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Beliefs About Breastfeeding: Informing (Future) Mothers

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Beliefs About Breastfeeding: Informing (Future) Mothers*

Abstract

Breastfeeding improves child and maternal outcomes, but it may also carry labor-market costs for mothers. This paper examines whether women hold accurate beliefs about benefits and costs of breastfeeding and whether evidence-based information can change these beliefs and breastfeeding intentions. We design and implement a survey experiment with UK women with childbearing intentions. We first elicit their prior beliefs on the effects of breastfeeding on child health, child cognitive outcomes, maternal health, and maternal labor-market outcomes. We then randomize participants to receive targeted information treatments based on existing evidence. We document substantial baseline misperceptions. Providing information leads to significant belief updating across three domains and improves belief accuracy. Treatment effects are largest among women who initially underestimated breastfeeding's effects. Information increases intended breastfeeding duration by approximately 0.67 months on average, with the strongest response among childless women, suggesting that information interventions are a potentially powerful policy tool.

JEL classification

D83, I12, I18, J13, J22

Keywords

breastfeeding, beliefs, information treatment, misperceptions, maternal labor supply, survey experiment

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1 Introduction

Breastfeeding entails a multidimensional trade-off. A well-established body of literature documents its positive effects on children's health and cognitive development, as well as maternal health (Kramer et al., 2001, 2008; Chowdhury et al., 2015). However, breastfeeding can also interact with maternal employment patterns and long-term career progression (Baker and Milligan, 2008; Lawler and Yewell, 2023; Hatsor and Shurtz, 2025). Despite sustained efforts by public health bodies, breastfeeding rates in the UK remain relatively low. While 81% of UK mothers initiated breastfeeding at birth in 2010, only 23% were still breastfeeding exclusively at 6 weeks (McAndrew et al., 2012), with strong socioeconomic gradients (Oakley et al., 2013). Progress since has been slow: the most recent national survey reports 86% initiation and 47% exclusive breastfeeding at 6 weeks (Office for Health Improvement and Disparities, 2026).[1] Initiation and duration are lowest among younger mothers, white mothers, and those with lower education, lower socioeconomic status, or residence in more deprived areas (Oakley et al., 2016). Rates have shown little improvement in recent years (Royal College of Paediatrics and Child Health, 2020).

A natural explanation for these patterns is that infant-feeding decisions are shaped not only by constraints, but also by what women believe breastfeeding does. Under a standard model of maternal choice, a mother weighs the expected returns to breastfeeding for her child and for herself against its expected costs in time, discomfort, and forgone earnings. Because none of these returns is directly observable at the time the decision is made, the choice is governed by her subjective beliefs rather than by the true parameters. A substantial literature in public health and psychology establishes that attitudes, intentions, and self-efficacy predict both initiation and duration (Spitzmueller et al., 2016; Wallenborn et al., 2019; Saavedra Sanchez et al., 2024; Wu et al., 2025). What this literature does not tell us is whether women's beliefs about the *magnitude* of breastfeeding's effects are accurate, in which direction they err, and whether they are responsive to evidence. Answering these questions requires eliciting quantitative beliefs about specific, well-identified effects and then exposing them to information, an approach now standard in economics but not yet applied to this setting (Wiswall and Zafar, 2015; Haaland et al., 2023). The distinction is not merely one of measurement, and it carries direct policy content. If women overestimate the costs or underestimate the benefits, their intended and realized behaviors may differ from those based on accurate information. Conversely, overly optimistic beliefs may lead to disillusionment and shorter breastfeeding durations when women encounter reality (Gianni et al., 2019; McNee et al., 2025). Evidence from other domains shows that misperceptions are widespread and can affect attitudes toward immigration (Haaland and Roth, 2020), racial discrimination (Haaland and Roth, 2023), the gender wage gap (Settele, 2022), and the effectiveness of psychotherapy (Roth et al., 2024). Thus, providing accurate information can help correct these misperceptions and shift intended choices (Bursztyn et al., 2020; Haaland and Roth, 2020, 2023; Cortés et al., 2024; Roth et al., 2024).

[1] The 2010 survey is the last UK-wide round; the 2024 survey covers England only and, due to changes in sampling frame and mode, is not directly comparable.

This paper provides the first evidence on the accuracy of women's quantitative beliefs about the returns to breastfeeding and on whether those beliefs, and the intentions they support, respond to information. We ask three questions. First, how accurate are women's beliefs about the effects of breastfeeding on child health, child cognitive development, maternal health, and maternal labor-market outcomes, and does the direction of the error differ across these domains? Second, do women update their beliefs when presented with evidence-based information, and does the extent of revision depend on the sign and the original gap? Third, do these belief updates translate into changes in breastfeeding intentions?

To answer these questions, we design and implement a novel survey experiment on a sample of 1,337 women aged 23 to 39 living in the UK, recruited through Prolific and screened to be in a relationship of at least one year and to intend to have children. We elicit quantitative point beliefs in four domains – infant gastrointestinal illness, child cognitive development, maternal ovarian-cancer risk, and maternal full-time employment – using a common format. For each domain we give the respondent the observed level of the outcome among children who were not breastfed, or among mothers who did not breastfeed, and ask them to report the corresponding level under breastfeeding. Beliefs are thus denominated in the same units as the estimates available in the literature, so that the gap between a woman's prior and the evidence is directly measurable rather than inferred from an attitude scale. Respondents are then randomly assigned either to a control group, which proceeds directly to the second elicitation, or to a treatment group, which receives four short evidence summaries drawn from the PROBIT trial (Kramer et al., 2001, 2008), the Million Women Study (Gaitskell et al., 2018), and our own estimates from Understanding Society, with the order of the summaries randomized. We re-elicit beliefs on the same four questions and, to limit anchoring and to test for spillovers across domains, on four further questions that cover the same domains through different outcome measures. Breastfeeding intentions and post-birth work plans are elicited both before and after the treatment, which allows us to trace the chain from information to beliefs to intended behavior within a single instrument.

