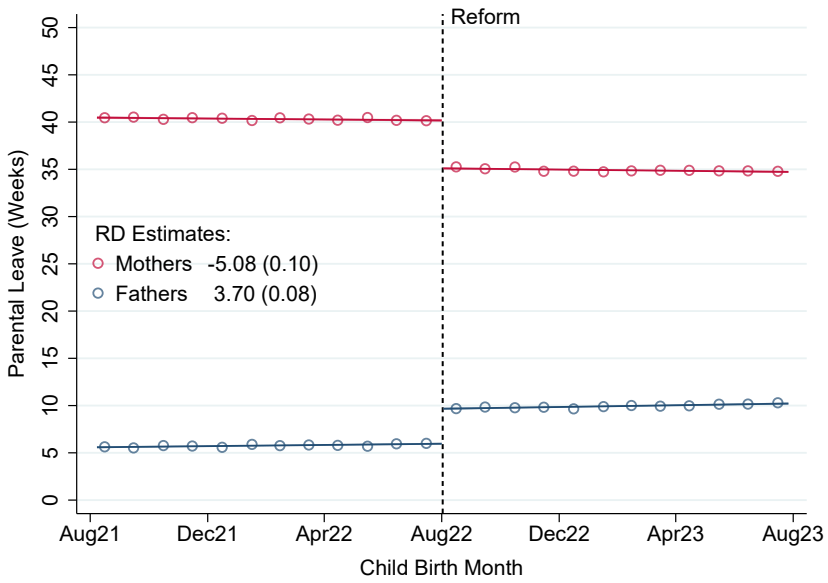


Notes: This figure plots fathers' share of parental leave against elicited agreement with the following statement: "A pre-school child is likely to suffer if their mother works full time." Survey responses are taken from the first wave. The sample is restricted to the pre-reform period and includes both mothers and fathers, reweighted using the specification described in Section 3.3. The size of the dots represents the number of individuals in each category. The relationship between fathers' leave taking and gender-conservative beliefs is strongly negative.

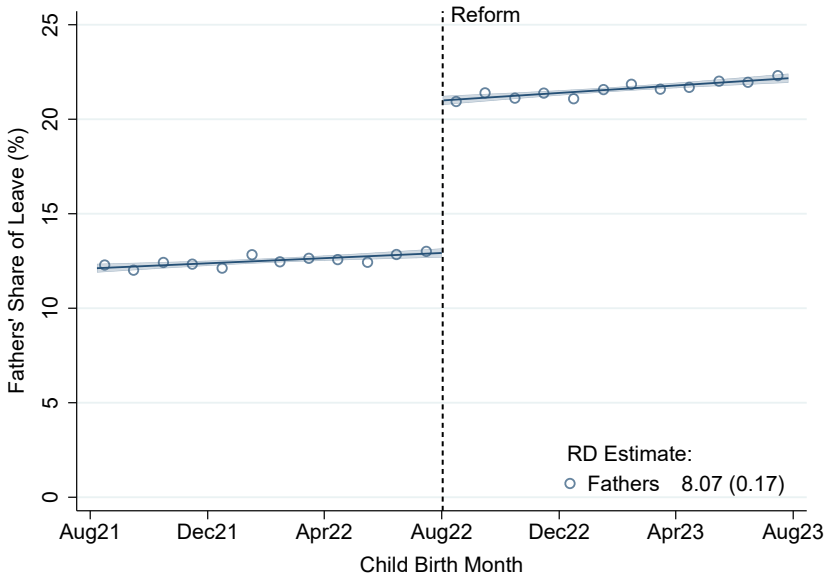
FIGURE 2: FIRST STAGE

IMPACT ON LEAVE DURATION

A. Leave Duration for Mothers and Fathers



B. Fathers' Share of Leave

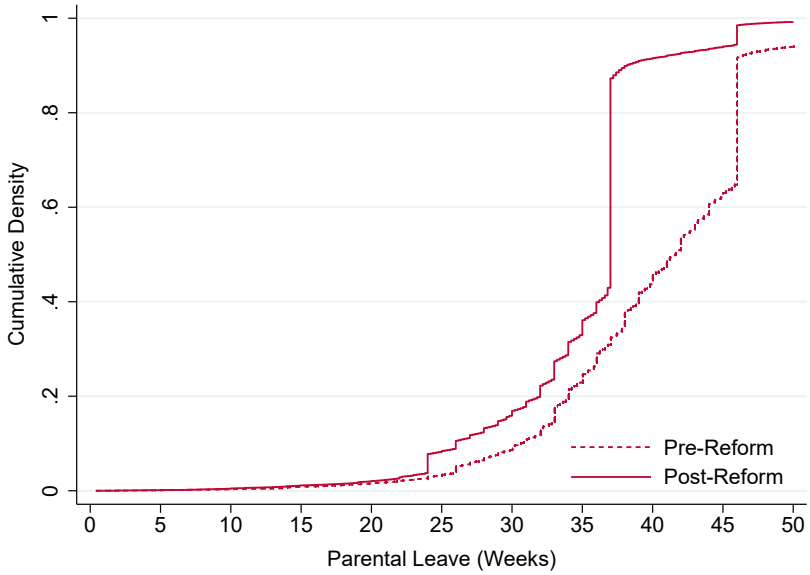


Notes: This figure presents RD evidence on the first-stage effects of the reform on leave duration for mothers and fathers (Panel A) and on fathers' share of household leave (Panel B). The trend lines and RD coefficients are estimated using equation (1). The reform increases fathers' leave by 3.7 weeks and reduces mothers' leave by about 5 weeks. As a result, fathers' leave share increases by 8.1pp—from 12.4% before the reform to 20.5% after the reform. Standard errors are clustered at the child level.

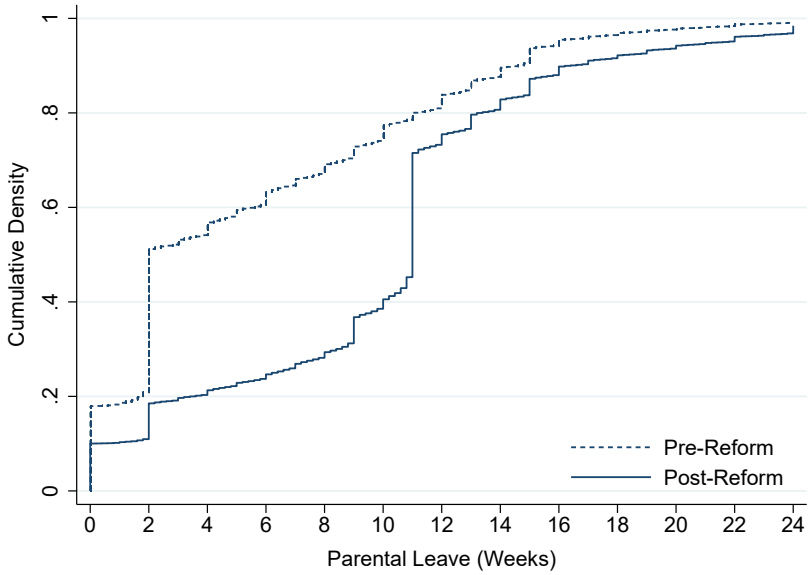
FIGURE 3: FIRST STAGE

CUMULATIVE DISTRIBUTIONS OF LEAVE DURATION

A. Mothers

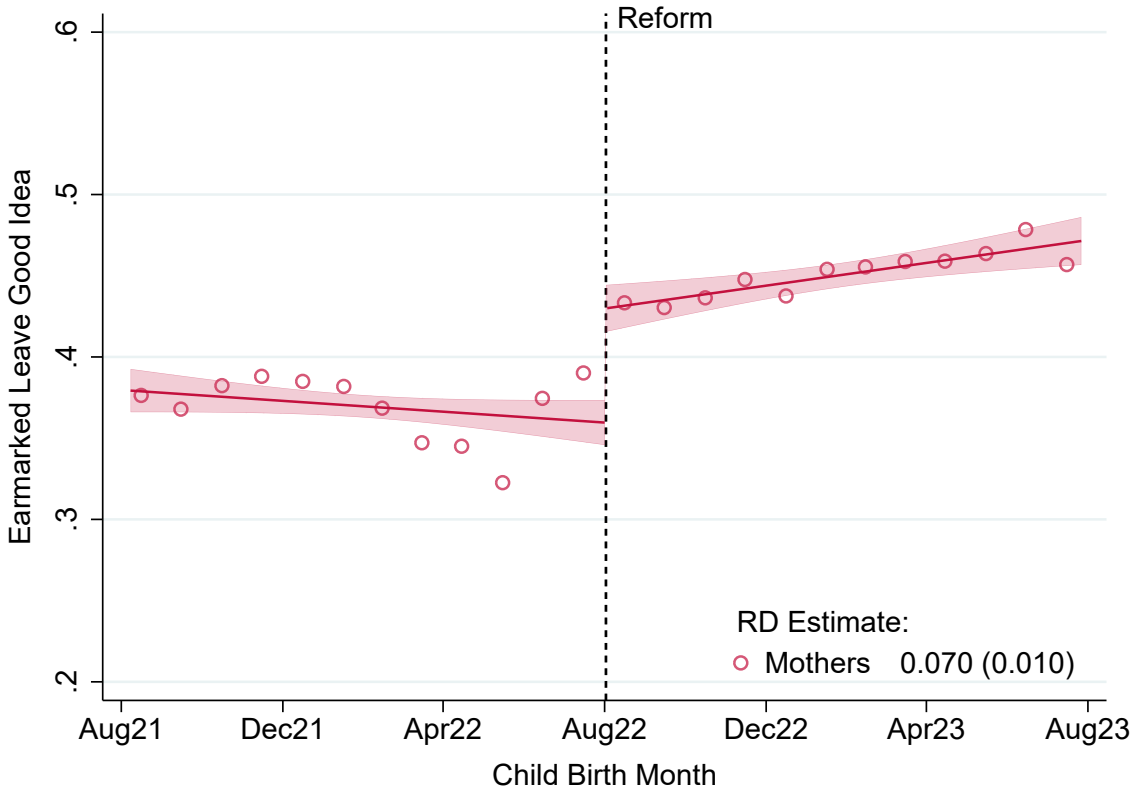


B. Fathers



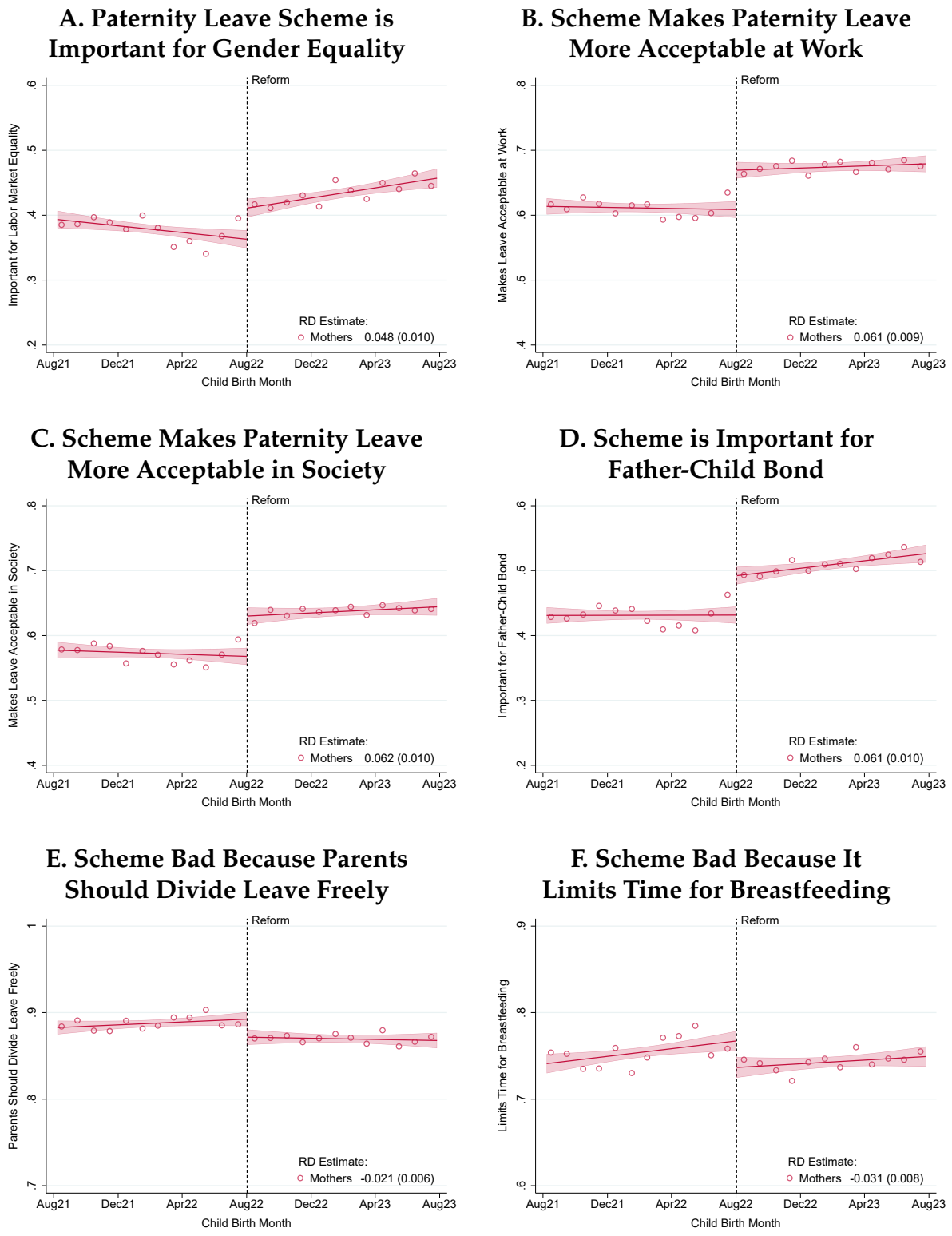
Notes: This figure shows first-stage effects of the reform on the cumulative distribution of leave for mothers (Panel A) and fathers (Panel B). In each panel, the pre-reform (post-reform) distribution includes parents who have children during the 12-month window before (after) the reform cutoff date. The reform shifts the distribution to the left for mothers and to the right for fathers. For mothers, the distribution exhibits a discontinuity at 46 weeks before the reform and at 37 weeks afterward, corresponding to the maximum amount of paid maternity leave under the two regimes. For fathers, the distribution displays a discontinuity at 2 weeks before the reform and at 11 weeks afterward, reflecting the amount of earmarked paternity leave under the two regimes.

