(NOT) THINKING ABOUT THE FUTURE: FINANCIAL INFORMATION AND MATERNAL LABOR SUPPLY*

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Does information about the long-run financial costs of reduced labor supply increase mothers' working hours? We document descriptively that long-term financial factors are not top of mind when mothers decide on their employment level. Moreover, a substantial share of women holds overly optimistic expectations about pension receipt and wage growth under part-time work. In a large-scale field experiment in Switzerland, we randomly assign mothers working part-time as teachers to receive objective information about the long-run costs of reduced labor supply. The treatment increases both demand for financial information and future labor supply plans, in particular among women who underestimate the costs of part-time work. Leveraging linked employer administrative data one year post-intervention, we find that this group of mothers increases working hours by 7%. These findings underscore that policies reducing information frictions in labor supply decisions may help address remaining gender gaps in the labor market.
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I. Introduction

Mothers' reduction in labor force participation and income after the birth of a first child, the so-called child penalty, is large and persistent across countries (Lundborg, Plug, and Rasmussen 2017; Cortés and Pan 2023; Kleven, Landais, and Leite-Mariante 2025). It drives remaining gender inequality in the labor market in industrialized countries (Bertrand, Goldin, and Katz 2010; Kleven et al. 2019; Kleven, Landais, and Leite-Mariante 2025), and implies profound financial consequences throughout the life cycle: women miss out on a significant portion of their potential lifetime earnings and save less for retirement, making them financially more vulnerable and dependent. However, research on exactly how mothers make these pivotal labor supply decisions has been scarce. Are mothers consciously accounting for the full long-term financial implications when deciding how much to work as a parent?

We open this black box and shed light on mothers' decision-making processes around their labor supply, employing two complementary approaches: a descriptive survey among a representative sample of Swiss mothers, and a large-scale randomized control trial (RCT) among mothers working part-time as public school teachers. Our descriptive survey documents that long-term financial consequences are not top of mind when mothers decide on their labor supply. Although the overwhelming majority of women do not explicitly consider this dimension, mothers vary in the extent to which they perceive reduced hours to carry long-term financial costs. Building on these patterns, our field experiment shows that mothers adjust their financial planning and employment plans when they receive objective, individualized information about these costs. Using administrative employment data, we find that women who were initially overly optimistic about the financial implications of part-time work increase their work hours one year after the intervention.

These findings suggest that information constraints may play a role in explaining why mothers' labor supply responses to policies remain muted in settings where the societal default is low participation (Kleven et al. 2024): government interventions, such as parental-leave reforms or child care expansions, may not deliver desired results precisely because mothers do not fully in-

ternalize the potential benefits of these policies (Mullainathan, Schwartzstein, and Congdon 2012; Chetty 2015).[1]

Our field experiment studies mothers who work as public school teachers in Switzerland, a country with one of the largest child penalties in earnings and relatively conservative gender norms (Kleven et al. 2019). After childbirth, the vast majority of mothers in Switzerland return to the labor market working part-time, defined as working less than 90% of the 42 hours that correspond to a full-time equivalent (FTE) (BFS 2022b). The average employment level among female teachers in our RCT is 54% of an FTE, comparable to part-time working mothers across Switzerland (50% of an FTE). Female teachers similarly reduce their working hours around parenthood and refrain from substantially increasing their employment level later in their career. Consequently, the average female teacher earns about 20% less over her lifetime and receives 25% less in occupational pension benefits compared with the average male teacher. Remarkably, these disparities exist even though the teaching career features many of the key ingredients highlighted as conducive for gender equality (Goldin 2014; Goldin and Katz 2016): linear returns to hours in terms of salary, negligible effects of hours on promotion, and prevalent part-time work.

The teaching occupation exhibits several appealing features for our study design. First, teachers are paid according to a deterministic salary and promotion scale, which enables us to produce accurate, individualized projections of the impact of reduced hours on long-term financial well-being. Second, the Department of Education (DoE) in our study region is a major employer, thus ensuring a sufficiently large subject pool that we can link to administrative data. Third, teachers decide on their working hours on a yearly basis, which allows us to study whether mothers react to the information provision by adjusting their labor supply. We conduct the intervention at the start of the yearly employment planning period amid teacher shortages. Because our study population faces relatively low adjustment barriers, this context serves as a proof of concept for establishing the role of information constraints in maternal labor supply.

1. In particular, strong conservative gender norms may be one reason why women fail to consider the financial implications of a reduced labor supply in the first place, with the lack of consideration further reinforcing the societal default (see Akerlof and Kranton 2000; Schwartzstein 2014; Epley and Gilovich 2016).

To motivate our field experiment, we first show that mothers do not have long-term financial consequences top of mind when deciding on their labor supply after childbirth. Our descriptive survey targets a representative sample of Swiss mothers and uses open-ended questions and a vignette featuring a part-time mother to elicit concrete short- and long-term financial estimates. We adjust the numerical context of the vignette based on respondents' education level, and make long-term financial projections with the Future Calculator, an online tool that we developed in cooperation with a Swiss bank for the purposes of this study.

We highlight two main patterns: First, for most mothers (89%), long-term financial factors such as pension implications or professional considerations are not top of mind in their labor supply decisions.[2] Second, using financial guesses based on the vignette, we document heterogeneity with respect to how women assess the effect of low part-time hours on pension receipt and long-term wage growth. Women with overly optimistic expectations, a group we refer to as "cost underestimators," lack a more general understanding of the financial implications of part-time work: They are more likely to believe that increasing work hours in the vignette is not financially worthwhile and report having learned something new after receiving financial projections.

Building on these insights, we conduct a large-scale field experiment. We test whether learning about the long-term financial consequences of part-time work affects women's financial planning and labor supply—two key levers to closing gender gaps in lifetime earnings and pensions. We randomly expose about 2,400 mothers who work part-time as public school teachers to either an informational video discussing the long-term financial consequences of reduced working hours or a placebo video. The treatment video follows a female teacher with children who is considering an employment-level increase. It discusses the effects of part-time employment on lifetime earnings, monthly pension receipt, and financial well-being after potential adverse events and sets these magnitudes in perspective to child care costs. The treat-

2. We document a similar pattern among a representative sample of fathers. Not having long-term financial factors top of mind is thus a more general phenomenon. However, our main focus here is on mothers, as mothers (but rarely fathers) drastically reduce hours after childbirth and thus suffer the financial consequences of not attending to this dimension.

ment group also receives access to the Future Calculator for personalized financial projections based on work history and plans.

Because social learning may be present in our context, we use a two-stage randomization design to address potential spillovers among colleagues that could otherwise attenuate our treatment effects. One-third of schools and their teachers are assigned to a "pure control" group, while teachers in the remaining two-thirds of schools are randomized into treatment or control at the individual level. We estimate treatment effects relative to teachers in pure control schools both for directly treated teachers, as well as the spillover group (control teachers in treatment schools).

In the RCT, we first document that the main descriptive patterns replicate in the teacher population and show that teachers understand the treatment information. The treatment group is 31.26 percentage points (58% over the pure control mean) more likely to correctly rank the relative magnitude of long- and short-term financial factors. The treatment increases demand for financial planning, with treated mothers being more likely to sign up for additional financial information and planning tools. Estimating nonparametric treatment effects based on respondents' part-time pension estimate at baseline, we show that demand for financial tools is driven by mothers who have overly optimistic expectations about pension receipt (cost underestimators). On average, this group of mothers increases their demand by one-third of a standard deviation. Regarding future labor supply plans in the full sample, treated teachers report a 3.13 percentage points higher planned employment level in 10 years, and a 1.69 percentage point increase in employment levels for the next academic year. Cost underestimators, however, plan meaningful labor supply adjustments also in the short run: They report a 4.95 percentage points (9% over the pure control mean) increase in employment level for the next academic year and plan to sustain this increase for up to 10 years. Two months after the intervention, treatment effects persist both with respect to retaining the treatment information overall, and the planned increase of employment levels among women who underestimate the cost of part-time work.

Using linked administrative data from the DoE, we assess teachers' realized labor supply choices one year after the intervention. Consistent with cost underestimators demanding more financial information, we find that this group also increases their

actual employment level by 3.87 percentage points (or 7% over the pure control mean). This effect is of a similar magnitude as indicated in plans elicited immediately after the treatment, and it is meaningful: for the average cost underestimator, this shift—if maintained—reduces the gender gap in total lifetime income and pension among teachers by almost a fifth (around 18%).

We consistently observe an asymmetry in mothers' reaction to the treatment information based on initial priors: women who are overly pessimistic about part-time pension receipt do not reduce their labor supply upon learning they are better off than expected. As these mothers already acknowledge the financial consequences of part-time work (even if they lack precise estimates), the treatment information may not be sufficiently novel to meaningfully challenge prior choices, consistent with agents responding less to gains than losses (Kahneman and Tversky 1979).

Our study design enables us to shed light on potential social interaction effects. Indeed, treated teachers are 11.59 percentage points more likely to discuss their video with their colleagues. Two months after the intervention, we observe some learning among cost underestimators in the spillover group, albeit imprecisely estimated: control teachers in treated schools who underestimate the cost of part-time work absorb the general message of the treatment information and adjust short-term labor supply plans upward. Consistently, we observe a noisy increase in actual employment levels among this group that amounts to one-third of the effect size of their treated counterparts.

We explore the channels through which the treatment intervention changes mothers' plans and behavior. We document that the treatment initially causes a negative emotional reaction, suggesting that this information constitutes a somewhat inconvenient truth—particularly so for cost underestimators, who report significantly more negative emotions. Two months after the intervention, this reverses with the treatment group on average reporting that they feel more in charge of their lives and cost underestimators returning to a neutral emotional state. Treated women also engage more with the study material by discussing it with their partners and in their social circles. We find no effects on partners' employment or fertility plans. We further shed light on why some women may underestimate the costs of part-time work: this group tends to hold more traditional gender attitudes and shows less interest in financial topics, providing a possible explanation for why these mothers may remain uninformed in the

absence of an intervention. These patterns further hint at lack of information, rather than salience, as the behavioral mechanism behind observed adjustments in the RCT. Exposing more mothers to similar content by default via their employer or pension fund may be a promising policy intervention. Last, we explore the generalizability of our results. We document that incremental labor supply adjustments are feasible for broader segments of the population, and we show that our short-term RCT findings replicate among a small sample of pregnant women in Switzerland.