The information we provide is not uniformly favorable to breastfeeding. The employment module informs treated women that mothers who worked full time before having their first child and breastfed are roughly nine percentage points less likely to be working full time three to seven years after birth than otherwise similar mothers who did not. The experiment therefore delivers good news in some domains and bad news in others, which allows us to ask whether women revise symmetrically and how they weigh gains to both themselves and the child against costs to themselves when forming intentions.

Our findings are as follows. Beliefs are inaccurate in all four domains at baseline, and the direction of the error differs systematically across them. Women understate the benefits of breastfeeding for their children: they expect diarrhoea incidence among breastfed infants to lie above the benchmark, and they place average child IQ below it. They also understate its labor-market cost, expecting a higher rate of full-time employment among mothers who breastfed than we estimate in the data. Maternal health is the one domain in which the error runs the other way, and there it is large: the evidence implies only a modest reduction in

ovarian-cancer incidence, yet respondents expect a reduction many times that size. Women therefore hold an overly favorable view of what breastfeeding does for their own health, while understating both what it does for their children and what it costs them at work.

Information moves beliefs in three of the four domains, and moves them toward the evidence. Treated respondents revise their beliefs about child IQ, maternal ovarian-cancer risk, and maternal full-time employment by between 0.24 and 0.60 standard deviations, in each case toward the benchmark. The treatment reduces the average absolute belief error by more than half in these domains, so it does not merely shift beliefs but makes them more accurate. Beliefs about child diarrhoea are the exception and do not respond. Updating is strongly asymmetric with respect to the initial error: women whose priors begin furthest from the evidence move the most, so the average effects understate what information does for those who are most misinformed.

These revisions carry through to intended behavior. Treated women intend to breastfeed 0.67 months longer than controls, an increase of roughly 8%, and they are 1.6 percentage points less likely to plan full-time work after birth. The treatment does not raise the probability of reporting that one is very likely to breastfeed. Yet this null masks movement along the full likelihood scale: when we separate upward from downward revisions, treatment raises the probability of an upward revision in breastfeeding likelihood by 2.8 percentage points and leaves downward revisions unaffected. Intended duration shows the same one-sided pattern, with a 10.7 percentage-point increase in the probability of an upward revision and no effect on downward revisions. Information thus moves women up the intention scale without necessarily pushing them into the top category, the response that baseline underestimation would predict. Treatment effects on duration are larger among women with less favorable prior beliefs about breastfeeding's implications for child IQ and maternal employment. Finally, the duration effect is concentrated among women who have not yet had a child, whose intended duration rises by about a month, more than three times the response of women who already have children.

Related literature. This paper speaks to three strands of literature. The first estimates the effects of breastfeeding on child and maternal outcomes; its estimates are the benchmark against which we measure the accuracy of beliefs and the content of the information we provide. The second studies the determinants of breastfeeding decisions, to which we add subjective beliefs as a determinant that is both measurable and manipulable. The third uses information experiments to study belief formation and updating; we build on its standard design and extend it to information about several outcomes at once.

Let's start from the evidence on the consequences of breastfeeding. For child health, the Promotion of Breastfeeding Intervention Trial (PROBIT), a cluster-randomized trial of over 17,000 mother–infant pairs in Belarus, showed that an intervention promoting breastfeeding reduced the incidence of gastrointestinal infection and atopic eczema in the first year of life (Kramer et al., 2001); Lawler and Skira (2025) provides further evidence using a policy-variation design. Brenøe et al. (2026) revisit the PROBIT data and show that these gains

operate largely through nutrition: increased caloric intake accounts for most of the effect on weight-for-age in infancy, and the effect persists into adolescence.

For cognitive development, Kramer et al. (2008) find gains in IQ and academic ratings at age 6.5 in the PROBIT cohort. The size of these gains is debated: IQ assessors were not blinded to treatment status, and at age 16 the trial finds no effect on overall neurocognitive function, although a smaller benefit persists for verbal ability (Yang et al., 2018). Evidence from other settings and research designs, however, supports a positive effect of breastfeeding on cognition. Studies using rich prenatal controls with matching or doubly robust estimators find gains of roughly 0.1 to 0.2 standard deviations in test scores through age 14 to 18 (Borra et al., 2012; Khudri and Hussey, 2025). A natural experiment exploiting weekend variation in hospital breastfeeding support in the United Kingdom finds larger effects, of about half a standard deviation, among children of mothers with low education (Fitzsimons and Vera-Hernández, 2022). These gains rise with duration up to eight to twelve months and level off thereafter (Borra et al., 2012; Khudri and Hussey, 2025), and they persist through adolescence, although their size in adolescence differs across studies. Descriptive evidence also links breastfeeding to early cognitive outcomes and to long-run educational and labor-market attainment (Rothstein, 2013; Bjerregaard et al., 2024). The PROBIT estimate we use in the information treatment therefore lies within a literature that consistently finds cognitive benefits of breastfeeding, even if their magnitude varies across studies.

On the maternal side, breastfeeding is associated with lower risks of ovarian and breast cancer. For ovarian cancer, meta-analytic and pooled case-control estimates imply roughly 25–30% lower odds among women who ever breastfed, with odds ratios of 0.70 (Chowdhury et al., 2015) and 0.76 (Babic et al., 2020), while the prospective Million Women Study finds about a 10% lower risk per 12 months of breastfeeding (Gaitskell et al., 2018). Evidence on maternal labor-market outcomes is more recent and relies on quasi-experimental variation: Baker and Milligan (2008) exploit Canadian maternity-leave mandates, Lawler and Yewell (2023) hospital postpartum-care regulations, and Hatsor and Shurtz (2025) an infant-formula scare. Two features of this body of work matter for our design. First, the evidence on child outcomes comes from a randomized trial, so we can present it to respondents as causal rather than as a correlation; the evidence on maternal health comes from large cohort studies that adjust for observed characteristics. Second, the evidence points in opposite directions: breastfeeding benefits the child's health and cognitive skills and the mother's health, but it carries a cost to maternal labor-market outcomes. This makes the decision a genuine trade-off rather than a matter of knowing more, and because the information does not uniformly favor breastfeeding, it also limits concerns about experimenter demand.