FIGURE 4: EARMARKED PATERNITY LEAVE IS A GOOD IDEA



Notes: This figure presents RD evidence on the effect of the reform on whether mothers agree with the statement that "earmarked paternity leave is a good idea." As described in Section 4.1, we account for belief intensity by coding the five response categories as 0.00 (strongly disagree), 0.25 (disagree), 0.50 (neither agree nor disagree), 0.75 (agree), and 1.00 (strongly agree). The trend lines and RD coefficient are estimated using equation (1). Standard errors are clustered at the child level.

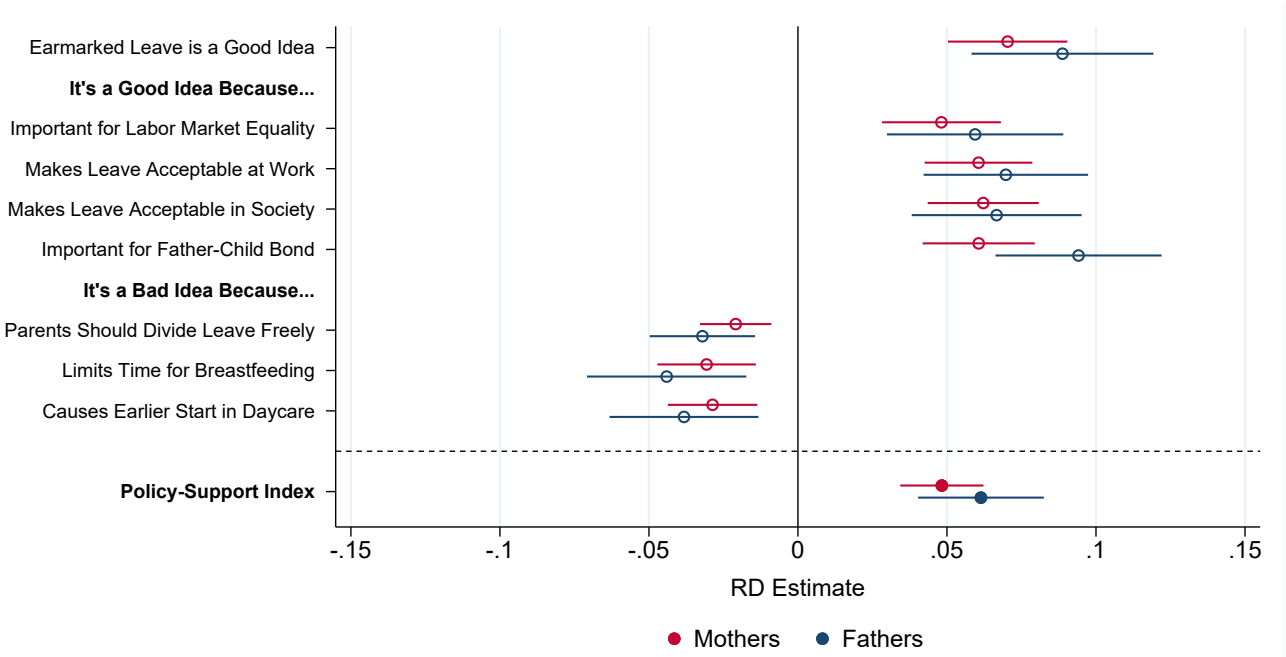
FIGURE 5: BELIEFS ABOUT PATERNITY LEAVE POLICY



Notes: This figure presents RD evidence on the effects of the reform on mothers' beliefs about earmarked paternity leave. Each panel is constructed in the same way as Figure 4. Panels A-D focus on perceived benefits, while Panels E-F focus on perceived costs. The findings are consistent across outcomes: respondents are skeptical of the policy at baseline—agreeing more strongly with its costs than its benefits—but treatment reduces this skepticism significantly. The strongest effects are found for the following beliefs: that the policy makes paternity leave more acceptable in the workplace, that it makes paternity leave more acceptable in society, and that it is important for the father-child bond. Standard errors are clustered at the child level.

FIGURE 6: BELIEFS ABOUT PATERNITY LEAVE POLICY

SUMMARY OF ESTIMATES

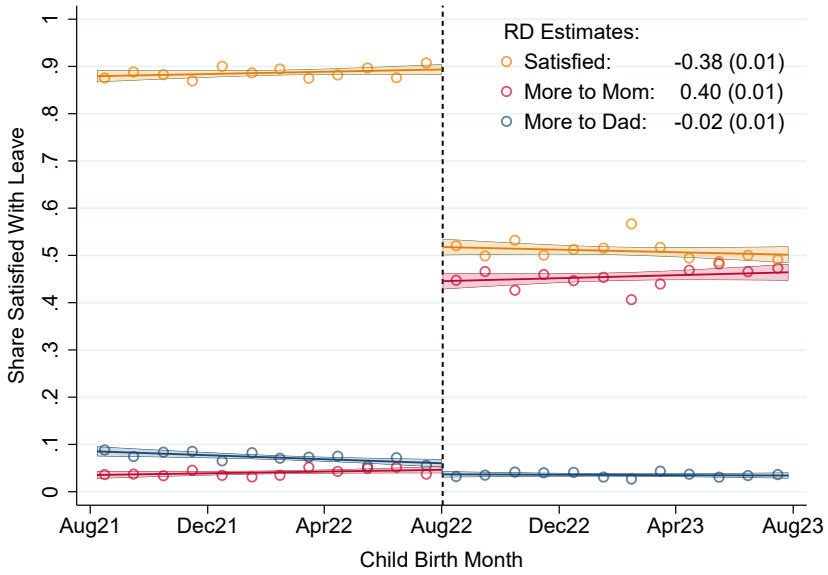


Notes: This figure summarizes our RD estimates of the effects of the reform on beliefs about earmarked paternity leave, showing estimates for mothers in red and fathers in blue. Across all outcomes and for both parents, treatment increases the perceived benefits of earmarked leave and reduces the perceived costs. The figure also reports effects on a policy-support index—capturing average effects across all outcomes in this category—reversing the numerical coding of perceived costs to ensure comparability with perceived benefits. The average effects are sizable—about 0.05 for mothers and about 0.06 for fathers—and precisely estimated. Standard errors are clustered at the child level.

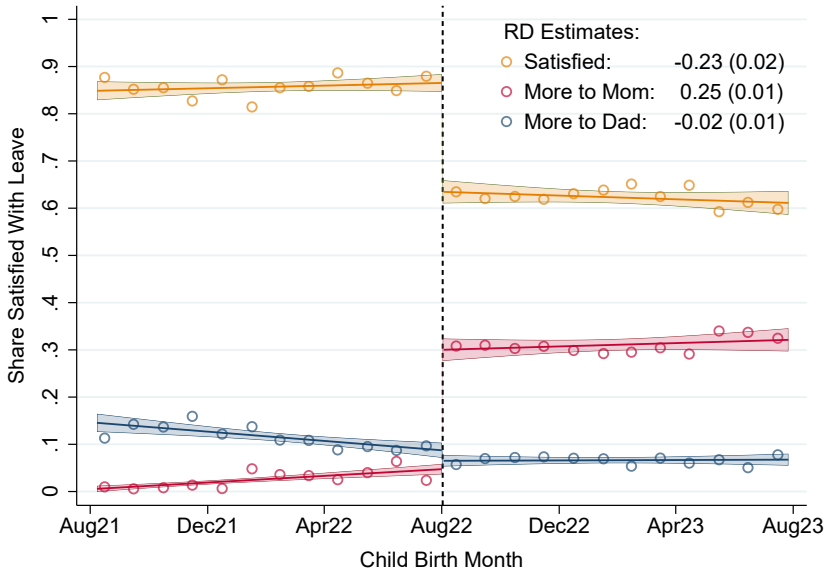
FIGURE 7: LEAVE SATISFACTION

MOTHERS AND FATHERS BECOME LESS SATISFIED

A. Mothers

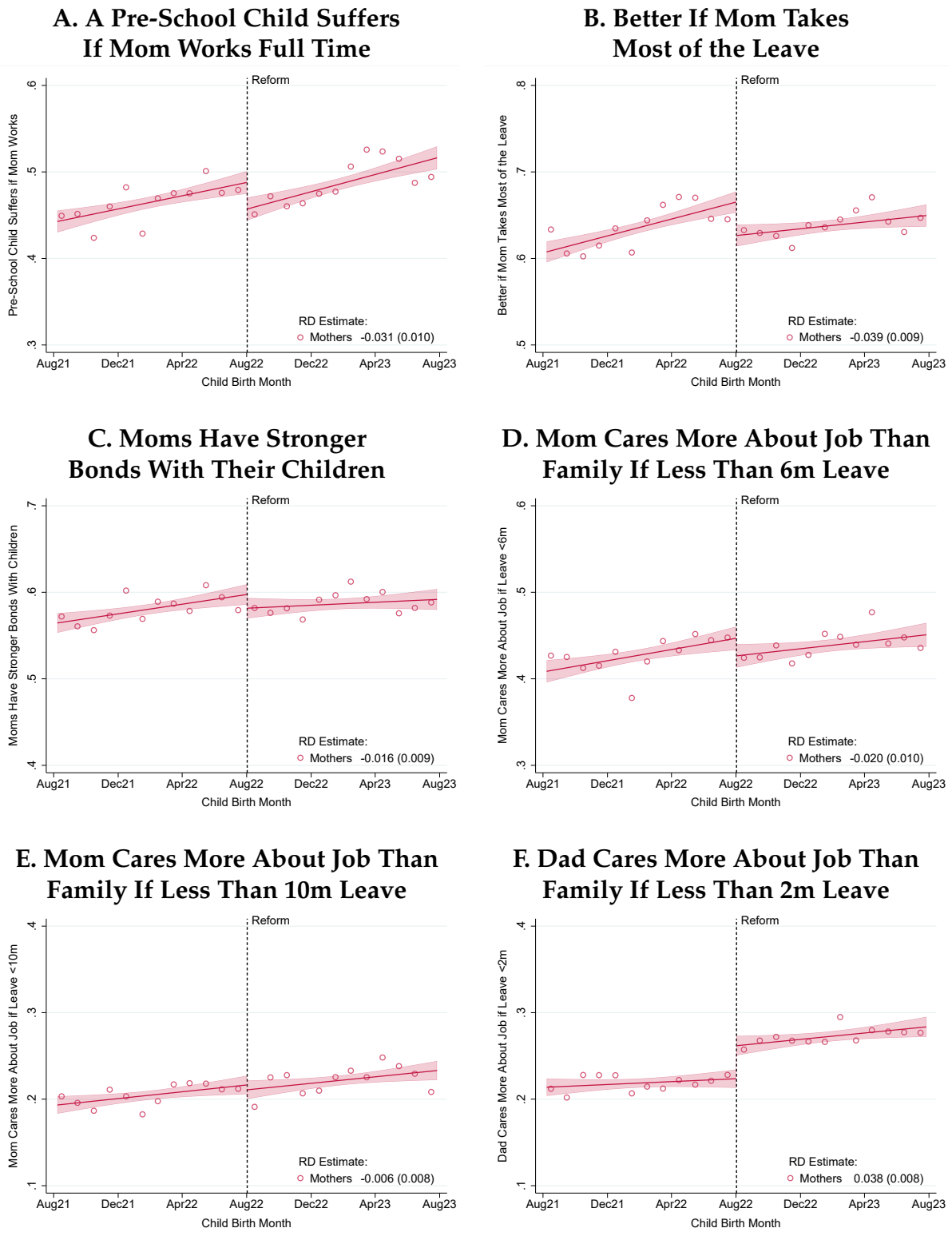


B. Fathers



Notes: This figure presents RD evidence on the effects of the reform on leave satisfaction (yellow dots) and on preferences for reallocating leave toward mothers (red dots) or fathers (blue dots). Panel A reports mothers' responses and Panel B reports fathers' responses, elicited in the first wave of the survey. About 90% of both mothers and fathers were satisfied with their leave allocation prior to the reform, but this share falls to 50% for mothers and 60% for fathers after the reform. Both groups attribute their dissatisfaction to the same concern: a preference for allocating more leave to mothers rather than reducing leave for fathers.