We contribute to three main strands of literature. First, this article isolates the role of information constraints in mothers' labor supply decisions, thus highlighting their relevance for tackling gender inequality in the labor market: ensuring that families solve the correct optimization problem could help fully unlock the potential of policies designed to encourage women's labor supply. Prior literature on the drivers of maternal labor supply has primarily focused on institutional factors (see Olivetti and Petrongolo 2017 for an overview), such as parental-leave reforms (Lalive and Zweimüller 2009; Lalive et al. 2014; Schönberg and Ludsteck 2014; Dahl et al. 2016), child care availability (Blau and Currie 2006; Havnes and Mogstad 2011; Hermes et al. 2024; Humphries et al. 2024; Kleven et al. 2024), work arrangements (Goldin 2014; Goldin and Katz 2016; Bütikofer, Jensen, and Salvanes 2018; Ciasullo and Uccioli 2024), and cultural norms (Fernández, Fogli, and Olivetti 2004; Bursztyn, González, and Yanagizawa-Drott 2020; Kleven 2026; Boelmann, Raute, and Schönberg 2025). Work that attempts to understand the role of mothers' beliefs around their labor supply is scarce. Notable exceptions are Kuziemko et al. (2018), who document changes in gender attitudes around childbirth, and Boneva et al. (forthcoming), who collect a comprehensive array of beliefs around maternal labor supply and show that perceptions of how mothers' employment affects child development are malleable to information.

Second, the combination of rich survey and administrative data allows us to trace how shifts in self-reported intentions translate into behavioral changes. We contribute to studies that highlight the role of information in helping agents more fully account for returns on investment regarding their (financial) future, but typically rely on just one type of data. Literature on retirement planning shows that information can increase enrollment and savings (Duflo and Saez 2003; Goda, Manchester, and

Sojourner 2014; Dolls et al. 2018; Angelici et al. 2022), and self-reported employment in old age (Liebman and Luttmer 2015). Shifting students' perceptions on the average return to education can increase demand for schooling (Jensen 2010; Wiswall and Zafar 2015; Bleemer and Zafar 2018), but Deshpande and Dizon-Ross (2023) show that lowering expectations about future government transfers does not discourage parental investments in children's human capital. Several recent papers have explored workers' biased perceptions of their outside options and the consequences thereof for labor market inefficiencies (Cullen and Perez-Truglia 2022; Jäger et al. 2024).

We further relate to a rich body of work that examines how people respond to emphasizing factors in decision-making that may not be immediately top of mind (Gennaioli and Shleifer 2010; Kahneman 2011; Hanna, Mullainathan, and Schwartzstein 2014; Schwartzstein 2014; Enke 2020; Graeber 2023; Andre, Schirmer, and Wohlfart 2023) or about which they may have incomplete information (Haaland, Roth, and Wohlfart 2023).

This article is structured as follows. The next section details the study context. Section III provides descriptive evidence on how mothers perceive and calculate the long-term consequences of reduced employment levels. Section IV describes our experimental design, and Section V presents the results of the RCT. Section VI discusses mechanisms, followed by robustness checks in Section VII. The final section concludes.

II. Study Context

II.A. Maternal Labor Supply in Switzerland

Compared with other Organisation for Economic Co-operation and Development (OECD) countries, the labor force participation rate of mothers in Switzerland is relatively high. However, most mothers work low part-time hours: 76% of mothers with a child below the age of 14 in Switzerland are employed (OECD average: 71%, United States: 67%), but almost 80% of those employed work part-time, defined as working less than 90% of an FTE (OECD 2024a). With mothers' earnings dropping by 68% relative to fathers' 10 years after the birth of the first child, Switzerland has one of the largest long-term child penalties (Kleven et al. 2019; Krapf, Roth, and Slotwinski 2020). The share of mothers working part-time decreases slightly as children age,

but most mothers never return to full-time employment: 78% of working mothers with children below the age of 4 work part-time, compared with 65% of mothers with children aged 18–24 (BFS 2024a, 2024b). External child care costs in Switzerland for children under the age of four are comparatively high, and families rarely use external care full-time (BFS 2020; OECD 2024b). After the age of four, public kindergarten and school are free of charge but typically do not cover the full day. In our study region, child care is widely available and flexibly accommodates part-time employment.

II.B. Part-Time Work and the Swiss Pension System

Apart from potentially slower career progression and the implied decrease in wage growth, part-time employment in Switzerland also entails considerable reductions in future pension receipt. The Swiss pension system is composed of three pillars. The first pillar (OASI) ensures basic needs only, and part-time penalties are small. The second pillar, the occupational pension scheme (PP), serves to maintain the standard of living in old age. Employed people are affiliated with a second-pillar pension fund if they exceed a minimum yearly earnings threshold. The second pillar fund invests the federally mandated employer-employee contributions and converts it into a pension on retirement. Due to the minimum yearly earnings threshold and contributions being directly proportional to earnings above the threshold, part-time work has a heavy effect on pension receipt from this pillar, resulting in an average gender pension gap of 47.5% (BFS 2022a). The third pillar consists of voluntary private pension savings that offer some tax benefits. The pension system in Switzerland is federally mandated, and any funds accumulated pertain to the individual and are fully transferable between employers. Since reduced working hours primarily affect the occupational pension scheme (second pillar), it is the main focus in our study and we refer to it simply as "pension" throughout the article.

II.C. Female Teachers and the Cost of Reduced Employment

1. *Female Teachers.* Our main study population are female public school teachers with children in a large German-speaking region in Switzerland. As in other professions, female teachers usually reduce their employment level after having a child. Online Appendix Figure A.1.1, Panel A displays the average fe-

male teacher's employment level in our study region by age. Female teachers reduce their employment level to below 60% of an FTE during child-rearing ages, while that of male teachers remains constant. Although women's employment level rises slightly as they age, it never fully recovers to its original level and remains substantially below that of male teachers until retirement. We observe a similar age gradient for employment levels in the general female working population, displayed in Panel B.

Using data from the Swiss Labor Force Survey (SLFS; BFS 2024c), Online Appendix Table A.1.1 presents summary statistics for working mothers aged 25–50 (column (1)), and female teachers with children in the same age range (column (3)). We set these representative samples in context to female teachers in our administrative data more generally (column (4)), as well as mothers in our main RCT sample (column (5)).[3] The characteristics across groups are strikingly similar: on average, working mothers are 40 years old and have 1.7 to 2 children, where the youngest is about six years of age. Working mothers across all occupations have an employment level of 61% of an FTE, whereas female teachers with children work about 55% of an FTE. This is the case despite the fact that the teacher sample skews heavily toward the highest education level (70% versus 41% in the general population of working mothers). In our region of study, around 20% of female teachers work at the kindergarten level, 62% at the primary, and 18% at the secondary school level.

2. *Costs of Reduced Employment Level.* To illustrate the long-term financial costs of part-time work, we compare the long-term financial outcomes for a teacher working at the average female teacher's employment level against a scenario of full-time employment. We assume that teachers stay in their occupation based on high retention rates (see Section IV.C.3). The most significant financial consequence of reduced labor supply is the decrease in earnings. Over her working life, the teacher in the full-time scenario accumulates lifetime earnings of around 5.12 million CHF, while the average female teacher reaches around 3.34 million CHF (Online Appendix Figure A.1.2, Panel A). This represents a reduction in potential gross lifetime earnings of

3. Online Appendix Table B.1.1 provides an overview of the different data sources used in our study.

around 35%. Accounting for taxes and child care costs reduces this loss only marginally.[4] Reduced earnings directly affect future retirement income. Pension payments from the occupational scheme are 43% lower compared with a full-time scenario (Online Appendix Figure A.1.2, Panel B), and similar to the second-pillar gender pension gap in Switzerland (47%; BFS 2022a). Since teachers' salaries adhere to a deterministic pay scale without a part-time penalty, the financial consequences of reduced hours in this setting likely represent a lower bound relative to other professions.

III. Descriptive Evidence: Perceptions of the Long-Term Financial Costs of Reduced Labor Supply

III.A. Survey Design and Sample

We recruit a representative sample of mothers from German-speaking Switzerland, aged 25 to 50, through one of the main local survey companies (Intervista). We invite participants regardless of their labor force status. Our final analysis sample comprises 547 mothers. While not the primary focus of our study, we also recruit fathers in the same age group to assess the main descriptive patterns by gender. Online Appendix B documents sample selection and the coding of open text questions. All survey documentations, including questionnaires and financial projections, are provided via Online Appendix Table C.1.1.

1. *Survey Design.* We ask participants to describe the most important factors behind their labor supply decision after having their first child in an open-ended question (Haaland et al. 2025): "Please think back to the time when you decided whether and how much you would like to work after the end of your maternity leave after the birth of your first child. What factors were most important to you when you were deciding whether and how much to work after the end of your maternity leave? Please write as

4. Calculations use the Future Calculator and are based on gross earnings; documentation is available via Online Appendix Table C.1.1. If we take into account joint taxation and assume the most conservative scenario with her partner having very high earnings (400,000 CHF), the average tax rate would be 24.5% (full-time) versus 23% (part-time) and the total loss in earnings would still amount to 33% of her potential net income.

much as you like—this question is very important for us to better understand parents' decisions regarding their employment level."

We elicit several financial estimates based on a vignette adjusted to respondents' own education level (low: apprenticeship; middle: higher professional education; or high: university degree) as follows: "Sara is 33 years old and lives with her husband and 3-year-old child in a city in Switzerland. Sara is thinking about her future employment level. Sara has [education level] and, since having a child, she has been working 40% (two days a week). She earns CHF [wage] (gross) per month. She is now considering increasing her employment level to 80% (i.e., working four days a week instead of two). While Sara is working, her child is looked after at the local nursery. Her husband works full-time."[5]

Using a vignette allows us to anchor participants' beliefs about current employment level, salary, and child care use (Stantcheva 2023). We choose 40% as a baseline employment level that is representative for part-time working mothers with small children (SLFS), and 80% as an employment level that is commonly perceived as necessary and sufficient to obtain meaningful promotion opportunities in Switzerland (Bonoli et al. 2016; Sander et al. 2024). Even in the short run, increasing work hours results in higher net income for all three education groups.