A second strand investigates the determinants of breastfeeding. This work concentrates on constraints and on the price of the alternatives: parental-leave entitlements (Baker and Milligan, 2008; Kottwitz et al., 2016; Pac et al., 2023), hospital and postpartum-care regulation (Lawler and Yewell, 2023), workplace conditions and employer accommodation (Spitzmueller et al., 2016; Del Bono and Pronzato, 2024), promotion interventions delivered through health systems (Kramer et al., 2001; Lawler and Skira, 2025), and the quality and availability of infant formula (Hatsor and Shurtz, 2025). A parallel literature in public health

and health psychology has established that attitudes, intentions, and self-efficacy are strong proximate predictors of initiation and duration (Wallenborn et al., 2019; Saavedra Sanchez et al., 2024; Wu et al., 2025). That literature measures orientation toward breastfeeding rather than knowledge of its returns: an attitude scale records how favorably a woman regards breastfeeding, but it cannot say whether she is right about how much it raises her child's test scores or how much it benefits her health or costs her at work, and it therefore cannot tell us whether her orientation would survive contact with the evidence. We take beliefs to be quantitative objects, elicit them in the units of the estimates reported above, and show that they are inaccurate in ways that differ by domain and that they respond to information. This signals that beliefs can be considered a potential policy instrument.

Our design belongs to the now-large literature on information-provision experiments, surveyed methodologically by Haaland et al. (2023). That literature has documented misperceptions and their consequences across a wide range of settings, including immigration and labor markets (Haaland and Roth, 2020), racial discrimination (Haaland and Roth, 2023), the gender wage gap (Settele, 2022), norms about women's employment (Bursztyn et al., 2020; Cortés et al., 2024), the returns to schooling choices (Wiswall and Zafar, 2015), and the effectiveness of psychotherapy (Roth et al., 2024). Most of these studies correct a misperception about a single object and deliver news of a single valence. Our setting differs in three respects. First, each treated respondent receives news about four distinct outcomes at once, which allows us to observe domain-specific updating within the same individual and to test for spillovers across domains rather than assuming them away. Second, the information is deliberately of mixed direction. The evidence favors breastfeeding for child health, child cognitive skills, and maternal health, but it is unfavorable for maternal career. We can therefore ask whether women revise symmetrically in response to good and bad news, and how they weigh the domains against each other. Third, and unlike the policy attitudes that are the usual object of study, the returns at stake accrue largely to someone other than the decision-maker, which makes breastfeeding a natural setting in which to observe how information about a child's outcomes competes with information about a mother's own.

Our contribution is thus threefold. We provide the first quantitative measurement of what women believe breastfeeding does, across the child and maternal domains jointly, and show that the misperception is large and directionally specific rather than uniformly optimistic or pessimistic. We show experimentally that these beliefs are malleable, and that correcting them improves accuracy. We then trace belief updating through to intended behavior, identifying which domains carry the effect and where women respond, which may be important for policy designing.

The remainder of the paper is organized as follows. Section 2 describes the survey design, sample, and data. Section 3 presents the empirical approach. Section 4 reports the results. Section 5 concludes.

2 Survey Design and Data

2.1 Recruitment and Sample

We administered the main survey on the platform Prolific to women residing in the UK. Data was collected from February to March of 2026.[2] The current analysis uses a sample of 1,337 respondents. We used information on Prolific as eligibility criteria, requiring that the respondents be female, aged 23–39, living in the UK, and in a relationship with or without children.

Table A.1 in Appendix A reports summary statistics for the estimation sample, separately by treatment arm. Respondents are on average 31 years old; 75% hold a university degree, 79% identify as White, 88% are employed, and 62% work full-time. The sample is positively selected on education and labor-force attachment relative to UK women of childbearing age generally. Table A.2 in Appendix A compares the sample to the general population of UK women aged 23–39, using the most recent Annual Population Survey (APS, January–December 2025). Age, ethnicity, and weekly hours worked are closely matched, but the sample is positively selected on education (75% hold a university degree, versus 54% in the general population) and, to a lesser extent, on employment (88% versus 80%) and full-time work (62% versus 53%); among employed respondents specifically, the sample also skews toward higher earners than employed women generally, consistent with its higher educational attainment. This is a pattern consistent with recruitment through an online panel such as Prolific. We therefore interpret our estimates as internally valid for this relatively selected population.

We included two prescreening questions to ensure that the respondents were part of the survey's target population. First, we asked if respondents intend to have children in the future. Second, we asked about the length of their relationship to screen out those who have been in a relationship for less than one year. These questions ensure that the respondents are likely to be in stable relationships and potentially making joint decisions about childbearing and breastfeeding in the future.

Survey flow. After the prescreening questions and participant information and consent, the main survey consists of the following sequential blocks:

1. Attention check at the start of the survey.

2. Intended work plan after childbirth and prior breastfeeding intentions, including factors hindering breastfeeding.[3]

3. Prior beliefs elicitation on the four outcome domains described below.

[2] The survey was fielded in several batches over this period. Base payment varied across batches, from £1.50 to £2.50, to encourage participation.