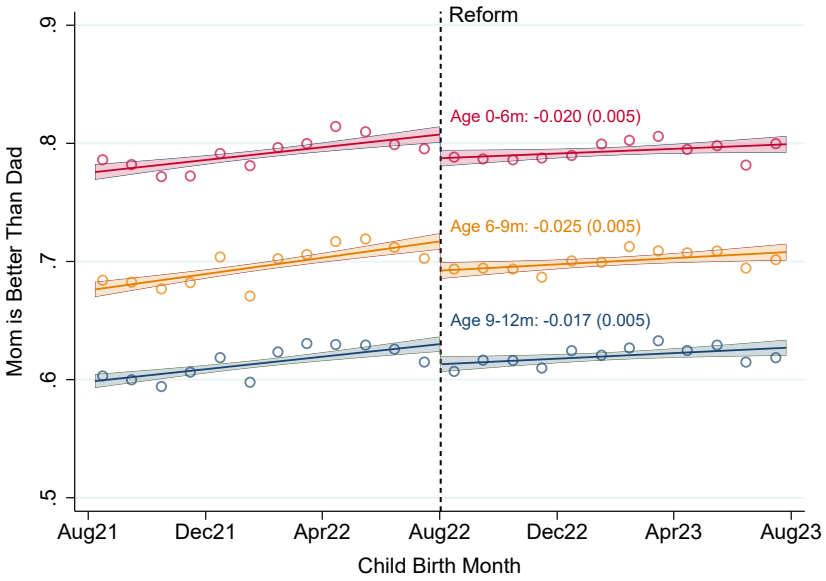
FIGURE 8: BELIEFS ABOUT GENDER ROLES



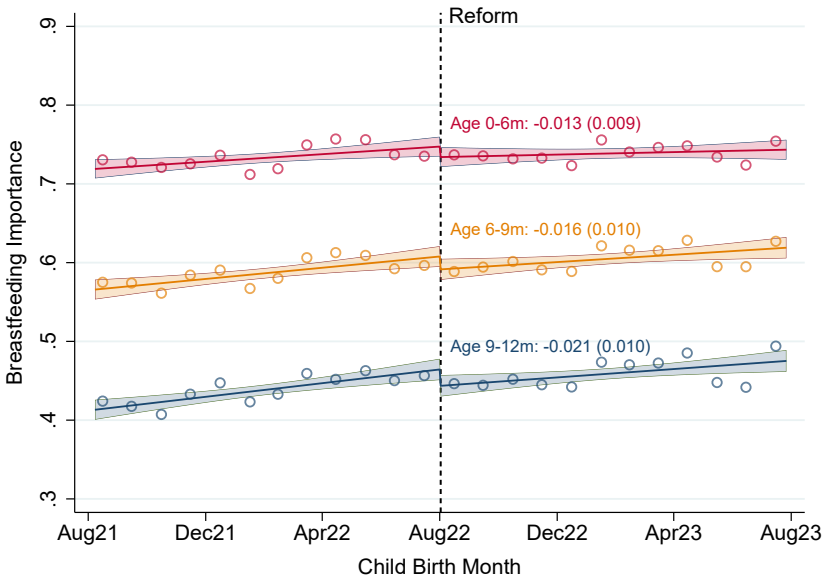
Notes: This figure presents RD evidence on the effects of the reform on gender-role beliefs among mothers. The figure considers six gender-role questions, with responses coded from zero (strongly disagree) to one (strongly agree). Trend lines and RD coefficients are estimated using equation (1), with standard errors clustered at the child level. The paternity-leave expansion shifts beliefs in a more gender-progressive direction, reducing, for example, agreement with the statements that a pre-school child suffers if their mother works full time (Panel A), that mothers should take most of the leave (Panel B), and that mothers have stronger bonds with their children than fathers (Panel C).

FIGURE 9: BELIEFS ABOUT CHILDCARE

A. Moms are Better at Childcare Than Dads



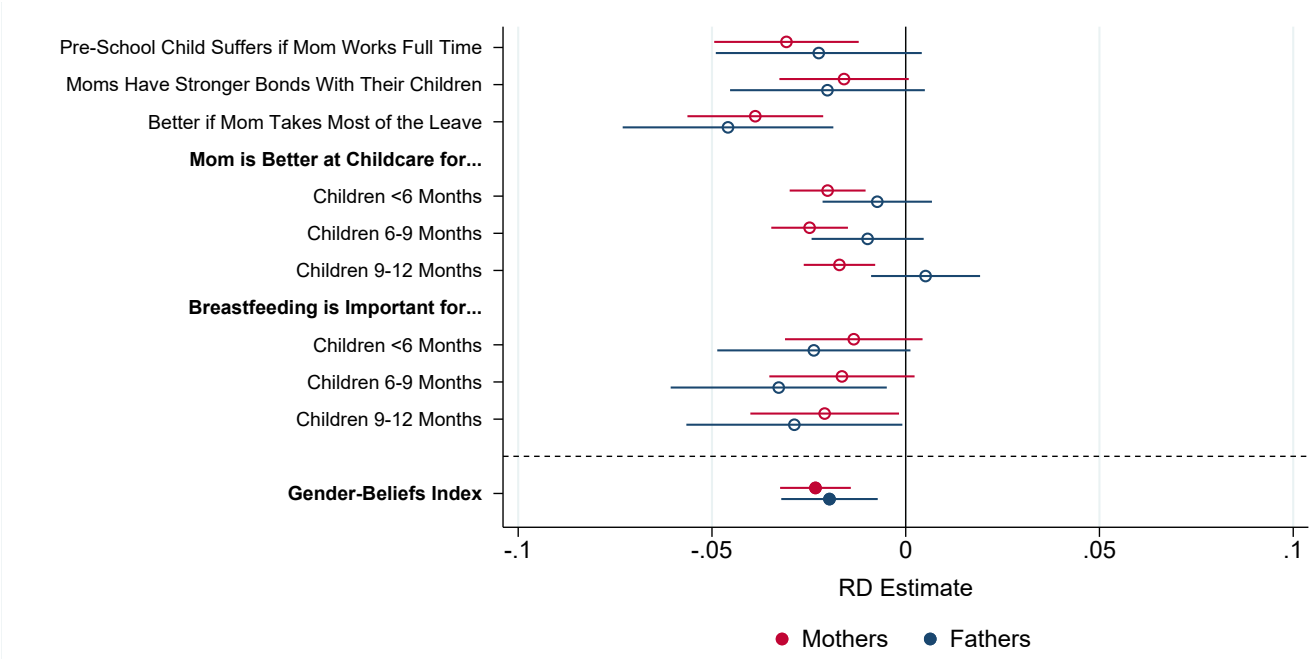
B. Breastfeeding is Important



Notes: This figure presents RD evidence on the effects of the reform on childcare beliefs among mothers. Panel A focuses on mothers' perceptions of relative childcare ability, while Panel B focuses on their beliefs about the importance of breastfeeding. For childcare ability, respondents select one of five categories, coded from 0 (fathers are much better) to 1 (mothers are much better), with 0.5 indicating equal ability. For breastfeeding importance, responses are likewise coded from 0 (strongly disagree) to 1 (strongly agree). Each question is answered separately for children aged 0-6 months, 6-9 months, and 9-12 months. The trend lines and RD coefficients are estimated using equation (1), with standard errors clustered at the child level. Mothers generally view themselves as better at childcare and regard breastfeeding as important, but the reform reduces both perceptions.

FIGURE 10: BELIEFS ABOUT GENDER ROLES AND CHILDCARE

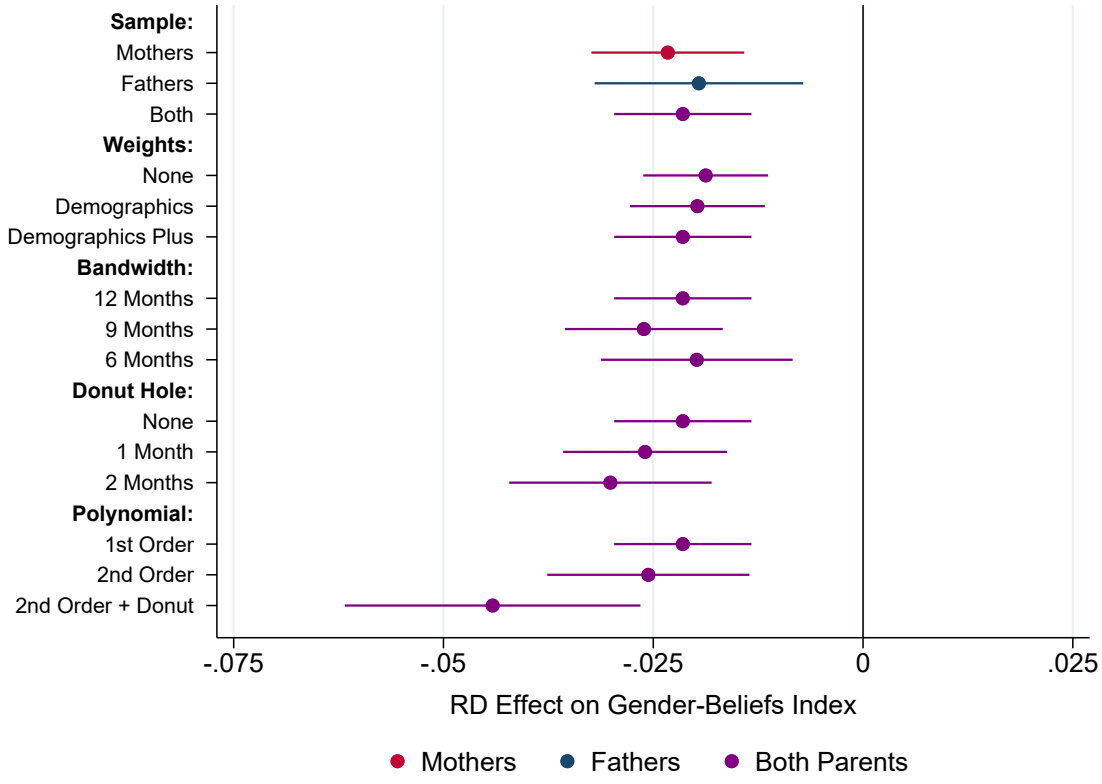
SUMMARY OF ESTIMATES



Notes: This figure summarizes our RD estimates of the effects of the reform on gender-role and childcare beliefs, showing estimates for mothers in red and fathers in blue. The findings are consistent across outcomes and groups: treatment reduces beliefs in traditional gender-role and childcare models. The figure also reports effects on a gender-beliefs index—capturing average effects across all outcomes in this category—which are sizable and similar in magnitude for mothers and fathers. Standard errors are clustered at the child level.

FIGURE 11: ROBUSTNESS OF ESTIMATES

GENDER-BELIEFS INDEX



Notes: This figure examines the robustness of our RD estimates of the effects on the gender-beliefs index. Starting with the baseline estimates for mothers (red), fathers (blue), and both parents pooled (purple), the figure reports pooled estimates under alternative specifications of sample weights, bandwidths, donut holes, and polynomial orders of the trend functions. The results are highly robust: the estimates are consistently negative and statistically significant, and their magnitudes remain stable across most specifications. Standard errors are clustered at the child level.