Based on this vignette, we elicit participants' perception of the general magnitude of different financial factors, starting with whether respondents consider it financially worthwhile for Sara to increase her level of employment. We then ask participants to rank which factor (total child care cost, total future salary, total pension savings, and faster career progress) would have the largest long-term financial impact if Sara increases her hours. Because child care costs rank below total future salary and pension savings across all education groups (see vignette documentation in Online Appendix Table C.1.1), we measure whether participants correctly rank child care after pensions and forgone earnings. Next we ask participants to provide their best numerical guess of four financial figures in an open-text box, displayed in random order:

5. We insert the wage corresponding to each education category based on the median earnings at 40% of an FTE for each group in the SLFS (2018–2022): low: 2,250 CHF/month; middle: 2,700 CHF/month; and high: 3,200 CHF/month.

(1) *Current salary* (80% FTE): The current monthly salary when Sara works 80% of an FTE.
(2) *Pension receipt* (40% FTE): Monthly pension receipt if Sara continues working at 40% of an FTE for the rest of her working life.
(3) *Salary in 10 years*: Monthly salary Sara would earn in 10 years if she works
    (1) 40% of an FTE for the next 10 years.
    (2) 80% of an FTE for the next 10 years.

We aim to capture two main dimensions along which women might make mistakes when assessing the long-term financial implications of part-time work: pension receipt and wage growth. Women may have incorrect priors about the overall level of pension receipt under (low) part-time hours. At the same time, women may also fail to take decreased returns to experience into account when working part-time. To assess respondents' perception of relative wage growth under part-time work, we take the ratio between estimated 10-year salary at the 40% employment level, and the 80% employment level. For example, a respondent who thinks that there are no returns to experience and therefore guesses that earnings at the 40% employment level are half of the earnings at the 80% employment level will have a ratio of 0.5.

We make financial projections for these numbers based on the Future Calculator and incentivize truthful reporting with an additional voucher for the participant whose guesses are closest to our projections. We reveal these projections to participants at the end of the survey.

2. *Sample.* Online Appendix Table A.1.1, column (2) shows summary statistics for the descriptive survey sample, next to the general population of working mothers (column (1)). In terms of demographics, mothers in the descriptive survey are similar to working mothers in the general population. On average, mothers have close to two children, with the youngest child being 7.6 years old. Respondents in the descriptive survey are slightly more educated compared with working mothers in Switzerland generally, and 92% currently hold a job. Among those in employment, almost all work part-time with an average employment level of about 2.5 days per week (53% of an FTE).

Online Appendix Table A.2.1 shows additional demographic characteristics of the descriptive survey sample by education

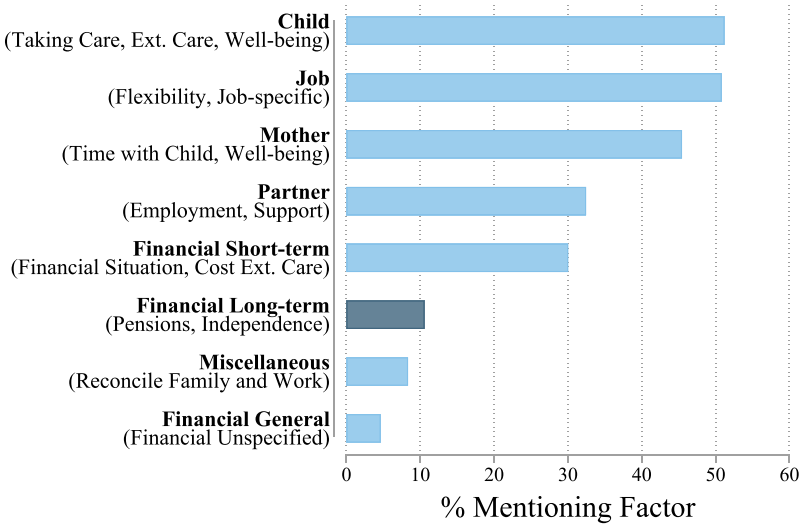


FIGURE I

"Top of Mind": Factors Considered in Labor Supply Decision After Childbirth

This figure shows the percentage of women who mention a given topic when asked which factors they considered for their labor supply decision after the birth of their first child. Coding and validation are documented in Online Appendix B.5. Data: Female sample, descriptive survey. $N = 539$.

group. Mothers in the high education group are somewhat more likely to be working (high: 94%; mid: 92%; low: 88%), and they also work at slightly higher employment levels (high: 60%; mid: 57%; low: 52% of an FTE). The monthly salary displayed in the respective vignettes matches respondents' own salary relatively closely. Both in terms of financial literacy (Panel E) and gender norms (Panel D), we observe an education gradient where mothers with lower educational levels are less financially literate and hold more conservative opinions about mothers' roles.

III.B. Stylized Facts

1. *Long-Term Financial Factors Are Not "Top of Mind".* Which factors do women consider when deciding on their labor supply after the birth of their first child? Figure I shows the percentage of women who mention a given topic when asked to describe the factors they considered. Around half of the sample highlights considerations related to child well-being (care, time spent with the child), the mother's own well-being, and job-related

factors, such as flexibility. A substantial proportion (around 30%) also mentions short-term financial factors, including child care costs and the family's current financial situation. In contrast, only a small fraction of mothers (around 11%) mentions any factor related to long-term financial aspects, such as pensions, financial independence, or long-term career considerations. Online Appendix Table A.2.2, Panel A shows that this share is low across all education categories (low: 6%; mid: 12%; high: 13%).

The fact that for most respondents long-term financial considerations are not top of mind in labor supply decisions is not unique to women. Online Appendix Figure A.2.1 shows that a similarly small share of fathers (10%) mentions this dimension when it comes to their employment-level decisions as a parent. However, mothers are the group that faces the brunt of financial consequences when failing to consider this aspect, as mothers (but rarely fathers) substantially adjust their labor supply when becoming a parent.[6]

2. *How Do Mothers Assess Long-Term Financial Factors?* To better understand how mothers assess these long-term factors when prompted, we present participants with the part-time vignette scenario described in Section III.A. We gauge how mothers assess the relative magnitude of the different financial factors involved in the vignette's employment-level decision in the ranking exercise. About 30% of women incorrectly rank child care costs above either total future salary or pension savings (see Online Appendix Table A.2.2, Panel B). A bit less than a quarter of women incorrectly deem the described employment-level increase not financially worthwhile.

Online Appendix Figure A.2.7 depicts histograms of participants' numerical guesses for the financial dimensions of the part-time vignette, and Online Appendix Table A.2.3 provides a summary. We consider an answer to be "correct" if a participant's guess is within a 10% bandwidth from the projected value we calculated with the Future Calculator. Participants struggle to assess the financial effect of working part-time beyond the direct effect on monthly salary. Almost all women across education

6. The partner who reduces their labor market participation bears the primary financial risk associated with household specialization, since postdivorce income sharing is not required to be equal and assets accumulated after divorce are considered individual property in Switzerland.

levels are able to correctly calculate the monthly salary impact of increased hours (low: 88%; mid: 93%; high: 97%). However, women's priors with respect to pension receipt and wage growth under part-time work are much more dispersed. Fewer than 10% of women provide an estimate within a 10% bandwidth of projected pension receipt. Regarding wage growth expectations, respondents' median guess of the 10-year salary under low part-time work (40% FTE) is too high. However, at the higher employment level (80% FTE), respondents' median guess is remarkably close to the projected value. This suggests that participants may ignore decreased returns to experience under low part-time hours.[7]

A substantial share of women have quite optimistic priors for both pension receipt and expected wage growth under low part-time work (Online Appendix Table A.2.2, Panel B): 62% of women overestimate pension receipt by more than 10%, and a similar share thinks that wage growth is at least as high under a low part-time relative to an 80% employment level. Forty-two percent of the sample is overly optimistic with respect to both concepts. Online Appendix Figure A.2.2 shows that these two measures of misperceiving the long-term financial cost of part-time work are correlated: respondents' deviation from projected pension receipt is positively associated with their deviation from the projected salary ratio.

3. *Correlates of (Mis)Perceiving the Cost of Part-Time Work.* Do overly optimistic pension and wage growth expectations reflect a broader lack of respondents' financial understanding that part-time work is costly? In Online Appendix Table A.2.2, Panel B, we observe an education gradient, with women with higher education levels being relatively less likely to overestimate projected pension receipt by more than 10% (low: 77%; mid: 62%; high: 51%), and less likely to think that there is no wage growth penalty under (low) part-time work (low: 74%; mid: 62%; high: 54%).

Online Appendix Figure A.2.4 examines the relationship between the extent to which respondents are cost underestimators, as measured by pension receipt and wage growth expec-

7. Indeed, across all education groups, the modal salary ratio is 0.5, with slightly more than one-fifth of participants simply dividing the salary at the 80% employment level by two.

tations, and their general financial understanding of part-time work. We plot these correlations based on a continuous measure of underestimating the cost of part-time work that standardizes and then aggregates respondents' deviations from projected pension receipt and part-time wage growth. We interpret respondents with higher versus lower values on this scale as having priors that are more versus less optimistic regarding the costs of part-time work.[8] Underestimating these costs correlates negatively and significantly with measures of being financially savvy: women who underestimate these costs more are less likely to deem the employment-level increase described in the vignette as financially worthwhile. They are also less likely to get the relative ranking of child care costs correct and show less interest in getting access to an online tool to calculate the pension implications of part-time work for themselves.

After we reveal the projected numbers for women's financial guesses, we see that women who underestimate costs more are more likely to indicate that they learned something new. While cost underestimation thus seems to reflect respondents generally being less financially informed, we do not observe a relationship between having overly optimistic priors and "top of mind" patterns: cost underestimation does not meaningfully correlate with women mentioning long-term financial factors in the open-ended question, or with having calculated the financial implications of a reduced employment level for themselves (Online Appendix Figure A.2.5). Taken together, these patterns suggest two distinct notions of limited financial considerations. First, almost all women do not have long-term financial factors top of mind when deciding on their employment level. Second, when we guide participants to think through the magnitude of these factors, we observe substantial heterogeneity in how informed women are of the general notion that part-time work carries costs. As cost-underestimating women exhibit lower levels of financial sophistication and are more likely to indicate that they "learned something new" from the revealed calculations in the survey, this group may lack important pieces of information to make a fully informed labor supply decision.