[3] The question on factors hindering breastfeeding was shown only to respondents who reported being very or somewhat unlikely to breastfeed; 97 respondents answered it. Nearly half chose "Prefer not to say," leaving 16 to 19 respondents for each substantive reason (job-related factors, lack of support, and no perceived benefit). These subsamples are too small to support heterogeneity analysis.

4. Confidence check to measure subjective uncertainty around prior beliefs.

5. Randomized information treatment.

6. Posterior beliefs elicitation, including both the same four questions as in Block 3 and four additional questions on the same domains but with different outcomes, to minimize anchoring effects.

7. Posterior work plan and breastfeeding intentions.

8. Final attention check and expected timing of having children.

We follow Haaland et al. (2023) and include attention checks at the beginning and end of the survey.[4] Since we elicit quantitative point beliefs, which do not allow respondents to express uncertainty, we also include a qualitative question about how confident respondents are in their answers. This allows us to distinguish between strong misperceptions and a lack of knowledge, and to examine whether subjective confidence is related to the accuracy of beliefs and to the degree of updating.[5] In the Appendix Section C.1, we provide screenshots of the whole survey.

2.2 Prior Beliefs and Intentions Elicitation

We ask participants about their labor market intentions after childbirth, their breastfeeding intentions, and, for those somewhat or very unlikely to breastfeed, their reasons for not breastfeeding. Focusing on breastfeeding intentions, we distinguish between extensive and intensive margin. We measure the former using a five-point scale from "very unlikely" to "very likely," and the latter through intended duration among neutral or likely respondents, with three categories: three months or less, four–six months, and seven months or more. For the empirical analysis, we construct a quantitative measure of intended breastfeeding duration by mapping each category to the corresponding mean duration among mothers in Understanding Society, assigning zero to respondents who are somewhat or very unlikely to breastfeed.[6]

We then elicit quantitative prior beliefs in four outcome domains. For each domain, we provide an outcome figure for children or mothers with no breastfeeding, or limited breastfeeding in the IQ question, and ask respondents for the corresponding outcome under

[4] The survey also includes a standard numeracy question before the prior belief elicitation. It is answered correctly by 97.6% of respondents, and incorrect answers are evenly split across treatment arms; excluding these respondents does not alter the results.

[5] Belief questions are incentivized for accuracy: at the end of the survey one belief question is drawn at random, and respondents earn a £0.50 bonus if their answer lies within two units of the corresponding reference value. All belief questions are eligible for selection except the one on GCSE attainment, because the relevant figure is based on a purely descriptive study. Both the pre- and the post-treatment questions enter the draw. 26% of respondents earned the bonus.

[6] We assign each category the mean breastfeeding duration among Understanding Society mothers reporting durations within the corresponding range: 1–91 days for "three months or less," 92–182 days for "four to six months," and 183 or more days for "seven months or more." We convert days to months by dividing by 30.44. This anchors the categorical responses to the empirical duration distribution in the same representative sample used to calculate the full-time employment statistics presented in the information treatment (Section 2.4).

breastfeeding. The four questions are as follows. Please see the related screenshots in the Appendix Section C.

Children's health. "In the UK, 27% of infants aged 6 months who have not been breastfed report one or more episodes of diarrhoea (a common symptom of gastroenteritis). What do you think the percentage would be if these children were breastfed for 4 months or longer?" [slider 0-100, pre-set at 27]

Children's cognitive skills. "IQ is the most widely used measure of intelligence. For adults, the average IQ is 100, and 9 out of 10 people have an IQ between 85 and 115. According to a UK study, the average IQ of children aged 8.5 who were not breastfed or were breastfed for less than one month is 101. What do you think the average IQ of these children would be if there were breastfed for at least 6 months?" [slider 85-115, pre-set at 101]

Maternal health. "A large UK study of around 800,000 women aged 50 and older shows that among those who never breastfed, there are about 50 cases of ovarian cancer per 100,000 women per year. How many women out of 100,000 who breastfeed do you think would develop ovarian cancer?" [slider 0-100, pre-set at 50]

Maternal labor-market outcomes. "Using data from a UK Survey, we calculate that among mothers who were working full-time before the birth of their first child and chose not to breastfeed, 30% are working full-time 3 to 7 years after the birth of their child. We then computed the same statistics for women with similar income and education levels who breastfed for 7 months or longer. What percentage of those mothers breastfeeding 7 months or longer do you believe is working full-time?" [slider 0-100, pre-set at 30]

2.3 Baseline Intentions and Beliefs

Table 1 summarizes participants' intentions and beliefs before the information treatment. Panel A shows that breastfeeding intentions are generally high: 60.4% of respondents report being very likely to breastfeed and a further 28.0% are somewhat likely. Only 7.1% are somewhat or very unlikely, while 4.5% are neutral. Intended duration nevertheless varies substantially: 40.4% fall in the four–six-month category and 46.2% in the seven-months-or-more category. The constructed duration measure averages 7.89 months, with a standard deviation of 4.52 months. About 48.1% intend to work full-time after childbirth.

Panel B reports the means and standard deviations of prior beliefs. Respondents report an average infant diarrhoea incidence under breastfeeding of 17.81%, an average child IQ of 104.56, an ovarian cancer incidence of 35.72 cases per 100,000 women per year, and a maternal full-time employment rate of 27.21%. Beliefs vary substantially across respondents: the corresponding standard deviations are 13.23 percentage points, 4.59 IQ points, 15.33 cases per 100,000 women per year, and 15.93 percentage points.