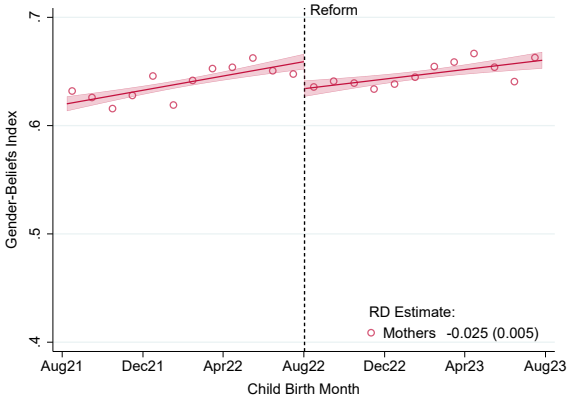
FIGURE 12: HETEROGENEOUS TREATMENT EFFECTS

GENDER-BELIEFS INDEX

Heterogeneity by Reform Exposure

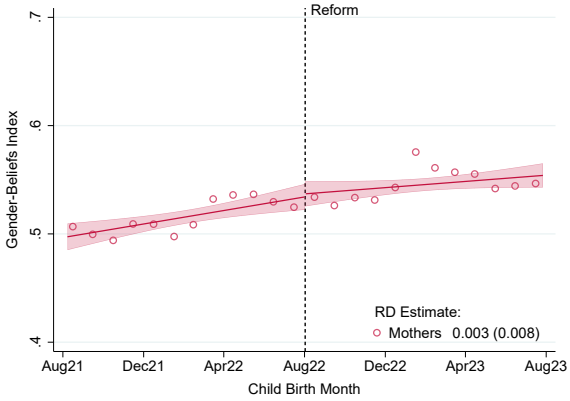
A. Exposed

Paternity Leave < 12 Weeks



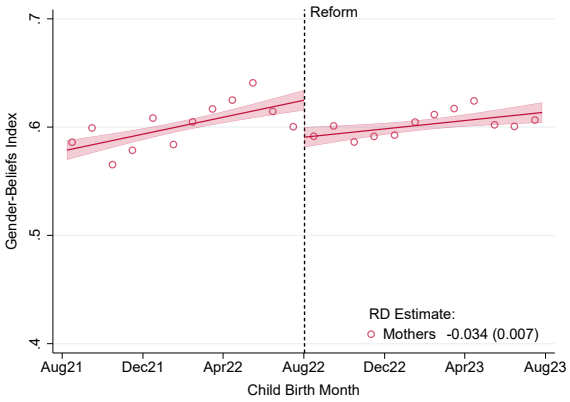
B. Unexposed

Paternity Leave ≥ 12 Weeks

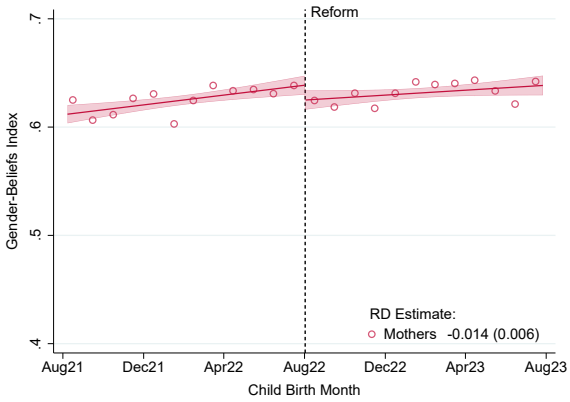


Heterogeneity by Birth Order

C. First-Time Births



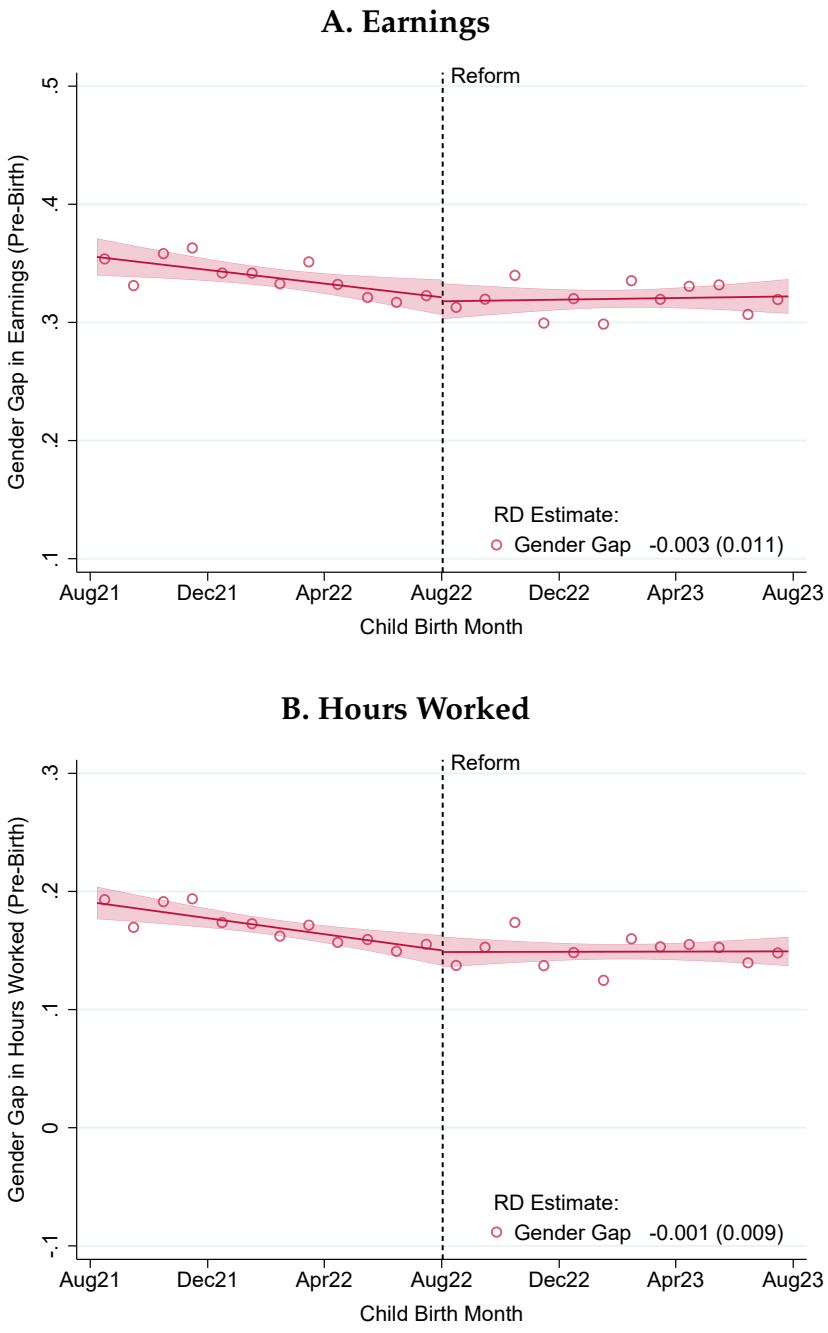
D. Higher-Parity Births



Notes: This figure examines heterogeneity in the effects of the reform on mothers' gender beliefs. The top row compares exposed and unexposed families, where "unexposed" families are defined as those taking more than twelve weeks of paternity leave regardless of treatment assignment. Because the reform expands earmarked paternity leave to eleven weeks, families taking strictly more than this amount are defined as always-takers and should not respond to the reform. Consistent with this, the effects on gender beliefs are driven entirely by exposed families. The bottom row compares families with first births to those with higher-parity births. The effects on gender beliefs are substantially larger for first-time parents, suggesting that the steady-state effects of the reform may exceed our baseline estimates. Standard errors are clustered at the child level.

FIGURE 13: EFFECTS ON PRE-BIRTH GENDER GAPS

PLACEBO TESTS

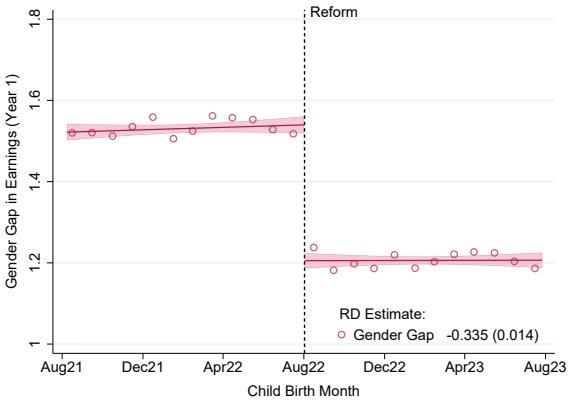


Notes: This figure provides RD evidence on the effects of the reform on pre-birth gender gaps, measured over the twelve months prior to childbirth. Because the reform does not affect pre-birth incentives, any estimated effects would indicate selection around the birth-date cutoff. The outcome variable is defined as the log difference between male and female outcomes—earnings or hours worked—within the household. The placebo effects on both earnings (Panel A) and hours (Panel B) are precisely estimated zeros, confirming the absence of selection. RD coefficients are estimated using equation (1), with standard errors clustered at the child level.

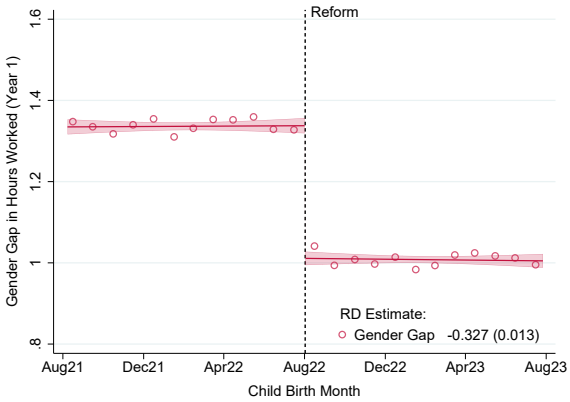
FIGURE 14: EFFECTS ON POST-BIRTH GENDER GAPS

1-12 Months After Childbirth

A. Earnings

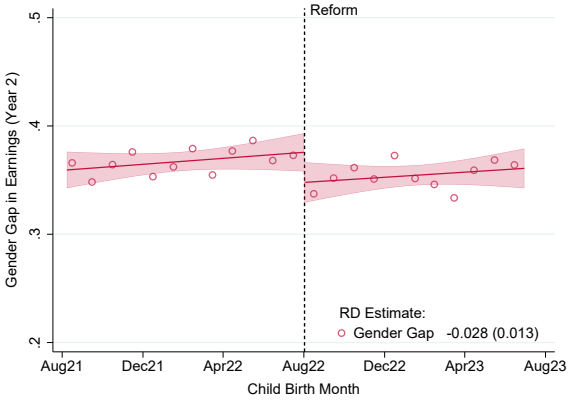


B. Hours Worked

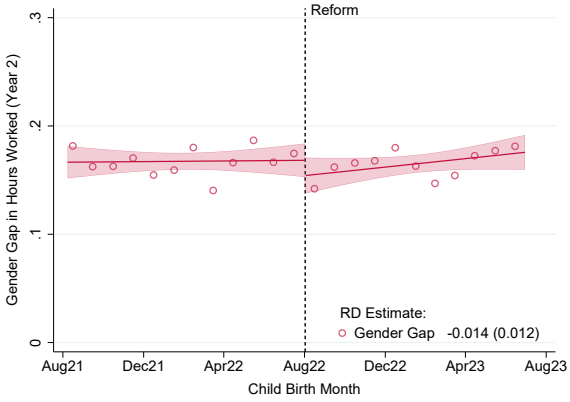


13-24 Months After Childbirth

C. Earnings



D. Hours Worked

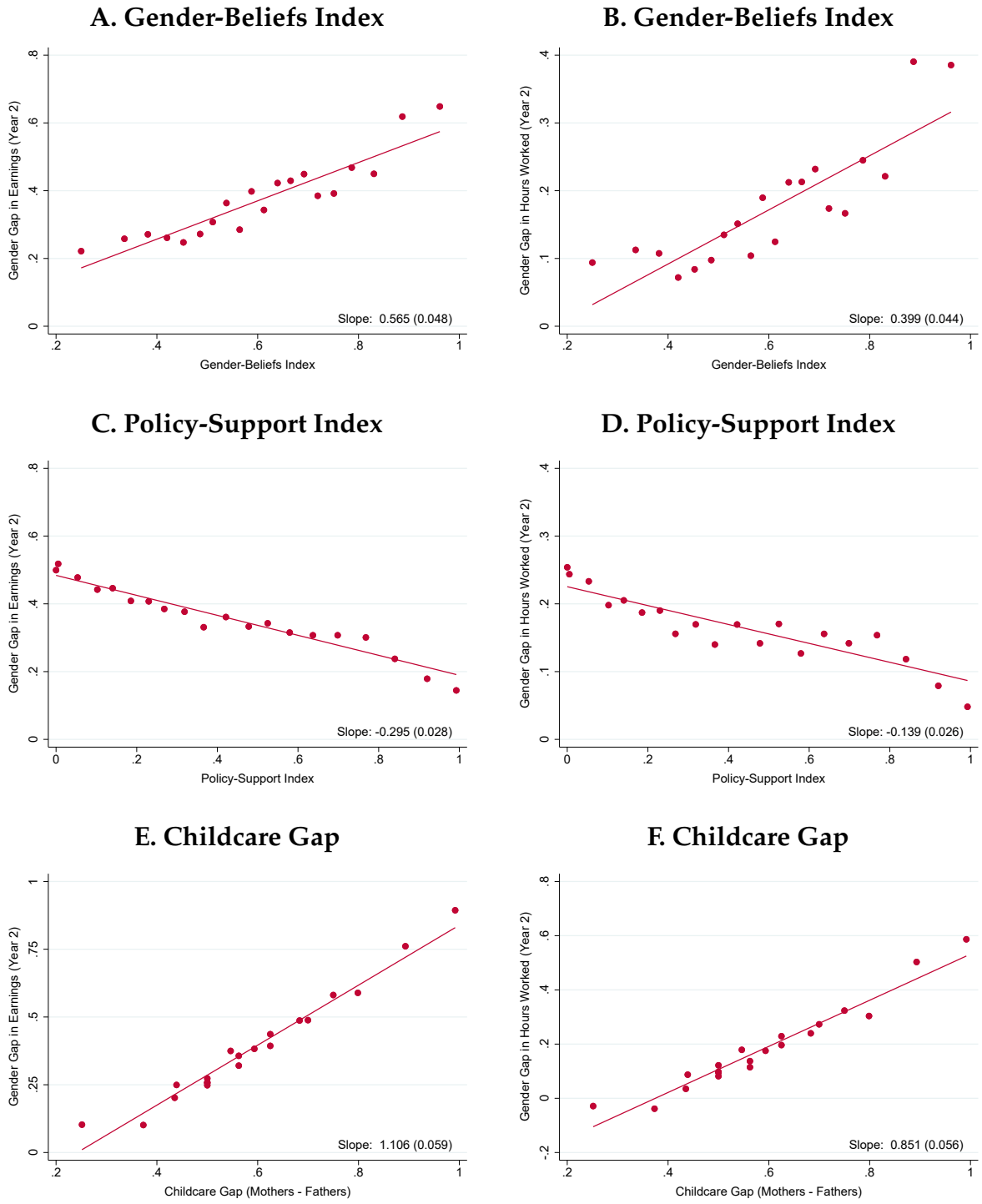


Notes: This figure provides RD evidence on the effects of the reform on post-birth gender gaps, measured over the first year after childbirth (top row) and the second year after childbirth (bottom row). The outcome variable is defined as the log difference between male and female outcomes—earnings or hours worked—within the household. The left panels show earnings and the right panels show hours worked. RD coefficients are estimated using equation (1), with standard errors clustered at the child level. The reform reduces gender gaps in both earnings and hours, with an earnings effect of 33.5pp in the first year (when leave is taken) and 2.8pp in the second year (after leave has ended).