8. Online Appendix Figure A.2.3 shows the correlation between the index and priors about wage growth and pension receipt separately. Online Appendix Figure A.2.8 shows the distribution of the index.

4. *Why Do Women Not Consider Long-Term Financial Factors?* Finally, we probe why women do not consciously account for long-term financial factors and whether they would be interested in obtaining this information if it were readily available. The vast majority of women (83%) indicate that they did not make concrete calculations about how their own employment-level decrease affects their pension receipt (Online Appendix Table A.2.2, Panel A). When asked why they did not calculate these numbers, most women give reasons that reflect a general lack of attention towards this topic, with 55% saying they were either not aware of it or that it did not seem important at the time they made the decision. About a quarter of respondents, however, also indicate that they did not know how to make these calculations. In contrast, only 13% of women in our survey report that they did not consider this dimension because their employment-level decision was of a temporary nature. Around 95% of respondents indicate interest in obtaining the financial projections for the vignette and think this type of information would be generally useful for women when making labor supply decisions. When we reveal the financial projections for the vignette, about 59% of women are surprised by those numbers, with most mentioning projected pension receipt (64%).

In sum, these patterns suggest scope for information provision to overcome mothers' general lack of consideration toward long-term financial factors when deciding on their labor supply. Especially for cost underestimators, who have not fully internalized the financial costs of part-time work, providing this information may offer novel insights and clash more strongly with initial priors.

IV. Experimental Design

IV.A. Recruitment, Sample, and Timeline

1. *Recruitment.* We collaborate with the DoE in a German-speaking region of Switzerland. The DoE provides us with the contact information of 9,369 female teaching staff with a cantonal employment contract, aged 25–50, in public schools (kindergarten, primary, and secondary) for the 2022/23 school year. We restrict the sample to teachers who work at only one school, resulting in 9,281 invited teachers employed across 495 schools. Our recruitment letter addresses female teachers with children.

We screen out women who do not have children (and are currently not pregnant) and invalid responses, resulting in a total sample of 2,767 participants. As outlined in the pre-analysis plan, we restrict the main analysis sample to mothers who do not work full-time. We further exclude pregnant women, as statutory maternity leave is likely to affect their employment level in the following school year.[9] Our final analysis sample consists of 2,359 women employed across 479 schools.[10]

2. *Timeline.* Online Appendix Figure A.6.1 shows the timeline of our field experiment. We sent invitations to our main survey, in which we also collected wave 1 outcomes, in late November 2022. Our intervention was strategically timed to precede the period when teachers communicate their preferred level of employment for the following school year to school principals. We conducted the follow-up survey in late January 2023 (about two months after the wave 1 survey began).[11]

IV.B. Intervention Material

1. *Treatment Material.* The treatment is designed to provide participants with objective information on the long-term financial costs of a reduced labor supply. These cost projections are calculated with the Future Calculator and tailored to teachers' deterministic salary and pension schedules.

Based on these projections, we design an informational video discussing the main long-term financial consequences of part-time work with the example of a female teacher. We focus on two main dimensions: the impact on lifetime earnings and monthly pension income in old age. We briefly note that financial risks become particularly relevant in the case of adverse life events, such as divorce. Finally, we put these figures into perspective by com-

9. We did not expect the intervention to have an impact on labor supply for women who are already working full-time (8% of mothers who respond to our survey work $\geq$ 90% of an FTE). In Online Appendix Table A.4.3, we show that estimates are qualitatively similar when including pregnant women.

10. Schools in our main sample employ 28 teachers on average. Among treated schools, the average share of treated teachers is 7%, and 2% for treated teachers who are cost underestimators.

11. We also sent all participants a link to the video they watched in wave 1 as a reminder in their decision-making process one week before sending the follow-up survey. However, take-up was low, with only 14% of participants clicking the link to rewatch the video.

paring them to child care costs. The video follows the decision-making process of a representative female teacher with two children who, together with her partner, is considering how much to work. We use several graphics, as well as qualitative descriptors, to ensure that the information is conveyed in an understandable way. In addition, we send participants in the treatment group a personalized log-in for the Future Calculator via email after they finished the wave 1 survey. Online Appendix C contains the script and screenshots from the treatment video, as well as an overview of the Future Calculator.

2. *Control Material.* The control group watches a video of similar length on an unrelated financial topic, each featuring charts with numbers. These videos were produced by the national public television as part of their regular programming (see Online Appendix Table C.1.1). We randomize the control group with equal probability to one of three different videos on the following topics: explained and unexplained variation in the gender pay gap, suggested tax breaks for families, and rent versus buy decisions in the current housing market.

IV.C. Data

1. *Wave 1 Survey.* The wave 1 survey consists of three parts: participants first complete a baseline survey, then watch either the control or treatment video, and proceed with a short end line survey, during which we assess wave 1 outcomes. We group all of these activities into one survey to minimize attrition.

i. Baseline. The first part of the survey gathers baseline data on sociodemographic characteristics, employment situation, family and work constraints, gender norms, decision factors regarding respondents' employment level after becoming a parent, and the perception of pension receipt under part-time work. To assess the extent to which a teacher underestimates the cost of reduced hours, we focus on pension receipt from part-time work, given the absence of a meaningful promotion penalty for teachers. We ask participants to provide an estimate of monthly pension receipt for a vignette that is comparable to the descriptive survey: "Now think of a teacher who is 32 years old, works at a 40% employment level, and intends to maintain this level until retirement. She earns 4,200 CHF per month. What is your estimate: how much would she receive each month as a pension from her second pil-

lar of pension savings?" To minimize the response burden in the RCT, participants select their answers from a drop-down menu with options ranging from 600 CHF to 4,200 CHF in 200 CHF increments.

ii. Intervention. Participants watch the video corresponding to their treatment assignment. All groups complete a "knowledge check" question after the video, with 96% to 99% of respondents answering correctly (Online Appendix Figure A.4.2). After the wave 1 survey, the treatment group receives access to the Future Calculator.

iii. Wave 1 Outcomes. After the video, we conduct a brief end line survey. Following Deshpande and Dizon-Ross (2023), we measure participants' emotional reaction by asking how they feel about the future. To assess whether participants correctly apply the treatment information, we measure the relative ranking of child care costs in a vignette-based ranking exercise of four financial factors, analogous to the descriptive survey. We measure participants' interest in receiving different materials about financial planning to capture financial behavior (financial tools).[12] We ask participants about their employment plans for the next school year and in 10 years, as well as their desired level of employment under hypothetical scenarios. Online Appendix Table B.1.2 provides an overview of the main outcome variables, along with the corresponding question numbers of each questionnaire.

2. *Follow-up Survey.* We recontact participants roughly two months after the wave 1 survey for a follow-up survey. The response rate is 72% ($N = 1{,}707$) and is balanced across treatment and control groups (see Online Appendix Table A.7.4, columns (1) and (2)). We assess the retention of the treatment information in the follow-up survey using a similar ranking exercise and vi-

12. These consist of an information sheet with an overview of their own pension savings, a video explaining how to best discuss finances in a couple, access to an online tool to calculate the financial implications of different employment levels (Future Calculator), an online course on wealth accumulation and financial security for women, and a course for couples on how to address gaps in the occupational pension plan. We also give participants the option to sign up for a consultation with a financial expert specialized in advising women. This outcome is incentivized: participants enter a lottery to win a voucher valued at approximately $570 for a popular online retail platform upon completing the study. They are asked to choose between using this voucher for the online platform or opting for the consultation. We implement the chosen option accordingly.

gnette as in wave 1 to avoid measurement error (see Stantcheva 2023). We include an open-ended question asking respondents to describe the key factors they will consider in their employment decisions 10 years from now to understand whether the narratives around their labor supply decisions have changed. In terms of employment levels, we ask participants about their plans for the next school year and 10 years into the future. We also add an incentive-compatible measure of employment plans, requesting participants to indicate their planned employment level in 3, 5, and 10 years. To encourage truthful reporting, we explicitly inform them that these responses will be used to generate a forecast for the DoE to address potential future teacher shortages. We collect information on whether participants took any actions in response to the video they watched and include a reduced version of the Perceived Stress Scale (Scott and Kelley 1995), as well as questions about future fertility plans and perceived satisfaction with different life domains. We further ask questions about the timing of participants' employment decisions and constraints to implement their desired level of employment.

3. *Administrative Data.* We link our survey data with administrative employer data from the DoE in our region of study to examine treatment effects on contracted employment levels in the academic year after the intervention. These personnel data contain information on teachers' employment situation and allow us to observe a participant's employment level as a share of an FTE in the current academic year (see Online Appendix Table B.3.1 for a complete list of variables). We obtained administrative data for the years 2020–2023 for all teachers employed at the DoE in our region of study. We are able to link 91% of our wave 1 respondents with the administrative data for the subsequent academic year, and document in Online Appendix Table A.7.4 that there is no differential attrition by treatment status or observable characteristics (columns (3) and (4)). Our treatment effect estimates based on the administrative data thus provide internally valid estimates for teachers' employment levels for the DoE in our region of study.

One drawback of these data is that we cannot observe a teacher's potential employment in other, non-DoE jobs. For most teachers in our sample, however, it is plausible that the DoE data cover their primary source of employment: As displayed in Online Appendix Table A.1.1, Panel D, 94% of female teachers with chil-

dren in Switzerland work exclusively as public school teachers. Among those who hold another, nonteaching job, their employment in public school teaching amounts to 46% of an FTE, and 16% of an FTE in the nonteaching job, suggesting only a minor role for jobs outside of teaching.[13]

IV.D. Randomization Design

Because of the potential presence of spillovers between teachers within schools, we include a hold-out control group in the experimental design (Duflo and Saez 2003; Haushofer and Shapiro 2016). In particular, we implement a two-stage randomization design:

i. *First stage:* We randomize two-thirds of the schools into treatment schools and one-third of schools into control schools (referred to as the pure control group in the following). We stratify the sample by school size terciles (proxied by the number of female teachers aged 25–50 years), school type (kindergarten/primary or secondary), and type of municipality (rural, semi-urban, city). Online Appendix Table A.7.1 shows that the treatment and pure control schools are balanced on school-level characteristics.
ii. *Second stage:* We randomize teachers in treatment schools at the individual level. The individual-level randomization occurs during the survey just before the intervention video. We assign half of teachers to treatment and half to control (referred to as the spillover control group in the following). We stratify by full-time employment status and whether a participant is pregnant.