Table 1. Baseline Intentions and Beliefs

	Mean / share	SD
Panel A: Baseline intentions		
Likelihood of breastfeeding (%)		
Very unlikely	1.94	–
Somewhat unlikely	5.16	–
Neither likely nor unlikely	4.49	–
Somewhat likely	28.05	–
Very likely	60.36	–
Intended breastfeeding duration (%)		
Zero (assigned)	7.03	–
Three months or less	6.36	–
Four–six months	40.39	–
Seven months or more	46.22	–
Constructed duration (months)	7.89	4.52
Intends full-time work after childbirth (%)	48.09	–
Panel B: Prior beliefs		
Infant diarrhoea (%)	17.81	13.23
Child IQ (points)	104.56	4.59
Ovarian cancer (cases per 100,000 per year)	35.72	15.33
Maternal full-time employment (%)	27.21	15.93

Notes: N = 1,337 for all variables. All measures are pre-treatment. Category shares are reported in percent; standard deviations are reported for continuous variables. Duration categories include recorded zeros. Positive categories are mapped to mean breastfeeding durations in Understanding Society (1.216, 5.023, and 12.515 months, respectively); zeros remain zero. Full-time work includes returning to the current or a different employer. Ovarian cancer incidence is measured per 100,000 women per year.

We next examine whether these beliefs are associated with intended breastfeeding choices. Table 2 reports separate regressions of the baseline very-likely indicator (Panel A) and constructed intended duration (Panel B) on each standardized prior belief, controlling for individual characteristics. A one-standard-deviation lower perceived incidence of infant diarrhoea is associated with a 5.0 percentage-point higher probability of being very likely to breastfeed and 0.53 additional months of intended breastfeeding. The corresponding associations for a one-standard-deviation higher perceived child IQ are 7.6 percentage points and 0.65 months; for a one-standard-deviation lower perceived ovarian cancer incidence, they are 6.3 percentage points and 0.66 months. All six coefficients are statistically significant at the 1% level. The association with employment beliefs is weaker: a one-standard-deviation higher perceived full-time employment rate among mothers who breastfed, that is, a smaller perceived career cost, is associated with a 1.7 percentage-point higher probability of being very likely to breastfeed, which is not statistically significant, and 0.21 additional months of intended duration, significant at the 10% level. Overall, more favorable beliefs about breastfeeding are associated with stronger breastfeeding intentions, particularly in the health and

cognitive domains. These associations need not be causal, since unobserved preferences may shape both beliefs and intentions; the information treatment allows us to address this.

Table 2. Prior Beliefs and Breastfeeding Intentions

	Diarrhoea (1)	IQ (2)	Ovarian cancer (3)	Full-time employment (4)
Panel A: Very likely to breastfeed				
Prior belief (standardized)	-0.050***	0.076***	-0.063***	0.017
	(0.014)	(0.013)	(0.013)	(0.013)
Individual controls	Yes	Yes	Yes	Yes
Observations	1337	1337	1337	1337
Panel B: Intended breastfeeding duration (months)				
Prior belief (standardized)	-0.534***	0.650***	-0.656***	0.214*
	(0.131)	(0.123)	(0.123)	(0.124)
Individual controls	Yes	Yes	Yes	Yes
Observations	1337	1337	1337	1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. Each column within each panel reports a separate OLS regression of the baseline intention outcome on the prior belief indicated by the column heading. Prior beliefs are standardized to mean zero and standard deviation one in the full survey sample. Higher values denote higher perceived incidence, IQ, or employment, without reversing signs. The dependent variable is an indicator for being very likely to breastfeed in Panel A and constructed intended duration in months, including zeros, in Panel B. Panel A coefficients are in probability units. All specifications control for age, age squared, race, marital status, education, employment status, and income.

2.4 Information Treatment

After eliciting participants' prior beliefs we randomly assign them to either the treatment group or the control group. Table A.1 reports the treatment-control difference in each baseline characteristic together with the p-value of the corresponding two-sample t-test. Twelve of the thirteen differences are statistically indistinguishable from zero at conventional levels; the only exception is the share in the lowest income band, which is 4 percentage points lower in the treatment arm and marginally significant ($p = 0.09$), about what we would expect by chance when testing thirteen characteristics. The last row of the table reports an omnibus test of random assignment: regressing treatment status on all characteristics jointly, we cannot reject the null that the coefficients are jointly zero ($F(12,1275) = 1.11$, $p = 0.35$). The demographic characteristics are therefore broadly balanced. Appendix Table A.3 additionally reports standardized outcome differences. Prior full-time employment beliefs are 0.13 SD higher in the treatment group (28.21% versus 26.20%, $p = 0.021$), while the other reported prior outcomes show no statistically significant differences. Our ANCOVA specifications control for the corresponding pre-treatment outcome. The control group receives no information and proceeds directly to the posterior belief elicitation. The treatment group receives evidence summaries, presented across four separate pages, on each of the following outcomes: children's gastroenteritis, children's IQ, ovarian cancer, and maternal employment.

To avoid order effects, we randomize the sequence in which the four information pages are shown.[7] The content of each information treatment is as follows. The child-health, IQ, and employment pages also include graphs illustrating the comparisons described below. Appendix C.1 reproduces the information pages, including their exact wording and graphs.

Children's health (gastroenteritis). Participants are informed that, in the Belarus PROBIT trial (Kramer et al., 2001), 20% of mothers assigned to receive breastfeeding support were still breastfeeding at twelve months, compared with 11% in the control group. During the first year, 9% of infants in the intervention group experienced gastroenteritis, compared with 13% in the control group.

Children's cognitive skills (IQ). Participants are informed that a follow-up of the same PROBIT trial (Kramer et al., 2008) found that, at age 6.5, children in the breastfeeding-support intervention group had an average IQ of 109.5, compared with 101.9 in the control group. These are the values presented in the survey.

Maternal health (ovarian cancer). Participants are informed that a large UK study of approximately 800,000 women (Gaitskell et al., 2018) found about 48 cases of ovarian cancer per 100,000 women per year among women who had breastfed, compared with 50 cases among those who had never breastfed.