FIGURE 15: BELIEFS AND CHILDCARE PREDICT GENDER GAPS

EARNINGS GAP VS SURVEY OUTCOMES

HOURS GAP VS SURVEY OUTCOMES



Notes: This figure shows pre-reform associations between gender gaps in labor market outcomes—earnings and hours worked—and either beliefs or childcare division. The labor market gaps are defined as the log difference between male and female outcomes within the household, measured in the second year after childbirth. The childcare gap is measured as average childcare for mothers relative to fathers across different tasks. Gender gaps are plotted against the gender-beliefs index in the top row, against the policy-support index in the middle row, and against the childcare gap in the bottom row. Each panel provides a binscatter and reports a slope coefficient estimated from the underlying individual data. We find strong correlations in the expected direction: greater gender conservatism and a greater childcare gap are associated with larger gender gaps in labor market outcomes.

Online Appendix:

"Expanding Paternity Leave: Effects on Beliefs, Norms, and Gender Gaps"

Henrik KLEVEN, Camille LANDAIS, Anne Sophie LASSEN,

Philip ROSENBAUM, Herdis STEINGRIMSDOTTIR, and Jakob SØGAARD

June 2026

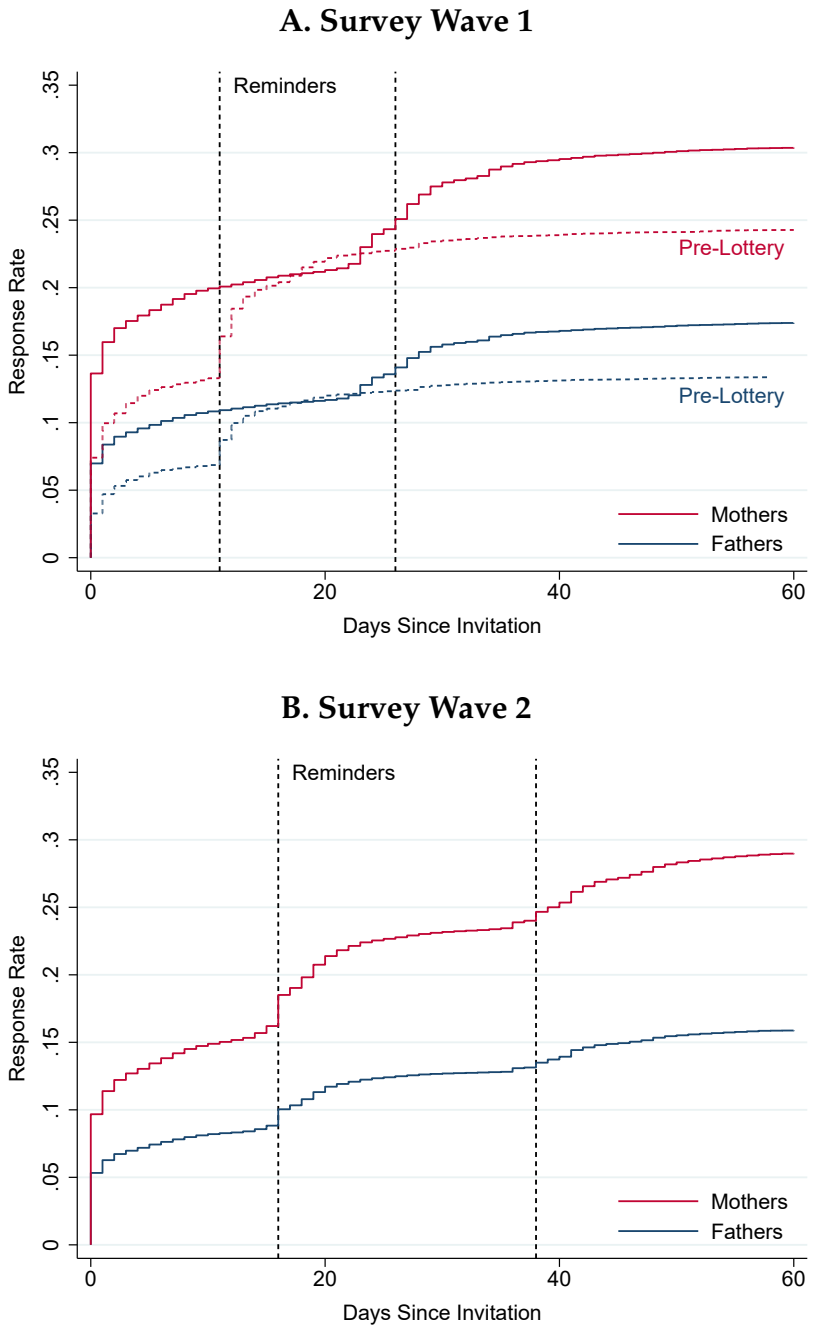
A Supplementary Exhibits

TABLE A.1: DECOMPOSING THE EFFECTS ON POST-BIRTH GENDER GAPS

	Gender Gap Effects		Share of Gender Gap Effects Explained	
	Year 1	Year 2	Year 1	Year 2
Effects on Log Earnings:				
Gender Gap	-0.335 (0.014)	-0.028 (0.013)	100%	100%
Mothers	0.259 (0.013)	0.017 (0.011)	78%	62%
Fathers	-0.075 (0.008)	-0.011 (0.009)	22%	38%
Effects on Log Hours:				
Gender Gap	-0.327 (0.013)	-0.014 (0.012)	100%	100%
Mothers	0.252 (0.011)	0.006 (0.010)	77%	42%
Fathers	-0.075 (0.006)	-0.008 (0.007)	23%	58%

Notes: This table decomposes the effects on post-birth gender gaps (from Figure 14) into changes in male and female outcomes. The decomposition is presented for earnings (top panel) and hours worked (bottom panel) in the first and second years after childbirth, respectively. The left panels report effects in log points, and the right panels report the implied shares of the gender gap effects explained by mothers and fathers. The reform-induced reductions in gender gaps are driven by both higher female outcomes and lower male outcomes. For earnings, 78% of the first-year effect and 62% of the second-year effect are driven by mothers, with the remainder driven by fathers. For hours worked, 77% of the first-year effect and 42% of the second-year effect are driven by mothers.

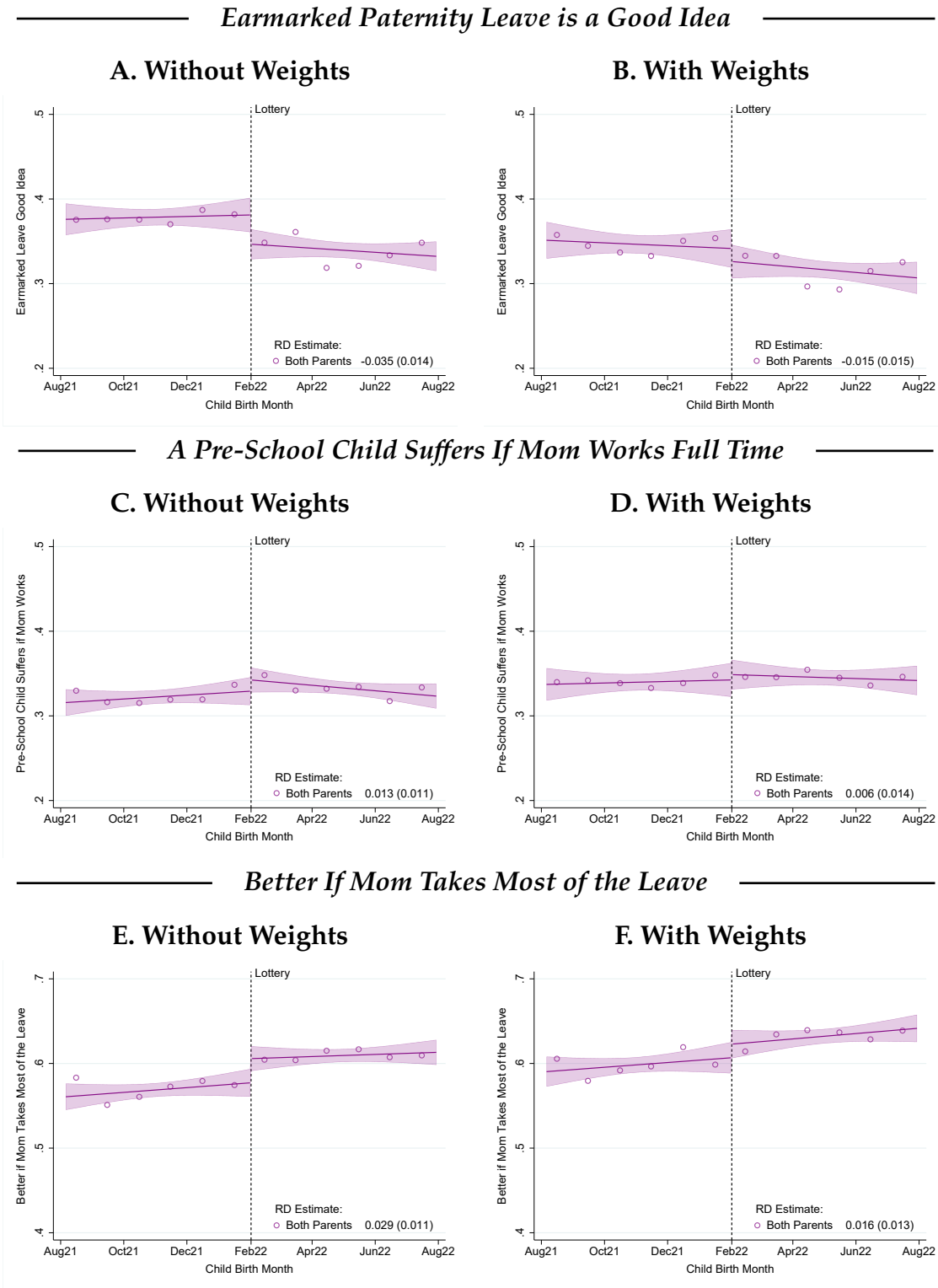
FIGURE A.1: RESPONSE RATES BY GENDER AND TIME



Notes: This figure shows cumulative response rates by time since invitation for the first survey wave (Panel A) and the second survey wave (Panel B), separately for mothers (red) and fathers (blue). Reminders—whose approximate timing is indicated by vertical lines—increased response rates in both waves. A lottery incentive was introduced during the first survey wave for parents with children born after February 2022. As shown in Panel A, this further increased response rates. The lottery incentive was maintained for all parents in the second survey wave. Across the two waves, response rates were approximately 30% for mothers and 15-20% for fathers, corresponding to nearly 40,000 parents per wave.

FIGURE A.2: SELECTION ON UNOBSERVABLES?