In both the spillover control group and the pure control group, teachers are randomized with equal probability to watch one of three control videos described in Section IV.B. Online Appendix Figure A.6.2 illustrates our experimental design and the sample size in each treatment arm. Online Appendix Table

13. Our treatment information is relevant even if teachers were to switch occupations, as pension contributions and receipt are governed by federal regulation and thus apply to any employer. The Federal Statistical Office documents a high retention rate (> 90% across five years) for teachers (BFS 2021). The year-to-year match rate for our main sample is similar to the match rate for all teachers (88%) and all women aged 25–55 (89%) employed at the DoE in 2022.

A.7.2 documents balance between treatment, spillover control, and pure control individuals.

IV.E. Empirical Strategy

For every primary outcome, we estimate the following specification:

$$(1) \quad Y_{is} = \beta_0 + \beta_1 \text{Treat}_{is} + \beta_2 \text{Spillover}_{is} + \beta_3 X_{is} + \beta_4 X_s + \gamma_f + \epsilon_{is},$$

where Y_{is} is the outcome of interest for individual i working in school s, Treat_{is} and Spillover_{is} are indicators for the treatment group and for the spillover control group (control individuals in treatment schools), respectively. X_{is} is a vector of individual-level baseline characteristics, and X_s are school-level controls. We include stratification-level fixed effects, γ_f. We use a post-double-selection LASSO to determine the set of controls (Belloni, Chernozhukov, and Wei 2016), and we feed the model with all individual-level baseline variables (pre-treatment) from our wave 1 survey as well as school-level controls (full list of variables in Online Appendix Table A.5.1). Standard errors are clustered at the school level.[14]

V. Does Information on the Long-Term Financial Costs of Reduced Labor Supply Affect Women's Behaviors?

V.A. Descriptive Patterns for Teachers

We use our RCT baseline data to document the two main patterns highlighted in the descriptive survey for our teacher sample. Similar to women in the general population, long-term financial implications are not top of mind for teachers when deciding on their employment level as a parent: in Online Appendix Figure A.3.1, about 15% of teachers mention any financial long-term factors and rather highlight dimensions related to child and own well-being.

14. Throughout our study, we follow our pre-analysis plan (accessible via Online Appendix Table C.1.1), with three exceptions. (i) Next to our main study region, we had originally planned to roll out the study in two additional, smaller regions (with a total number of female teachers of 5% and 20% relative to our main region of study), but we faced numerous implementation challenges. (ii) We do not use two additional survey questions as proxies for cost underestimation at baseline because they did not exhibit meaningful variation. (iii) We estimate both the treatment and spillover effect in a pooled specification relative to the pure control group.

Online Appendix Figure A.3.2 shows a histogram of teachers' pension estimates, which we use to assess the extent to which teachers may underestimate the financial implications of reduced hours. Similar to the general population, these estimates are quite heterogeneous, but teachers are not as overoptimistic: about a quarter of the teacher sample at baseline gives an estimate of pension receipt under part-time work that is too high, while a substantial share of teachers thinks that pension receipt is lower than the true value. Relative to the share of cost underestimators in the high education group in the descriptive survey (51%), this group is considerably smaller among teachers. One reason for this difference is likely that while both teachers and women in the descriptive survey provide similar pension receipt estimates relative to the vignette's monthly salary, teachers enjoy higher wage growth under (low) part-time hours.[15]

V.B. Financial Information and Demand for Financial Tools

Does providing information on the long-term financial implications of part-time work affect mothers' behavior? We examine treatment impacts on a prespecified financial index that combines two components: whether participants correctly apply the treatment information and take-up of financial tools.[16] Using a variant of the part-time vignette and the ranking exercise described in Section IV.C.1, we assess whether participants correctly rank child care costs after pension and earnings. We measure demand for financial tools with an index that captures respondents' willingness to sign up for resources related to financial planning, including an incentivized sign-up for a financial consultation with an expert. Table I, column (1) reports the treatment and spillover effects on the financial index. We find a positive treatment effect of 0.39 of a standard deviation. Column (2) shows that treated participants are 31.26 percentage points more likely to get the relative ranking of financial factors correct, relative to 54% of

15. Teachers' and the descriptive survey sample's median guess is that pension receipt constitutes 29% versus 28% of monthly salary, respectively. But projected pension receipt constitutes 40% of monthly salary (at age 33) for teachers versus 25% for the high-education group in the descriptive survey. These differences are due to wages growing faster for teachers (no part-time penalty) and gaps in pension contribution being larger than gaps in wages due to the minimum contribution threshold for the second pillar (see Section II.B).

16. All outcome indices are constructed by standardizing and weighting the respective components following a GLS weighting procedure (Anderson 2008).

TABLE I

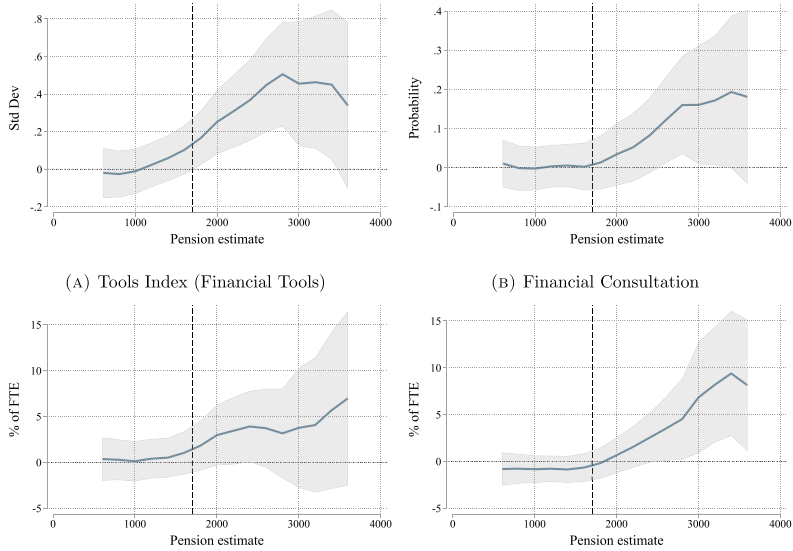
TREATMENT IMPACT ON FINANCIAL OUTCOMES

	Wave 1				Follow-up	
	Financial index (1)	Correct ranking (2)	Tools index (3)	Consultation (4)	Correct ranking (5)	Fin. long-term (6)
Panel A: Main estimates						
Treat	0.392***	0.313***	0.088*	0.020	0.226***	−0.020
	(0.046)	(0.022)	(0.050)	(0.023)	(0.029)	(0.020)
	[0.001]	[0.001]	[0.062]	[0.149]	[0.001]	[0.135]
Spillover	0.008	0.003	−0.004	0.013	0.034	−0.018
	(0.050)	(0.025)	(0.051)	(0.023)	(0.030)	(0.019)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Panel B: Heterogeneity						
Underestimators						
Treat	0.569***	0.297***	0.313***	0.069	0.279***	0.038
	(0.093)	(0.052)	(0.103)	(0.048)	(0.063)	(0.046)
	[0.001]	[0.001]	[0.002]	[0.066]	[0.001]	[0.158]
Spillover	−0.012	−0.028	0.009	0.015	0.087	−0.005
	(0.104)	(0.057)	(0.107)	(0.048)	(0.064)	(0.042)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]

TABLE I
CONTINUED

	Wave 1				Follow-up	
	Financial index (1)	Correct ranking (2)	Tools index (3)	Consultation (4)	Correct ranking (5)	Fin. long-term (6)
Overestimators						
Treat	0.344***	0.320***	0.022	0.007	0.205***	−0.037*
	(0.056)	(0.024)	(0.061)	(0.028)	(0.035)	(0.021)
	[0.001]	[0.001]	[0.362]	[0.362]	[0.001]	[0.070]
Spillover	0.040	0.013	0.022	0.014	0.013	−0.025
	(0.057)	(0.029)	(0.059)	(0.029)	(0.036)	(0.022)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Adjusted R^2	0.08	0.12	0.07	0.03	0.04	0.00
No. observations	2,216	2,216	2,216	2,216	1,656	1,642
Pure control mean	−0.00	0.54	−0.00	0.29	0.54	0.13
Pure control mean (under)	−0.06	0.54	−0.08	0.27	0.52	0.12
Pure control mean (over)	0.03	0.54	0.04	0.30	0.55	0.14
p-value	.05	.69	.02	.28	.32	.14

Notes. This table shows treatment and spillover effects on financial outcomes. Column (1): financial index, which aggregates the correct ranking (column (2)) and the tools index (column (3)). Columns (2) and (5): indicator for whether respondents correctly rank total future salary and pension savings above child care costs using the part-time vignette (see Section III.A). Column (3): tools index, measures the willingness to sign up to receive different information materials and resources related to financial planning, including the incentivized sign-up for a financial consultation. Column (4): indicator for signing up for a financial consultation (incentivized). Column (6): indicator for mentioning long-term financial factors in an open-ended text question about the most important factors considered for employment-level plans in 10 years. Data: wave 1 (columns (1)–(4)) and follow-up (column (5)–(6)). Panels A and B are estimated in separate regressions. Panel A: estimates of equation (1). Panel B interacts the treatment indicators in equation (1) with group indicators for cost under- versus overestimation and adds the group indicator to the set of controls. The adjusted R^2 is reported for Panel B. The control mean is reported for pure control group overall, under-, and overestimators. The p-value is for a test of equality of coefficients between under- and overestimators in the treatment group. All specifications use strata fixed effects and post-double-selection LASSO to determine the set of controls (Belloni, Chernozhukov, and Wei 2016), listed in Online Appendix Table A.5.2. Standard errors clustered at the school level are in parentheses, and sharpened q-values (Anderson 2008) for each row are in square brackets. * $p < .10$, ** $p < .05$, *** $p < .01$.