Maternal employment. Using data from the Understanding Society survey,[8] we calculate that among mothers who were working full-time before the birth of their first child and chose to breastfeed for more than 6 months,[9] 21% are working full-time 3 to 7 years after the birth. Among women with similar income and education levels who chose not to breastfeed, 30% are working full-time. This means that mothers who chose to breastfeed have a full-time employment rate that is 9 percentage points lower than similar mothers who chose not to breastfeed.

The correspondence between the information and the elicited beliefs varies across domains. For ovarian cancer and employment, the information directly supplies the comparison requested in the question. For diarrhoea and IQ, respondents must translate evidence

[7] Because the information on gastroenteritis and IQ are drawn from the same study (the PROBIT trial), they are always shown consecutively. We first randomize the order of three blocks: [(gastroenteritis, IQ), ovarian cancer, employment], and then within the (gastroenteritis, IQ) block, we randomize the order of gastroenteritis and IQ.

[8] University of Essex, Institute for Social and Economic Research. (2024). Understanding Society: Pregnancy and Early Childhood (PEACH), 2009–2022. 3rd Edition. UK Data Service. SN: 9075, DOI: `http://doi.org/10.5255/UKDA-SN-9075-3`. We link PEACH to the main Understanding Society sample of women aged 25 or older with at least one child. PEACH is a single cross-wave file combining parent-reported data, including breastfeeding history, for all children under age 10. We keep mothers working full time (35 hours or more per week) in at least one of the three years before the birth, and compare full-time rates three to seven years after the birth between mothers who breastfed for seven months or longer and those who did not. To compare mothers with similar income and education, we reweight the latter group to match the former on these characteristics, using odds weights from a probit of breastfeeding for seven months or longer on terciles of monthly labor earnings and educational attainment.

[9] The information page uses "more than 6 months," whereas the belief question uses "7 months or longer." We preserve these wordings when describing the survey.

on gastrointestinal infection and IQ from a breastfeeding-promotion trial into the UK breastfeeding scenario described in the elicitation.

2.5 Posterior Beliefs Elicitation

After the information treatment (or no information for the control group), we re-elicit beliefs on the same four domains using the same wording as in the prior-elicitation stage. To minimize anchoring effects and test for spillovers across domains, we additionally ask four new questions on the same broad domains but using different outcome measures, for a total of eight belief questions. The additional questions are as follows:

Children's health. "The UK study examining infants we described before shows that 16% of them who have not been breastfed experience one or more episodes of vomiting (a common symptom of gastroenteritis). What do you think the percentage would be if these children were breastfed for 4 months or longer?" [slider 0-100, pre-set at 16]

Children's cognitive skills. "According to a UK study examining about 5,000 children, 44% of children who were never breastfed passed 5 or more GCSEs[10] with a mark of 5 or higher. What is this percentage for these children if they were breastfed for 6 months?" [slider 0-100, pre-set at 44]

Maternal health. "Researchers have conducted numerous studies to assess the link between breastfeeding and breast cancer. A comprehensive review examined 98 studies from various countries. Out of these 98 studies, how many indicate that women who have ever breastfed have a lower risk of breast cancer?" [slider 0-98]

Maternal labor-market outcomes. "Using data from a UK survey, we estimate that the average monthly gross (before tax) labour earnings, between 3 and 7 years after the birth of their child, are around £1,080 for mothers who decided not to breastfeed their child. What do you think the labour earnings would be for these mothers if they had breastfed for 7 months or longer?" [text box]

Participants are not shown the corresponding numerical findings for these additional outcomes, which we derive from studies (vomiting, GCSE attainment, breast cancer) and our own calculation based on Understanding Society Survey data (maternal earnings). Fisk et al. (2011) report that 8.0% of infants breastfed for at least four months experienced one or more bouts of vomiting lasting at least two days during their first six months, compared with 16.3% of infants who were never breastfed. For educational attainment, Pereyra-Elías et al. (2023) report that 69.5% of children breastfed for six to less than twelve months passed at least five GCSEs at grade 5 or above, including English and Mathematics, compared with

[10] Passing five GCSEs at grade 5 or above is a metric commonly used as key benchmark for academic achievement at the end of secondary school in England, Northern Ireland, and Wales. This metric is roughly equivalent to achieving five National 5s at grades A to C in the education system in Scotland.

43.8% of children who were never breastfed.[11] In the review by Chowdhury et al. (2015), 89 of the 98 estimates comparing ever with never breastfeeding have point estimates below one, indicating a negative association with breast-cancer risk, not necessarily a statistically significant one. Finally, the earnings calibration used in our survey is £880 per month for mothers breastfeeding for seven months or longer, compared with the £1,080 figure supplied for mothers who did not breastfeed, based on our Understanding Society calculations for three to seven years after childbirth. Appendix Table A.4 reports the distribution of the elicited posterior beliefs for these four outcomes, alongside the four repeated domains and the intention outcomes.

3 Empirical Approach

Let y index the belief outcome. For outcomes elicited both before and after treatment, y denotes infant diarrhoea, child IQ, ovarian cancer, or maternal full-time employment; for the additional posterior outcomes, it denotes vomiting, GCSE attainment, breast cancer, or maternal earnings. We estimate the specifications below separately for each relevant outcome.

Main specifications. Following Wiswall and Zafar (2015) and Haaland and Roth (2023), we estimate the information treatment effects using two specifications, depending on whether the outcome is measured both before and after the information treatment or only after. All belief outcomes are standardized to have mean zero and standard deviation one in the full analysis sample.