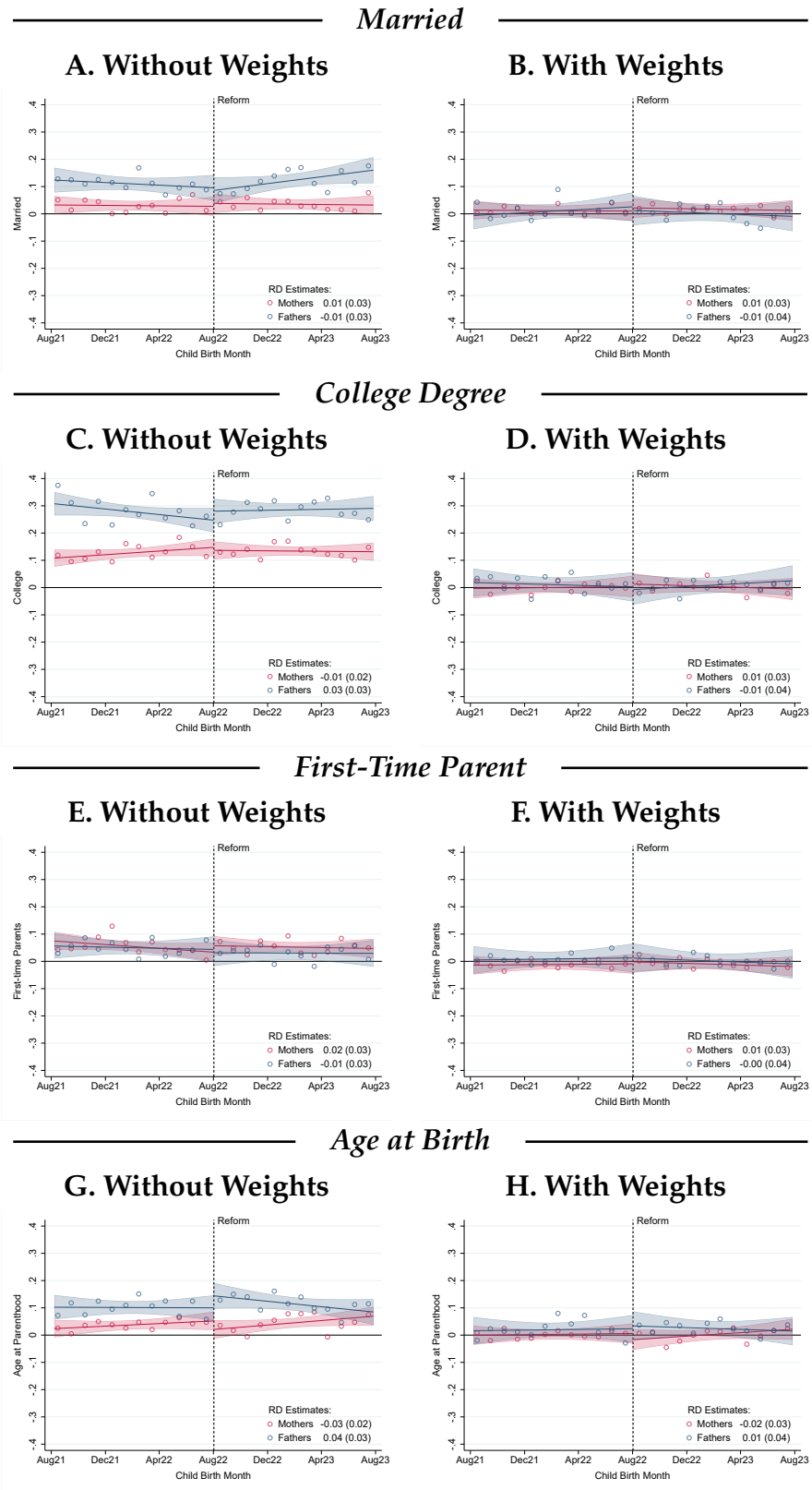
NO DISCONTINUITIES AT THE LOTTERY CUTOFF AFTER REWEIGHTING



Notes: This figure examines selection into the survey by exploiting the lottery incentive introduced during the first survey wave. As shown above, the lottery increased response rates by about 5pp. Using the lottery cutoff, the figure provides RD evidence on whether the lottery-incentivized sample differs from the non-incentivized sample across three belief statements from the first wave. Each panel plots the share of parents agreeing with a given statement. The left panels show unweighted outcomes, while the right panels show reweighted outcomes using the procedure described in Section 3.3. The unweighted specification indicates selection—the lottery induced participation by parents with more gender-conservative views—but the weighted specification resolves this issue. The weighted RD estimates are small and statistically insignificant. Standard errors are clustered at the child level.

FIGURE A.3: IDENTIFICATION CHECK

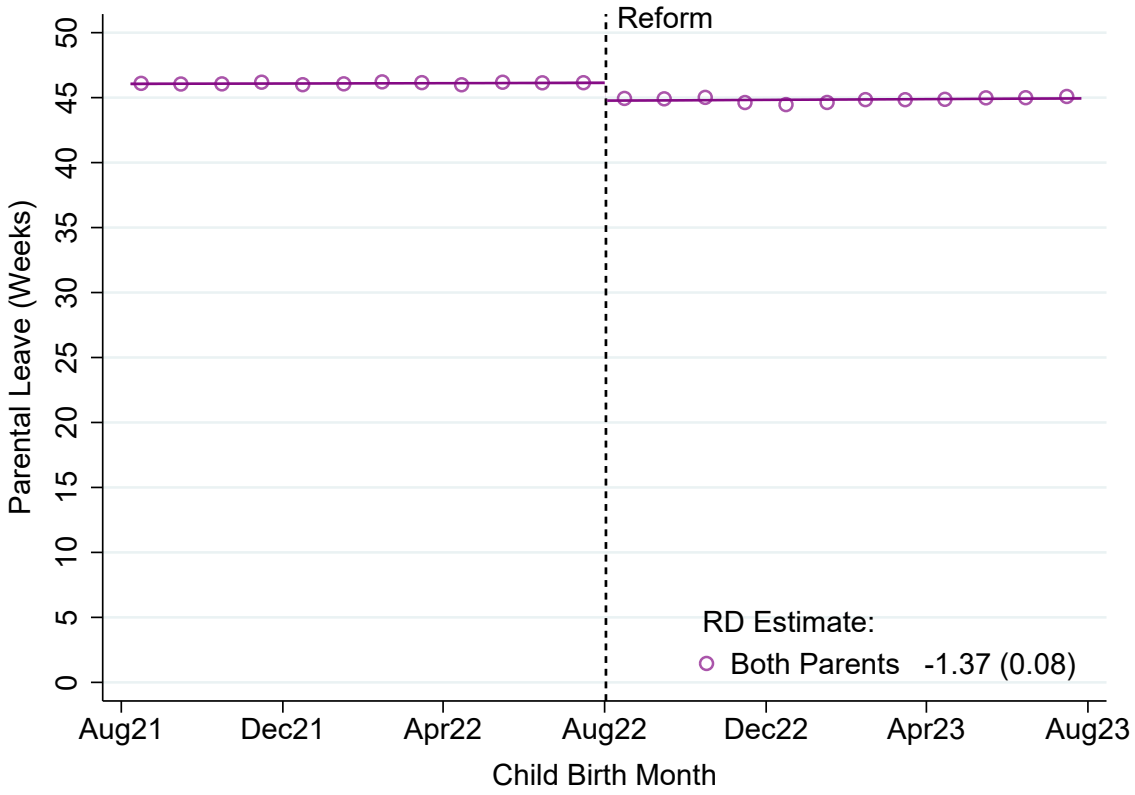
NO EFFECTS ON DEMOGRAPHIC OUTCOMES



Notes: This figure presents RD evidence on the effect of the reform on background characteristics in the raw and reweighted samples. Each outcome is standardized to have mean zero and standard deviation one in the full population of parents (by gender and birth month), so that positive values indicate that a given outcome is overrepresented among survey respondents. The outcomes are generally above zero in the unweighted sample but close to zero once reweighted, confirming that the estimation sample is representative on observables. Moreover, none of the outcomes display discontinuities at the cutoff, consistent with no manipulation of birth timing. Standard errors are clustered at the child level.