(A) Tools Index (Financial Tools)

(B) Financial Consultation

(C) Emp. Level 10 Years, Follow-up

(D) Δ Emp. Level 1 Year, Admin Data

FIGURE II

Nonparametric Heterogeneous Treatment Effects by Pension Estimate

This figure estimates treatment effects by respondents' part-time pension estimate for the part-time vignette using a series of locally weighted regressions for each bin of pension estimates (200 CHF). The dashed vertical line indicates the projected value. Triangular kernel with bandwidth 1,000, pension estimates above 3,600 CHF are binned at that value. All specifications use strata fixed effects. Ninety-five percent confidence intervals are based on standard errors clustered at the school level. The sample is restricted to treatment and pure control group. Data: wave 1 (Panels A, $N = 1{,}397$ and B, $N = 1{,}431$); follow-up (Panel C, $N = 1{,}064$); administrative data (Panel D, $N = 1{,}343$).

women in the pure control group. Column (3) shows an increase of 0.09 standard deviations on the tools index. We do not observe a statistically significant treatment impact on women's sign-up rates for the incentivized financial consultation in column (4). Results for the separate components of the financial tools index are displayed in Online Appendix Figure A.3.3. We do not find evidence of spillover effects for any of these outcomes in wave 1.

Do cost underestimators, that is, women who have overly optimistic priors around pension receipt under part-time work, respond more strongly to the treatment? To examine heterogeneity along this dimension, Figure II documents treatment effects by women's pension estimate at baseline using locally weighted

regressions. Both for the financial tools index (Panel A) and the financial consultation (Panel B), we observe no significant effect for women who give low or accurate pension estimates, but a positive slope for respondents with estimates above the correct value. In Table I, Panel B, and in all subsequent tables, we summarize these patterns by defining women as cost underestimators if their pension estimate at baseline is above the projected value, and as cost overestimators otherwise. We document heterogeneity by whether respondents are cost underestimators in a pooled specification that interacts the treatment indicators in equation (1) with group indicators for cost under- versus overestimation and adds a group indicator to the set of controls.[17] Panel B, column (1) shows that the treatment effect for the financial index is significantly higher for cost underestimators. This is driven by this group increasing their demand for financial tools by 0.31 standard deviations (column (3)). Column (2) documents that there is no differential effect for the ranking exercise as women learn about the information provided regardless of whether they are overly optimistic about part-time pension receipt. Taken together, these patterns highlight that the treatment increases knowledge about the relative magnitude of long-term financial factors more generally, while this information translates into higher demand for financial tools only for women who underestimate long-term costs.

In the follow-up survey, we assess the retention of the treatment information. Table I, column (5) documents that two months after the intervention, the treatment group is still significantly more likely (22.63 percentage points) to apply the treatment information correctly when presented with a similar vignette. We do not find a meaningful effect on the likelihood of mentioning long-term financial factors when respondents are asked about the decision factors for their labor supply in ten years (column (6)). There is no evidence of spillover effects for the full sample in the follow-up. Among cost underestimators, however, we measure an

17. We document in Online Appendix A.4.2 that results throughout the article are not sensitive to this particular definition of cost underestimation. We do not report separate treatment effects for the 154 women in our main sample who do not provide a pension estimate, with results qualitatively unchanged when excluding those respondents from the overall analysis. Online Appendix Table A.7.3 documents balance between the treatment, the spillover control, and pure control individuals in the subgroup of cost-underestimating women.

imprecise but economically meaningful 8.66 percentage points increase in correctly executing the ranking exercise (column (5)), suggesting some transfer of the treatment information.

V.C. Labor Supply Plans (Survey Data)

1. *Short-Term Labor Supply.* Table II examines planned changes in labor supply for the next school year directly after the treatment (wave 1) and in the follow-up. We define this outcome as the difference between teachers' planned employment level in the subsequent academic year relative to their current employment level measured in the administrative data. In Panel A, treated teachers on average plan to increase their employment level by 1.69 percentage points relative to the pure control group. The coefficient for the spillover group is not statistically significant, but is also positive. We show in Online Appendix Table A.3.1 that this positive spillover effect only materializes among later survey entrants who may have learned about the treatment material from their colleagues; it is not present for early survey takers.

The increase in planned labor supply is larger among cost-underestimators: in Panel B, we see that this group plans a sizable relative increase of 4.95 percentage points in their employment level, corresponding to an increase of 9% over the pure control mean (column (1)). While the effects in the full sample peter out by the time of the follow-up survey (Panel A, column (2)), the planned increase persists among cost underestimators, both for the treatment and spillover group (Panel B, column (2)).

2. *Long-Term Labor Supply.* Online Appendix Figure A.3.4 shows the density for women's planned level of employment in 10 years measured in wave 1 (Panel A) and the follow-up (Panels B and C). There is a visible shift in the distribution, with the mass of changes for the treatment group occurring between employment levels from 50% to 80% of an FTE. Table III reports treatment effects on long-term labor supply plans at 3, 5, and 10 years into the future based on the follow-up survey and wave 1. Because these measures are self-reported, we added an incentive-compatible elicitation in the follow-up survey by informing participants that their answers would be used to generate a forecast of the teacher workforce for the DoE. Columns (1)–(3) report estimates for the incentive-compatible elicitation at 3, 5, and

TABLE II

TREATMENT IMPACT ON SHORT-TERM LABOR SUPPLY OUTCOMES

	Δ*Employment level* 1 year		
	Wave 1 (1)	Follow-up (2)	Admin (3)
Panel A: Main estimates			
Treat	1.692**	0.141	−0.077
	(0.775)	(0.911)	(0.616)
	[0.098]	[1.000]	[1.000]
Spillover	1.275	0.522	0.061
	(0.800)	(0.894)	(0.582)
	[0.503]	[1.000]	[1.000]
Panel B: Heterogeneity			
Underestimators			
Treat	4.952***	3.536*	3.873***
	(1.694)	(2.000)	(1.321)
	[0.006]	[0.027]	[0.006]
Spillover	2.849*	4.238**	1.454
	(1.661)	(1.896)	(1.280)
	[0.096]	[0.085]	[0.150]
Overestimators			
Treat	0.967	−0.649	−0.999
	(0.919)	(1.042)	(0.748)
	[0.786]	[0.786]	[0.786]
Spillover	1.165	−0.451	0.065
	(0.984)	(1.084)	(0.726)
	[1.000]	[1.000]	[1.000]
Adjusted R^2	0.13	0.10	0.06
No. observations	2,302	1,687	2,152
Pure control mean (level)	54.75	55.37	53.30
Pure control mean	3.20	3.86	0.97
Pure control mean (under)	0.43	0.86	−1.42
Pure control mean (over)	3.69	4.41	1.56
p-value	.04	.07	.00

Notes. This table shows treatment and spillover effects on short-term labor supply. Column (1): change in next academic year's planned employment level in wave 1. Column (2): change in next academic year's planned employment level measured in the follow-up. Column (3): change in next academic year's employment level in the administrative data. All changes are relative to the employment level in administrative data at the time of the intervention (2022). Employment levels are measured as percent of a full-time equivalent. Panel A and B are estimated in separate regressions. Panel A: estimates of equation (1). Panel B interacts the treatment indicators in equation (1) with group indicators for cost under- versus overestimation and adds the group indicator to the set of controls. The adjusted R^2 is reported for Panel B. The control mean in levels is reported for the pure control group. Control mean (Δ*Employment level*) is reported for the pure control group overall, under-, and overestimators. The p-value is for a test of equality of coefficients between under- and overestimators in the treatment group. All specifications use strata fixed effects and post-double-selection LASSO to determine the set of controls (Belloni, Chernozhukov, and Wei 2016), listed in Online Appendix Table A.5.2. Standard errors are clustered at the school level in parentheses, and sharpened q-values (Anderson 2008) for each row are reported in square brackets. * $p < .10$, ** $p < .05$, *** $p < .01$.

TABLE III

TREATMENT IMPACT ON PLANNED LONG-TERM LABOR SUPPLY OUTCOMES

	Incentive compatible, follow-up			Follow-up	Wave 1	Follow-up
Employment level	3 years (1)	5 years (2)	10 years (3)	10 years (4)	10 years (5)	index (6)
Panel A: Main estimates						
Treat	0.564	1.006	0.663	1.063	3.129***	0.030
	(0.681)	(0.792)	(0.989)	(0.887)	(0.695)	(0.050)
	[0.840]	[0.629]	[0.840]	[0.629]	[0.001]	[0.840]
Spillover	0.390	−0.068	0.122	0.131	0.066	0.009
	(0.692)	(0.786)	(0.993)	(0.875)	(0.693)	(0.051)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Panel B: Heterogeneity						
Underestimators						
Treat	3.136*	4.351**	4.593*	4.335**	4.833***	0.274**
	(1.765)	(1.903)	(2.381)	(1.885)	(1.530)	(0.117)
	[0.036]	[0.030]	[0.036]	[0.030]	[0.011]	[0.030]
Spillover	2.087	0.428	−1.616	−1.731	0.619	0.003
	(1.767)	(1.819)	(2.241)	(1.885)	(1.614)	(0.115)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]

TABLE III
CONTINUED

Employment level	Incentive compatible, follow-up			Follow-up	Wave 1	Follow-up
	3 years (1)	5 years (2)	10 years (3)	10 years (4)	10 years (5)	index (6)
Overestimators						
Treat	−0.019	−0.058	−0.501	0.478	2.448***	−0.035
	(0.806)	(0.895)	(1.059)	(0.955)	(0.843)	(0.055)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.024]	[1.000]
Spillover	0.060	−0.255	0.441	0.969	−0.186	0.015
	(0.802)	(0.902)	(1.091)	(1.000)	(0.881)	(0.057)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Adjusted R^2	0.50	0.39	0.15	0.18	0.20	0.33
No. observations	1,652	1,641	1,636	1,684	2,295	1,626
Pure control mean	57.24	61.68	68.28	69.12	70.11	−0.00
Pure control mean (under)	55.04	60.17	67.33	68.60	69.01	−0.12
Pure control mean (over)	57.89	62.23	68.76	69.19	70.65	0.04
p-value	.12	.04	.04	.06	.19	.02