For outcomes measured both pre- and post-treatment, we estimate an ANCOVA specification:

$$z^{y}_{i,\text{post}} = \alpha_0 + \alpha_1 D_i + \alpha_2 z^{y}_{i,\text{pre}} + \alpha_3' X_i + \varepsilon_i, \tag{1}$$

where $z^{y}_{i,\text{post}}$ is the standardized posterior belief for individual i, D_i is a binary indicator for treatment assignment, $z^{y}_{i,\text{pre}}$ is the corresponding standardized prior belief, and X_i is a vector of pre-determined individual controls including age, age squared, race, marital status, education, employment status, and income. The coefficient α_1 captures the average causal effect of information provision on posterior beliefs, expressed in standard deviation units. We prefer this ANCOVA specification to a first-difference model because, absent a perfect pre-post correlation, ANCOVA yields a more efficient estimate of the treatment effect (McKenzie, 2012). Estimating α_2 rather than imposing a unit coefficient on the prior also accounts for mean reversion: because elicited beliefs contain measurement error, extreme priors tend to be followed by less extreme posteriors even without information.[12]

[11] The corresponding proportion for children breastfed for at least twelve months is 71.0%; this longer-duration category differs from the six-month duration specified in our question.

[12] The pre-registration specifies a first-difference model. Appendix B lists this and other departures from the pre-registration.

For outcomes measured only post treatment (the four additional questions designed to minimize anchoring), we estimate a level specification:

$$z_i^y = \beta_0 + \beta_1 D_i + \beta_2' X_i + u_i, \tag{2}$$

where the outcome is the standardized posterior belief on a domain not elicited prior to treatment.

Accuracy. For three of the four belief domains, the underlying empirical evidence reports outcomes for breastfeeding and non-breastfeeding groups corresponding to the comparisons in our questions. For diarrhoea, Table 3 of Fisk et al. (2011) reports an incidence of 10.1% among infants breastfed for at least four months, compared with 27.3% among those never breastfed. We use the rounded value of 10% as the reference. For ovarian cancer and maternal full-time employment, the corresponding reference values are 48 cases per 100,000 women per year and 50, respectively, as described in Section 2.4. These are empirical outcome comparisons, not necessarily causal effects of breastfeeding.

For IQ, the evidence provided to participants instead compares children assigned to a breastfeeding-promotion intervention with those in the control group. Both groups include breastfed children. A simple Wald adjustment does not yield a credible estimate for the breastfeeding comparison in our question, since the intervention changes both duration and exclusivity rather than a single binary breastfeeding exposure. We therefore retain 109.5, the intervention-group mean communicated to participants, as an informational benchmark rather than an estimate of the precise counterfactual IQ elicited by the question.

We use the term accuracy in the operational sense of proximity to these four reference values. To assess whether information moves beliefs closer to the respective reference, we estimate separately for each domain:

$$|\text{Belief}_{i,\text{post}}^y - \text{Ref}^y| = \gamma_0 + \gamma_1 D_i + \gamma_2 |\text{Belief}_{i,\text{pre}}^y - \text{Ref}^y| + \gamma_3' X_i + \varepsilon_i, \tag{3}$$

where $\text{Belief}_{i,\text{post}}^y$ and $\text{Belief}_{i,\text{pre}}^y$ are posterior and prior beliefs in the original units of outcome y, and Ref^y denotes its reference value. The absolute differences measure posterior and prior distances from that reference. A negative γ_1 indicates that the treatment reduces this distance. For IQ in particular, this measures convergence toward the communicated benchmark, rather than a reduction in error relative to a known counterfactual outcome.

Heterogeneity by prior underestimation. We investigate whether the effect of information on belief updating varies with prior underestimation in the corresponding outcome. For IQ, we use the term underestimation loosely to denote a prior below the informational benchmark of 109.5, rather than a demonstrated underestimate of the outcome under the breastfeeding scenario in our question. Following Haaland and Roth (2023), we estimate:

$$z_{i,\text{update}}^y = \delta_0 + \delta_1 D_i + \delta_2 D_i \times \text{Underest}_i^y + \delta_3 \text{Underest}_i^y + \delta_4' X_i + \varepsilon_i, \tag{4}$$

where Underest_i^y is the standardized prior gap for outcome y. Before standardization, this gap is $\text{Belief}_{i,\text{pre}}^y - \text{Ref}^y$ for diarrhoea and ovarian cancer and $\text{Ref}^y - \text{Belief}_{i,\text{pre}}^y$ for IQ and employment, so higher values indicate less favorable beliefs about breastfeeding relative to the corresponding reference. For employment, higher values indicate a lower prior full-time employment rate among breastfeeding mothers, that is, a larger perceived employment penalty. Each gap is then centered and scaled to have mean zero and standard deviation one; positive standardized values therefore indicate greater underestimation relative to the sample average, not necessarily a belief on a particular side of the reference. The dependent variable $z_{i,\text{update}}^y$ is the standardized change in beliefs, oriented so that positive values indicate a revision toward more favorable outcomes: posterior minus prior for IQ and employment, and prior minus posterior for diarrhoea and ovarian cancer. The interaction coefficient δ_2 captures how the treatment effect on this update varies with a one-standard-deviation increase in the outcome-specific prior gap.

Because Ref^y is constant across respondents, it cancels when the prior gap is mean-centered. Thus, Underest_i^y captures relatively less favorable prior beliefs within the sample, rather than whether a belief lies above or below an absolute benchmark. Holding the sample and sign convention fixed, changing a reference value, including the IQ benchmark of 109.5, leaves the standardized domain-specific regressors unchanged, and therefore does not alter the associated heterogeneity estimates. Consequently, this heterogeneity analysis does not require knowledge of the exact counterfactual outcome under breastfeeding: it identifies whether the effect of information varies with respondents' relative prior beliefs, independently of the numerical benchmark chosen. This invariance does not apply to the absolute-distance accuracy measure in equation (3), which depends on the reference value.