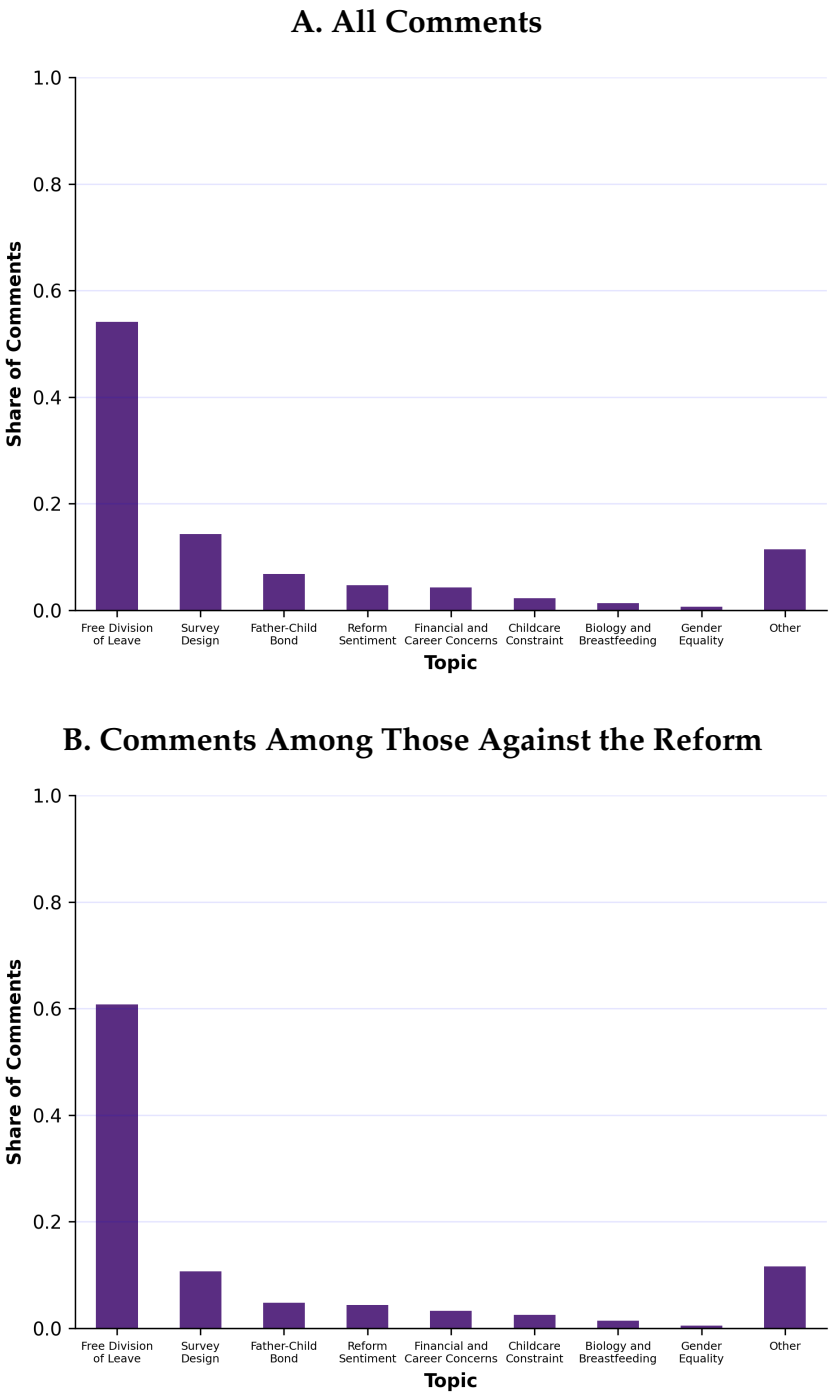
FIGURE A.4: FIRST STAGE

IMPACT ON TOTAL LEAVE DURATION FOR FAMILY



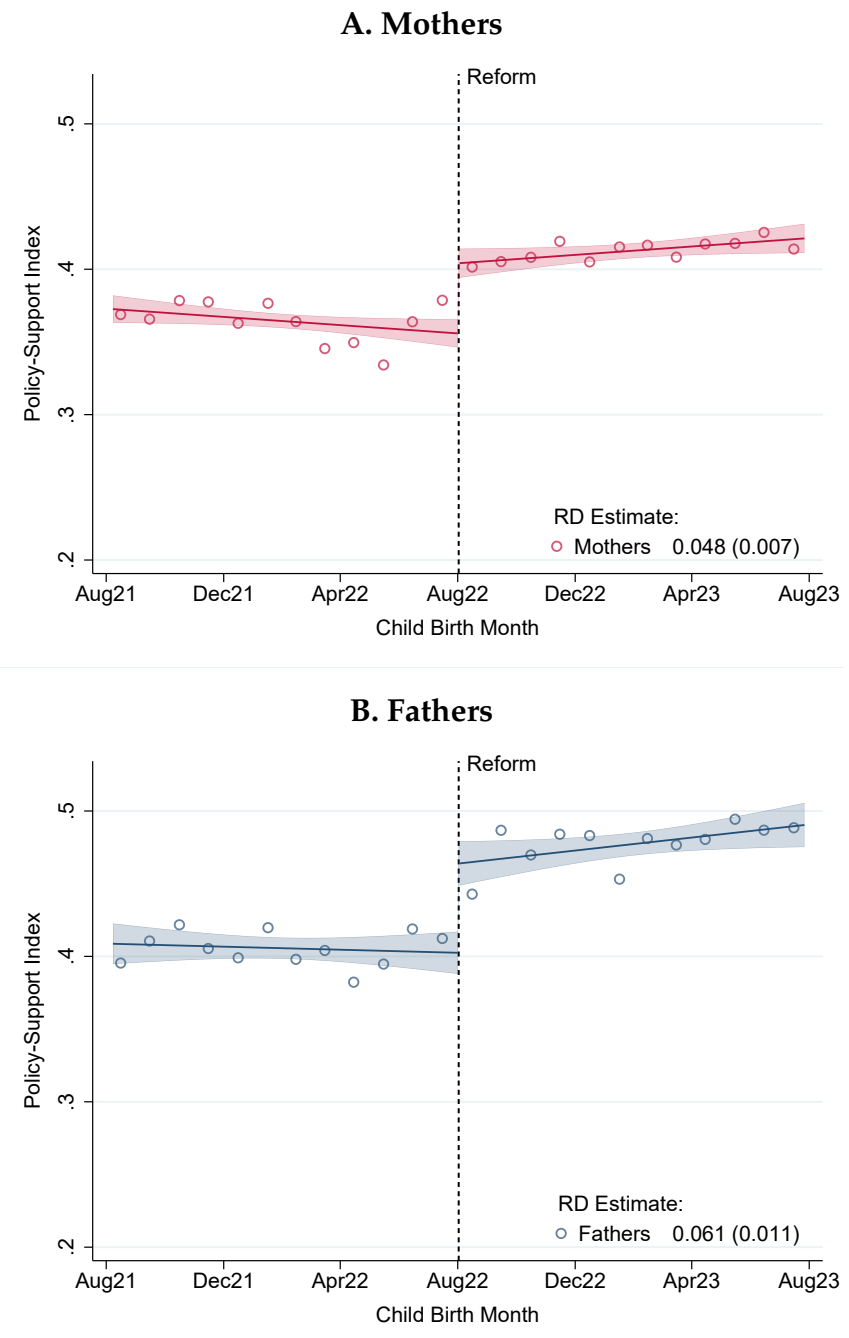
Notes: This figure presents RD evidence on the first-stage effect of the reform on total household leave, constructed in the same way as Figure 2. Because fathers increase leave by less than mothers reduce it, total leave falls by 1.4 weeks.

FIGURE A.5: OPEN-TEXT COMMENTS BY TOPIC



Notes: This figure provides a classification of topics discussed in survey participants' open-text responses to the following prompt: "If you have any reflections that are particularly important to you in relation to parenthood and maternity/parental leave, please write them here. Feel free to include both topics that have been covered earlier in the questionnaire and other relevant topics." Panel A considers all responses received (8,612 responses), while Panel B considers responses among those against the reform (5,036 responses)—i.e., among those disagreeing with the statement that "earmarked paternity leave is a good idea." Topics are identified using zero-shot text classification implemented with ScandiNLI-Large—a Scandinavian-language DeBERTa-v3 transformer model fine-tuned for natural language inference. Each comment is scored against a predefined set of topic labels. The classifier assigns a relevance score to each topic, which may be interpreted as the probability that the comment is about that topic among the set of candidate topics. Each comment is classified by the topic that receives the highest score.

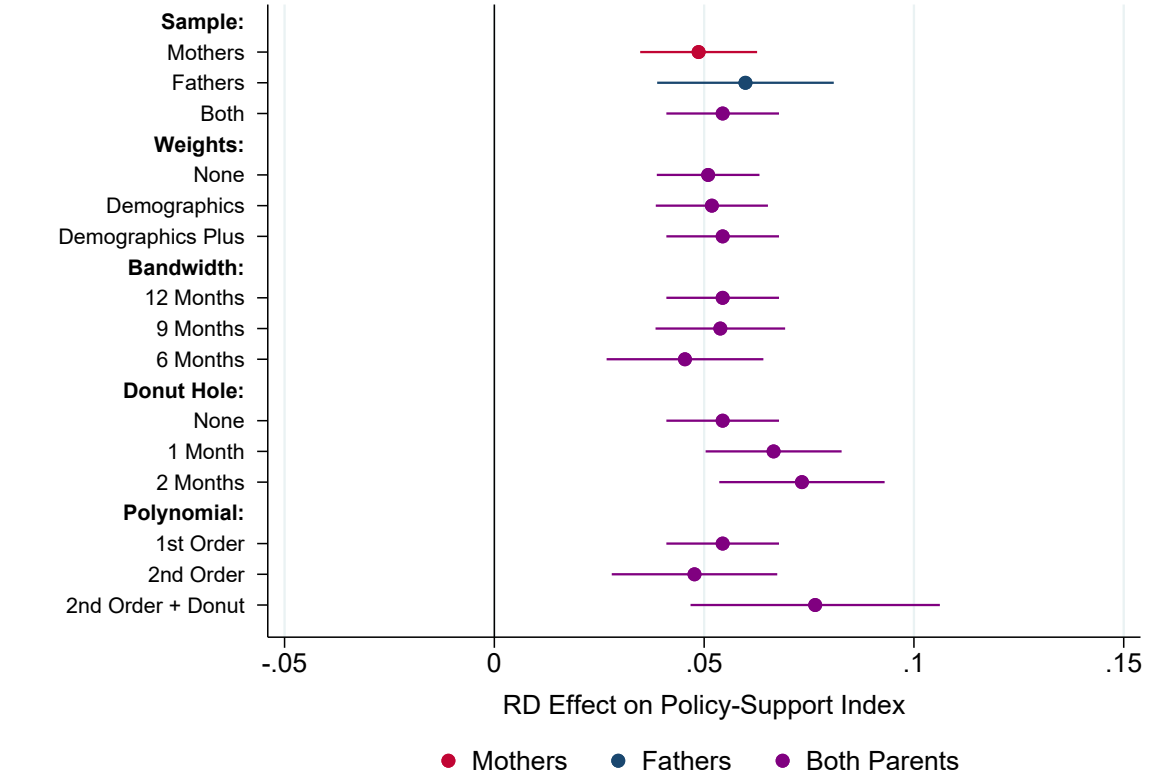
FIGURE A.6: POLICY-SUPPORT INDEX



Notes: This figure presents RD evidence on the effects of the reform on our policy-support index for mothers (Panel A) and fathers (Panel B). This index is constructed as an average of the individual outcomes shown in Figure 6, reversing the coding of perceived costs to ensure comparability with perceived benefits. The reform significantly increases support for earmarked paternity leave, by about 0.05 for mothers and about 0.06 for fathers. Standard errors are clustered at the child level.

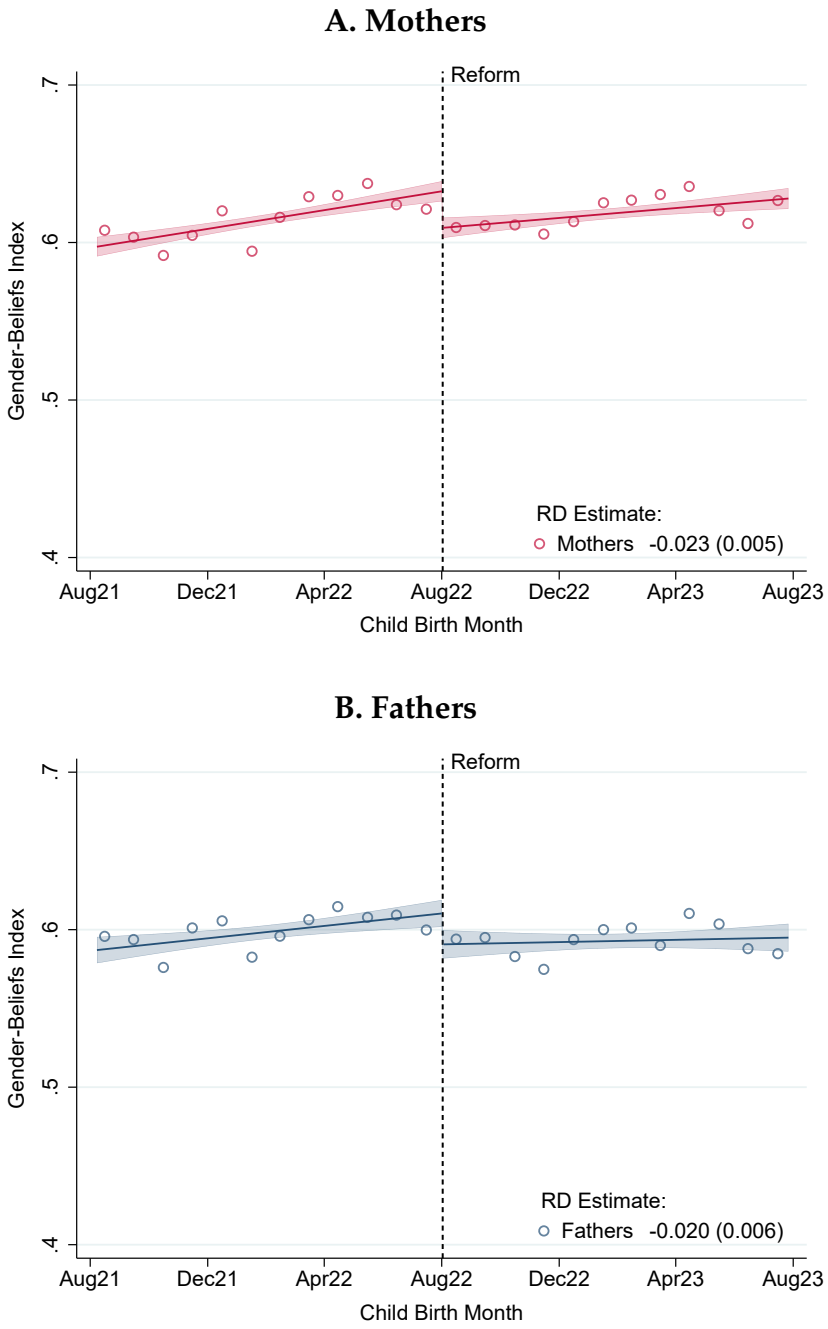
FIGURE A.7: ROBUSTNESS OF ESTIMATES

POLICY-SUPPORT INDEX



Notes: This figure examines the robustness of our RD estimates of the effects on the policy-support index. Starting with the baseline estimates for mothers (red), fathers (blue), and both parents pooled (purple), the figure reports pooled estimates under alternative specifications of sample weights, bandwidths, donut holes, and polynomial orders of the trend functions. The results are highly robust: the estimates are consistently positive and statistically significant, and their magnitudes remain stable across specifications. Standard errors are clustered at the child level.

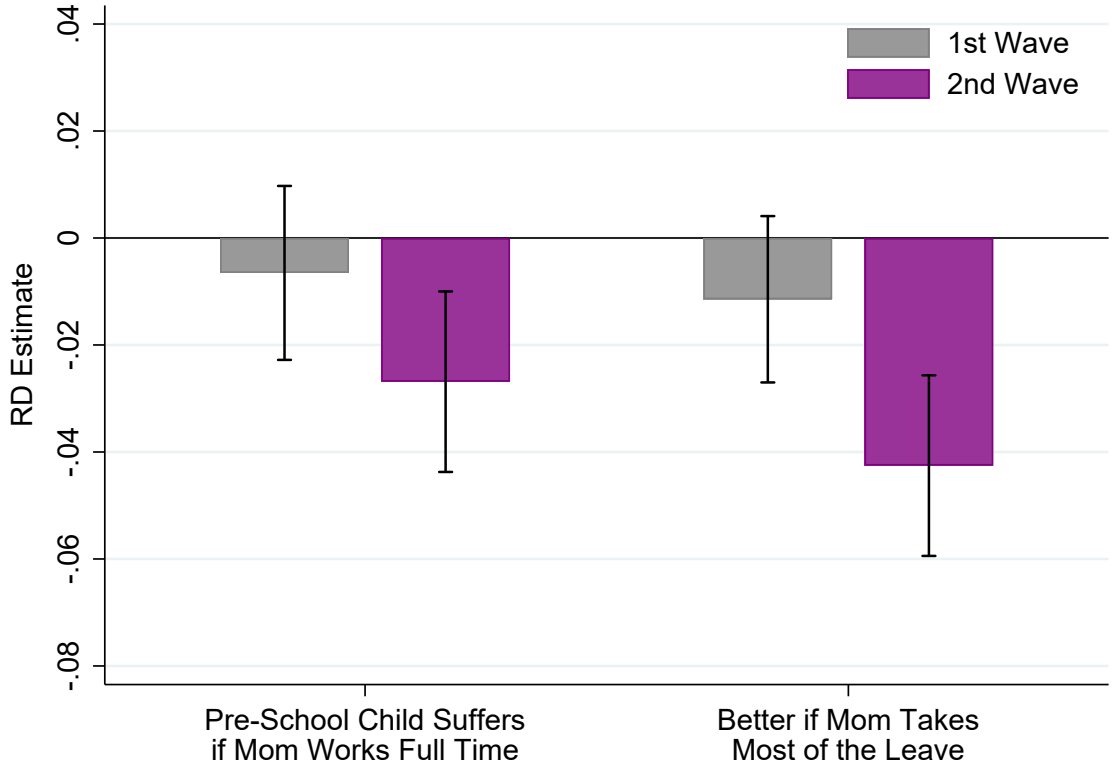
FIGURE A.8: GENDER-BELIEFS INDEX



Notes: This figure presents RD evidence on the effects of the reform on our gender-beliefs index for mothers (Panel A) and fathers (Panel B). This index is constructed as an average of the individual outcomes shown in Figure 10, capturing a range of beliefs about gender roles and childcare. beliefs. A negative effect on this index indicates a shift toward more gender-progressive norms. We find similar effects for mothers and fathers: -0.023 (0.005) for mothers and -0.020 (0.006) for fathers. Standard errors are clustered at the child level.