Notes. This table shows treatment and spillover effects on planned long-term labor supply. Columns (1)–(3): incentive-compatible elicitation of planned employment level in 3, 5, and 10 years for Department of Education measured in the follow-up. Column (4): planned employment level in 10 years measured in the follow-up (any employer). Column (5): planned employment level in 10 years measured in the wave 1 survey. Column (6): index across all long-term employment-level measures in the follow-up survey. For the incentive-compatible elicitation, we informed participants that their answers would be used to generate a forecast of the teacher workforce for the Department of Education. Employment levels are measured as percent of a full-time equivalent. Panels A and B are estimated in separate regressions. Panel A: estimates of equation (1). Panel B interacts the treatment indicators in equation (1) with group indicators for cost under- versus overestimation and adds the group indicator to the set of controls. The adjusted R^2 is reported for Panel B. The control mean is reported for the pure control group overall, and for under- versus overestimators. The p-value is for a test of equality of coefficients between under- and overestimators in the treatment group. All specifications use strata fixed effects and post-double-selection LASSO to determine the set of controls (Belloni, Chernozhukov, and Wei 2016), listed in Online Appendix Table A.5.2. Standard errors clustered at the school level are in parentheses, and sharpened q-values (Anderson 2008) for each row are reported in square brackets. * $p < .10$, ** $p < .05$, *** $p < .01$.

10 years, and columns (4) and (5) report employment plans at 10 years for any employer measured in the follow-up survey and in wave 1. We observe positive but statistically insignificant coefficients for medium-run employment levels in the overall sample. For longer-term employment levels 10 years into the future, the treatment group indicates a 3.13 percentage points higher level in wave 1 (column (5)). Estimates for the follow-up are somewhat smaller and noisy (columns (3) and (4)), and are not statistically different from zero when we combine all follow-up measures of long-term employment plans into an index (column (6)). We do not observe meaningful spillover effects on long-run labor supply plans in the full sample.

Consistent with the heterogeneous treatment effects on short-term labor supply plans, Figure II, Panel C illustrates that treated women who give low or accurate estimates for the part-time pension receipt do not meaningfully plan to increase their 10-year labor supply. However, we observe a positive slope for those with pension estimates above the true value. Table III, Panel B documents that treated cost underestimators plan to adjust their hours upward in the medium to long term. For the incentive-compatible measure in 3 years, we observe a marginally significant increase of 3.14 percentage points (column (1)), and systematic increases of around 4 percentage points for 5 and 10 years into the future across the different survey waves (columns (2)–(5)). This corresponds to an increase of between 6% and 7% over the pure control mean. This effect size is similar to the effect we find for short-term employment plans: cost underestimators increase their employment level for the next academic year and plan to sustain this higher employment level in the long run. While the coefficient on medium-run labor supply for cost underestimators in the spillover group is positive, this group's labor supply plan responses are generally not statistically significant for any outcome and are relatively small, especially for longer-term plans. We also find little evidence that the treatment would permanently change longer-term work intentions for women who give low or accurate estimates for the part-time pension. Although the treatment effect for this group is statistically significant and economically meaningful in wave 1 (column (5)), it dissipates by the time of the follow-up (columns (3) and (4)).

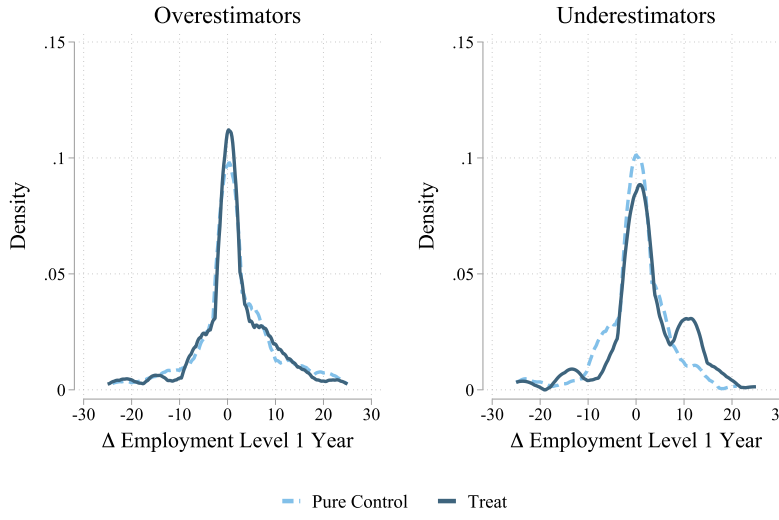


FIGURE III

Change in Labor Supply by Cost Underestimation, Administrative Data

This figure shows the density of the raw difference in employment level between 2023 (post-intervention) and 2022 (pre-intervention) by cost underestimation. Left: the change in employment level for cost overestimators ($N = 972$). Right: the change in employment level for cost underestimators ($N = 295$). Employment levels are measured as a percent of a full-time equivalent. The sample is restricted to the treatment and pure control groups and excludes outcome values above 25 and below −25. Data: administrative data.

V.D. Labor Supply (Administrative Data)

Our survey data suggest that cost underestimators in the treatment group plan to increase their labor supply relative to the pure control group. Do these intentions translate into actual employment adjustments one year after the intervention? Figure III plots the density of the difference in employment level between 2023 (one year after the intervention) and 2022 (before the intervention) in the raw administrative data. Though there is no shift in the distribution for cost overestimators, there is a visible increase in the employment level for cost underestimators in the treatment group, with the mass of changes concentrated around a 10 percentage points increase in employment level, corresponding to an additional half-day at work.

As documented in Table II, column (3), treated cost underestimators increase their employment level by 3.87 percentage

points relative to cost underestimators in the pure control group, which is similar to their planned increase immediately after the treatment. We examine treatment-effect heterogeneity nonparametrically in Figure II, Panel D. The pattern mirrors those of financial behavior and long-term employment plans with cost underestimators increasing their actual employment level while cost overestimators do not react. This asymmetry in response to the treatment information is consistent with cost overestimators already acknowledging the financial consequences of part-time work, even if they lack precise estimates. The treatment may not provide sufficiently novel information that challenges prior beliefs for this group. Consistent with financial outcomes in Table I indicating some learning spillovers, we find a positive but not statistically significant, coefficient for cost underestimators in the spillover group (Table II, column (3)), which amounts to approximately one-third of the average treatment effect.

The expansion of contracted working hours among cost underestimators represents a substantial increase of 7% over the mean employment level of the pure control group (53.30%). To put this magnitude into context, we can perform a back-of-the-envelope calculation: if we assume that—as indicated by longer-term labor supply plans—from age 40 onward, cost underestimators increase their employment level by 3.87 percentage points, these women will on average accumulate an additional 130,000 CHF in lifetime income and 40,000 CHF in pension wealth. This would shrink the gender gaps in lifetime income and pension receipt among teachers by almost one-fifth (18% and 18.5%, respectively).[18]

VI. Mechanisms

VI.A. Emotional Reaction

Through which channels does information about the long-term financial consequences of part-time work alter women's plans and behavior? We start by documenting women's emotional

18. Our own calculations with Future Calculator. We take the employment levels by age from the cross-sectional administrative data (see Online Appendix Figure A.1.1) and then calculate lifetime income, pension wealth, and monthly pension receipt for the average female and male teacher. Next, we assume a 3.9 percentage point increase in employment levels starting at age 40 for female teachers and recalculate these statistics.

response to the treatment. We ask participants how they feel about their future immediately after watching the video in wave 1. As shown in Table IV, column (1), women in the treatment group experience emotions that are more negative with a treatment effect of −0.41 standard deviations on an emotions index (see Online Appendix Figure A.3.5 for a detailed breakdown of all emotions). Cost underestimators experience a significantly more negative emotional response (−0.68 standard deviations). While learning about the financial costs of part-time work initially leads to emotional distress, the treatment could also empower women to make more informed decisions, thus reducing overall stress levels. Table IV, column (2) shows that the short-term discomfort reverses by the time of the follow-up. Women in the treatment group report feeling more in control and less stressed. Cost underestimators return to a neutral emotional state. This emotional reversal highlights that the treatment information—though somewhat inconvenient at first—allows women to take proactive measures to safeguard their (financial) future.[19]

VI.B. Engagement with the Study Topic and Patterns of Adjustments

We document that treated women engage more with the study topic in the follow-up. Table IV, columns (3)–(5) show that treated women are more likely to discuss the content of their video with their social circles. In column (3), women in the treatment group are 19.61 percentage points more likely to have spoken to someone. These treatment effects are similar for both under- and overestimators. Column (6) shows that treated women are 12.35 percentage points more likely to report having taken any action in response to their video. We show in Online Appendix Figure A.3.6 that a qualitative elicitation of adjustments taken renders conclusions similar to our quantitative results, where cost underestimators who take action are more likely to plan to work more.

Are women who face fewer short-term constraints better able to adjust to our treatment information? We report treatment ef-

19. We conducted a brief survey at the end of the 2023/24 academic year (1.5 years after the intervention) and again elicit the emotions and stress indices (see Online Appendix Table A.3.3). We find no effect of the treatment on stress levels in the long run (column (2)). This survey is preregistered at the AEA RCT registry, RCT ID 0013529.

TABLE IV

MECHANISMS: REACTIONS TO TREATMENT

	Emotions index	Stress index	Talk to			Take
	Wave 1 (1)	Follow-up (2)	Anybody (3)	Partner (4)	Colleague (5)	Action (6)
Panel A: Main estimates						
Treat	−0.412***	−0.124**	0.196***	0.177***	0.116***	0.124***
	(0.051)	(0.060)	(0.029)	(0.027)	(0.021)	(0.022)
	[0.001]	[0.007]	[0.001]	[0.001]	[0.001]	[0.001]
Spillover	−0.039	0.050	−0.053*	−0.026	0.011	0.011
	(0.049)	(0.057)	(0.029)	(0.026)	(0.017)	(0.016)
	[0.800]	[0.800]	[0.636]	[0.800]	[0.800]	[0.800]
Panel B: Heterogeneity						
Underestimators						
Treat	−0.680***	−0.035	0.141**	0.115*	0.101**	0.133***
	(0.113)	(0.130)	(0.067)	(0.066)	(0.051)	(0.051)
	[0.001]	[0.152]	[0.049]	[0.073]	[0.049]	[0.025]
Spillover	−0.081	0.053	−0.160**	−0.115*	−0.053	−0.014
	(0.112)	(0.127)	(0.067)	(0.065)	(0.041)	(0.040)
	[0.637]	[0.637]	[0.124]	[0.237]	[0.351]	[0.637]
Overestimators						
Treat	−0.337***	−0.117*	0.211***	0.187***	0.120***	0.134***
	(0.062)	(0.067)	(0.033)	(0.031)	(0.023)	(0.024)
	[0.001]	[0.014]	[0.001]	[0.001]	[0.001]	[0.001]
Spillover	−0.028	0.055	−0.015	−0.001	0.030	0.022
	(0.060)	(0.067)	(0.033)	(0.030)	(0.020)	(0.019)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Adjusted R^2	0.08	0.08	0.07	0.07	0.04	0.03

TABLE IV
CONTINUED

	Emotions index	Stress index	Talk to			Take
	Wave 1 (1)	Follow-up (2)	Anybody (3)	Partner (4)	Colleague (5)	Action (6)
No. observations	2,281	1,669	1,659	1,645	1,638	1,659
Pure control mean	−0.00	−0.00	0.38	0.30	0.08	0.08
Pure control mean (under)	0.16	−0.01	0.47	0.38	0.14	0.10
Pure control mean (over)	−0.05	−0.01	0.36	0.28	0.07	0.08
p-value	.01	.56	.35	.33	.74	.99

Notes. This table shows treatment and spillover effects on emotions and actions in response to the treatment. Column (1): emotions index measured in wave 1, with positive values indicating more positive emotions. Index across indicators for whether the respondent feels angry, anxious, hopeful, discouraged, happy, or motivated about their future. Column (2): stress index using a reduced version of the Perceived Stress Scale, with positive values indicating higher levels of stress. Columns (3)–(5): indicator for talking about the content of the video to anybody, their partner/family, or colleagues. Column (6): indicator for planning to take any action in response to the video. Column (1) based on data from wave 1, columns (2)–(6) based on data from the follow-up survey. Panels A and B estimated in separate regressions. Panel A: estimates of equation (1). Panel B interacts the treatment indicators in equation (1) with group indicators for cost under- versus overestimation and adds the group indicator to the set of controls. The adjusted R^2 is reported for Panel B. The control mean is reported for the pure control group overall, and for under- versus overestimators. The p-value is for a test of equality of coefficients between under- and overestimators in the treatment group. All specifications use strata fixed effects and post-double-selection LASSO to determine the set of controls (Belloni, Chernozhukov, and Wei 2016), listed in Online Appendix Table A.5.2. Standard errors clustered at the school level are in parentheses, and sharpened q-values (Anderson 2008) for each row are reported in square brackets. * $p < .10$, ** $p < .05$, *** $p < .01$.

fects for women's change in employment level for prespecified heterogeneity dimensions that proxy for current constraints in Online Appendix Figure A.3.7. We observe no meaningful differences across women who report more scope of increasing their labor supply in terms of the ease with which they could organize their family life, the age of their youngest child, or their own gender norms.

VI.C. Household-Level Adjustments

Given that we observe an increase in employment level among cost underestimators, we also explore adjustments within the household in the follow-up. As displayed in Online Appendix Table A.3.2, we observe no treatment impact on partners' planned labor supply for the next school year (column (1)) or on future fertility (column (2)). However, cost underestimators in the treatment group report being less satisfied: We find a negative treatment effect of −0.28 standard deviations on an index across all five satisfaction measures (column (3)). This is driven by cost underestimators being less satisfied with their friends' and family's understanding of the challenges they face as a working mother (column (4)). We find neither a statistically significant effect on satisfaction with the division of household tasks nor satisfaction with their partnership. However, these coefficients are negative for cost underestimators (columns (5) and (6)).[20]

VI.D. Which Women Are Cost Underestimators and Why Might They Adjust?

Most of our treatment effects are stronger among the group of cost underestimators, that is, women whose priors about pension receipt at baseline were overly optimistic. To better understand why this group is more likely to make an adjustment, we explore the correlates of underestimating the cost of part-time work in the descriptive survey and our RCT sample of teachers. Online Appendix Table A.3.4, Panels A–C show that cost underestimators do not meaningfully differ in terms of demographic

20. In a brief survey conducted at the end of the 2023/24 academic year (1.5 years after the intervention), we find no effect on overall satisfaction, see Online Appendix Table A.3.3, columns (3)–(6). Consistent with cost underestimators working more, we find a (marginally significant) negative effect on the perceived ease of coordinating household tasks compared to previous years (column (7)).

characteristics or current employment level, a pattern that is also present in the descriptive survey (Online Appendix Figure A.2.6, Panel A). In both samples, however, cost underestimators tend to be more gender-conservative (Online Appendix Figure A.2.6, Panel B and Online Appendix Table A.3.4, Panel C) and show substantially less interest in financial topics (Online Appendix Table A.3.4, Panel F). Cost underestimators in the RCT sample plan to work at lower employment levels in the medium to long term (Panel G).

While purely descriptive, these differences may offer some intuition on why cost underestimators adjust in response to the treatment information: underestimating the costs of reduced hours correlates with a general lack of knowledge and interest in financial matters. Paired with being more gender-conservative, this may provide a possible explanation for why this group is poorly informed. The treatment thus provides a novel piece of information, as cost underestimators learn that they are worse off than they had previously anticipated and subsequently re-optimize toward an overall higher level of labor supply. In contrast, cost overestimators do not react to the treatment information, possibly because it does not sufficiently challenge their prior beliefs. This asymmetric response is consistent with the notion that agents tend to react more strongly to losses relative to gains (Kahneman and Tversky 1979). Taken together, these patterns point to belief updating through information provision, rather than salience or priming effects, at the core of the observed adjustment among cost underestimators (Haaland, Roth, and Wohlfart 2023).

VII. Robustness

Online Appendix A.4 documents the robustness of our main RCT results with respect to experimenter demand effects in the survey measures (Online Appendix A.4.1) and the choice of estimation sample and specification (Online Appendix A.4.2). We also show that our study logistics were appropriately timed to give participants the opportunity to respond and that participants engaged with the treatment material as expected (Online Appendix A.4.3).

In Online Appendix A.4.4, we further examine the extent to which our results generalize beyond the occupation of teachers. We document that incremental increases in employment levels are feasible across a range of occupations: 85% of respondents

in the descriptive survey report being able to increase their employment level by half of a day. Using data from the SLFS, we also show in Online Appendix Figure A.1.3 that employment levels across different sectors cover most of the domain between 20% and 100% of an FTE, suggesting that schedules accommodating half-day increments may be more broadly feasible. Finally, we document similar short-term results of our intervention among a small sample of pregnant women in Switzerland recruited via a popular pregnancy app. In this sample, we measure cost underestimation based on overly optimistic expectations about wage growth under part-time work (see Online Appendix Figure A.4.3). Eliciting outcomes directly after the intervention video, Online Appendix Table A.4.10, Panel A documents positive treatment effects on participants' planned employment level when their child will be one year old (column (3)), and on plans to increase their employment level further in the future (column (4)). Panel B shows that cost-underestimating women drive these effects. While arguably based on a small sample, these results demonstrate that the findings from our main RCT could be more broadly applicable to women in occupations outside of teaching.

VIII. Conclusion

We shed light on the factors that mothers consider when making labor supply decisions and provide descriptive evidence that long-term financial consequences of reduced hours are not top of mind. We also show that a substantial share of women hold beliefs that are overly optimistic regarding the long-term financial implications of part-time work. By conducting a large-scale field experiment that combines rich surveys with administrative data on employment outcomes, we show that informing mothers about the long-term consequences of reduced employment leads to changes in their financial behavior and shifts their future labor supply plans upward. These changes are concentrated among women who are overly optimistic with respect to pension receipt under (low) part-time hours. Using linked employer administrative data, we show that the actual employment level of these cost underestimators increases significantly one year later. The magnitude of this adjustment is substantial: the expansion of contracted working hours among cost underestimators represents a 7% increase over the mean employment level of the pure control group. As the shift in long-term labor supply plans suggests,

women plan to sustain this increase. Such an adjustment would reduce the gender gaps in lifetime income and pension receipt among teachers by nearly one-fifth (around 18%).

Our findings offer policy-relevant insights. Although the overwhelming majority of women indicate that financial information on the long-term implications of different employment levels could be useful in making decisions about labor supply, very few women calculate these numbers for their employment decisions after having children. The results of our experiment demonstrate that a simple, low-cost intervention can generate relatively large responses in behavior, in particular among the least informed group. Our suggestive evidence on spillover effects also indicates that this information intervention initiates broader discussions in social networks, potentially leading to multiplier effects.

Given the observed drop in maternal labor force participation and income after the birth of a first child in many countries, this article serves as a proof of concept that information about the substantial financial consequences of these decisions can help women better plan for their future. While public schools provide an ideal laboratory to deliver objective and timely information about the long-run financial costs, they present two specificities: The deterministic salary scales allow us to abstract from uncertainty around wage-growth expectations related to promotions, and teachers can adjust their labor supply flexibly in small increments on a yearly basis. While our results replicate in a small sample of pregnant women from the general population, further research is needed to properly understand how choice under uncertainty may shape maternal labor supply.

Overall, our findings suggest that simple projections of the financial trade-offs associated with different employment levels can be a valuable tool for decision-making. More broadly, emphasizing the long-term financial implications within family policies—such as child care expansions and subsidies—may make these reforms more effective in promoting mothers' labor force participation.

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

The code underlying this article is available in Costa-Ramón et al. (2026), in the Harvard Dataverse, https://doi.org/10.7910/DVN/A9ZFFF.

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