FIGURE A.9: FIRST-WAVE VS SECOND-WAVE EFFECTS

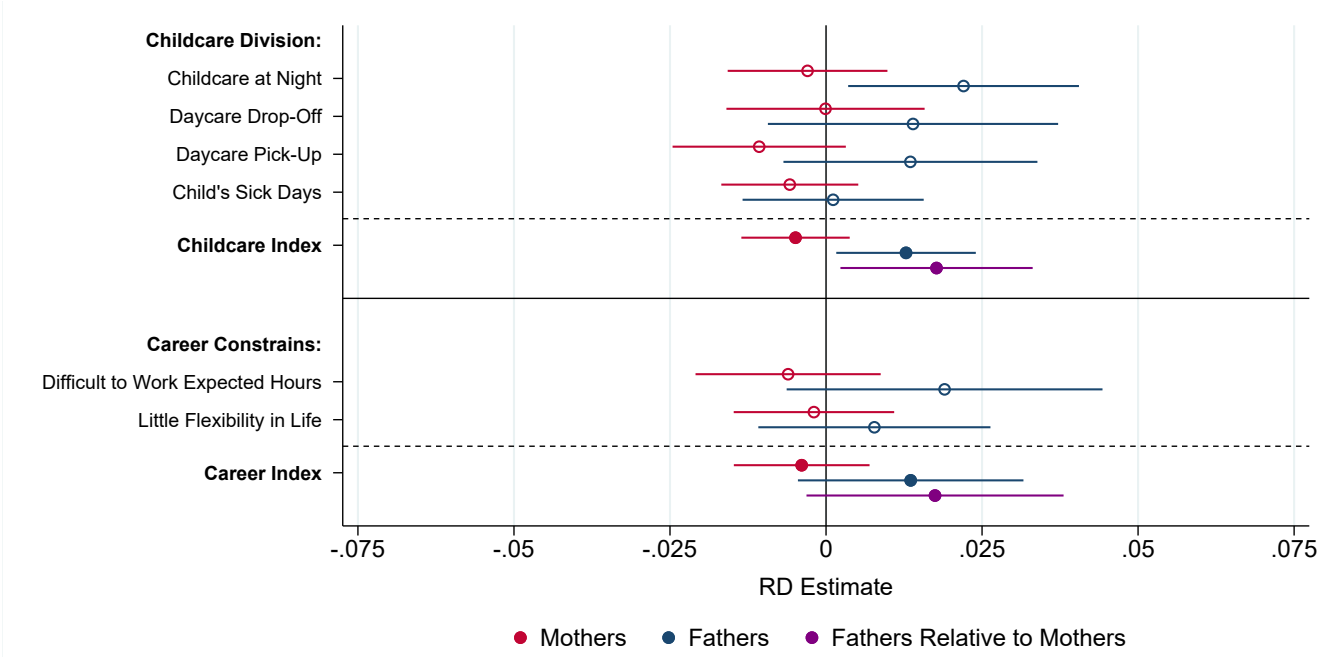
GENDER-ROLE BELIEFS



Notes: This figure compares RD estimates of the effects of the reform on gender-role beliefs elicited in the first wave (gray bars) and second wave (purple bars) of our survey. First-wave interviews were conducted when children were 4 months old (*before* fathers took the expanded paternity leave), while second-wave interviews were conducted when children were 18 months old (*after* fathers took the expanded paternity leave). The sample includes both mothers and fathers. First-wave effects are small and statistically insignificant. Second-wave effects are 3-4 times larger and strongly significant. Standard errors are clustered at the child level. The results in the figure speak against anticipatory effects of longer paternity leave and, more broadly, any effects driven by pure knowledge (without experience) of the new leave regime.

FIGURE A.10: ACTUAL CHILDCARE DIVISION AND CAREER CONSTRAINTS

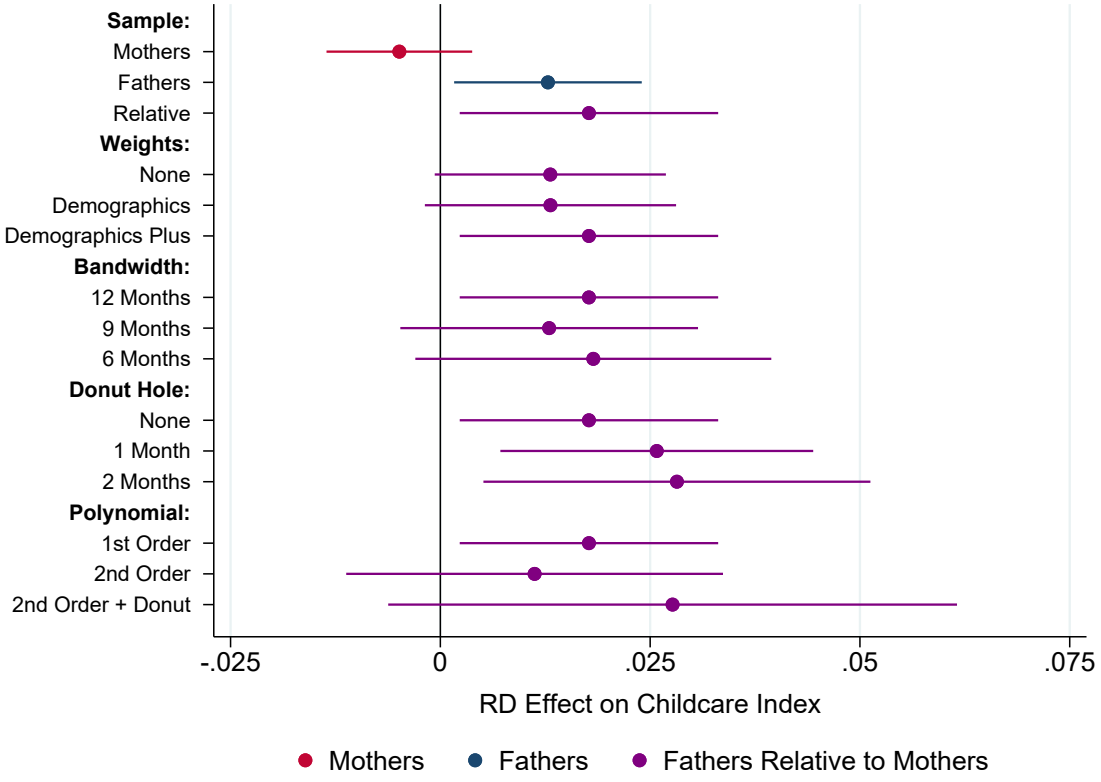
SUMMARY OF ESTIMATES



Notes: This figure presents RD estimates of the effects of the reform on childcare division and perceived career constraints, showing mothers in red, fathers in blue, and fathers relative to mothers in purple. For childcare, parents report whether a specific task falls mostly on them or their partner, using five categories coded from 0 ("My partner does almost everything") to 1 ("I do almost everything"), with 0.5 indicating equal division. For career constraints, parents report their agreement with the statements "Having children makes it difficult to work the expected hours" and "Having children leaves little flexibility in life," using five categories coded from 0 (strongly disagree) to 1 (strongly agree). Childcare and career-constraint indices are constructed as averages of the individual outcomes within each category. RD coefficients are estimated using equation (1), with standard errors clustered at the child level. The estimated effects on childcare division imply that fathers do more and mothers do less. The effects on the childcare index are statistically significant for fathers as well as for fathers relative to mothers. The estimated effects on career constraints suggest that the reform shifts those constraints from mothers to fathers, although the estimates are not quite significant at the 5% level.

FIGURE A.11: ROBUSTNESS OF ESTIMATES

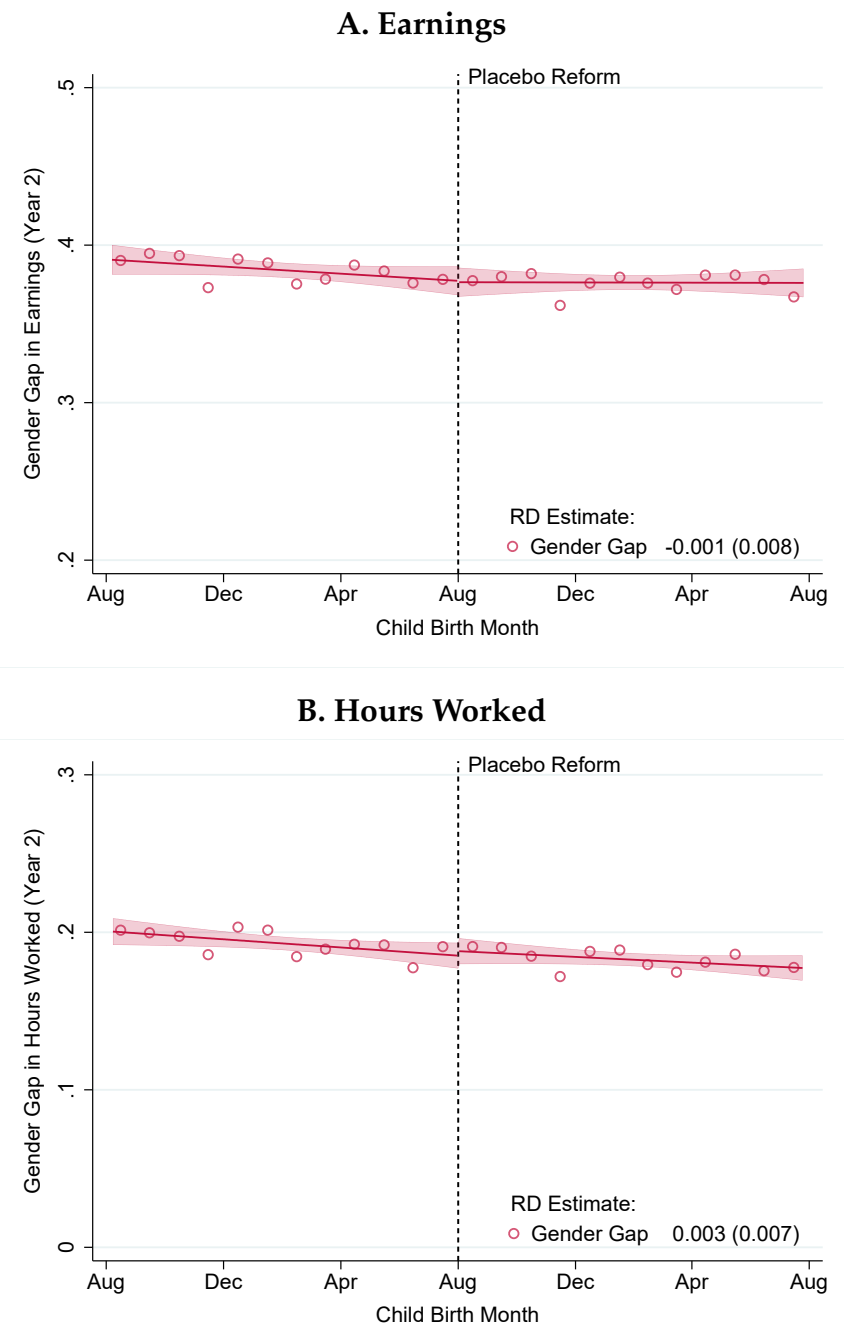
CHILDCARE INDEX



Notes: This figure examines the robustness of our RD estimates of the effects on the childcare index. Starting with the baseline estimates for mothers (red), fathers (blue), and fathers relative to mothers (purple), the figure reports estimates for fathers relative to mothers under alternative specifications of sample weights, bandwidths, donut holes, and polynomial orders of the trend functions. The point estimates are consistently positive—implying that the reform shifts childcare from mothers to fathers—and similar in magnitude across specifications. About half of the estimates are statistically significant at the 5% level, while the rest are marginally insignificant. Standard errors are clustered at the child level.

FIGURE A.12: EFFECTS ON POST-BIRTH GENDER GAPS

PRE-REFORM RDS 2018-2021 (PLACEBOS)



Notes: This figure provides placebo estimates of the effects of the reform on post-birth gender gaps, measured over the second year after childbirth. Specifically, we conduct RD analyses around the August 1st birth-date cutoff in pre-reform years (2018-2021). For each pre-reform year, we select parents of children born within one year of the cutoff and estimate RD coefficients on the stacked sample, using equation (1) and clustering standard errors at the child level. The outcome is defined as the log difference between male and female outcomes—earnings or hours worked—within the household. The placebo effects on both earnings and hours are precisely estimated zeros.