Treatment effects on intentions. We apply the ANCOVA approach in equation (1) to three intention outcomes: being very likely to breastfeed, intended breastfeeding duration in months, and planned full-time work, controlling for the corresponding pre-treatment responses and individual characteristics. We also examine whether information induces upward or downward revisions. Because experience with motherhood may shape responses to information, we estimate effects separately for women with and without children and test for differences using treatment interactions. Outcomes elicited only after treatment are analyzed using equation (2), without a control for a pre-treatment response to the same question. Finally, we examine heterogeneity by the four domain-specific prior underestimation measures, entered jointly with their treatment interactions, and by other baseline characteristics.

4 Results

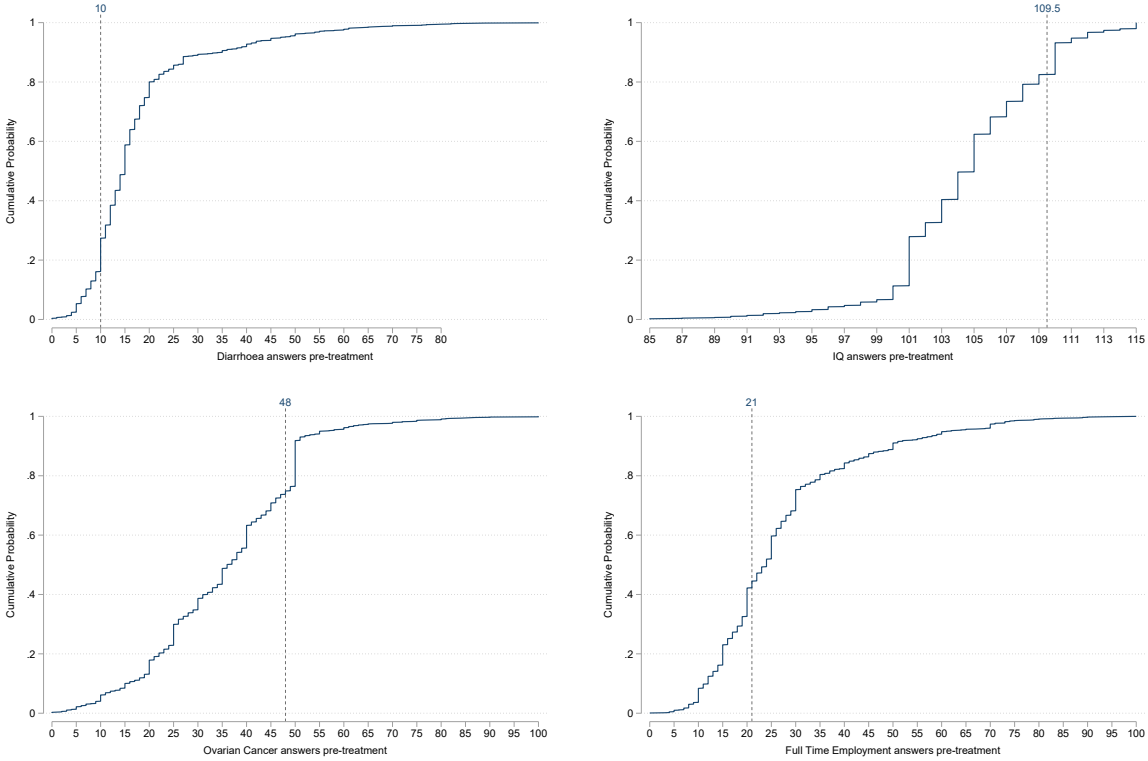
4.1 Baseline Misperceptions

Figure 1 presents the cumulative distributions of prior beliefs across the four outcome domains, with the reference values used in the analysis marked by vertical lines. Beliefs tend

to lie above these values for diarrhoea and maternal employment and below them for IQ and ovarian cancer.

Relative to these benchmarks, respondents tend to underestimate breastfeeding's benefits for children's health and cognitive development, overestimate its protective association with maternal ovarian cancer, and underestimate its associated maternal employment penalty. The interpretation for IQ remains qualified because the benchmark reflects assignment to a breastfeeding-promotion intervention rather than the breastfeeding comparison elicited in our survey. If the latter implies a higher counterfactual IQ, our benchmark understates the extent to which respondents underestimate cognitive benefits.

Figure 1. Cumulative distribution of prior beliefs and reference values



Notes: Each panel plots the empirical cumulative distribution function (CDF) of prior beliefs for one domain. The vertical line marks the reference value used in the analysis (10 for diarrhoea, 109.5 for IQ, 48 for ovarian cancer, and 21 for full-time employment).

4.2 Treatment Effects on Beliefs

Table 3 reports estimates of equation (1) for the four belief outcomes elicited both before and after the information treatment. All outcomes are standardized, so coefficients are in standard deviation units. The estimates reveal strong and precisely estimated treatment effects for three domains. Treated respondents update their posterior IQ belief by 0.595 standard deviations relative to controls, indicating substantial learning about breastfeeding's cognitive benefits. For ovarian cancer beliefs, the treatment effect is 0.567 standard deviations, a large shift toward the evidence. For maternal full-time employment, treated respondents

revise their beliefs downward by 0.237 standard deviations, consistent with learning about the labor-market costs of extended breastfeeding. By contrast, the diarrhoea coefficient is close to zero and statistically insignificant. The weaker response may partly reflect the less direct correspondence between the child-health information and the elicited breastfeeding comparison. Overall, information increases respondents' perceived cognitive benefits of breastfeeding, while reducing its perceived protection against ovarian cancer and lowering expectations of maternal full-time employment.

Experience with motherhood may shape how women respond to this information. Appendix Table A.5 therefore repeats Table 3 separately for women with and without children. The qualitative pattern is the same in both groups. Mothers show a larger upward revision in ovarian-cancer incidence beliefs (0.842 SD, compared with 0.376 SD among women without children), implying a larger reduction in breastfeeding's perceived protective effect.

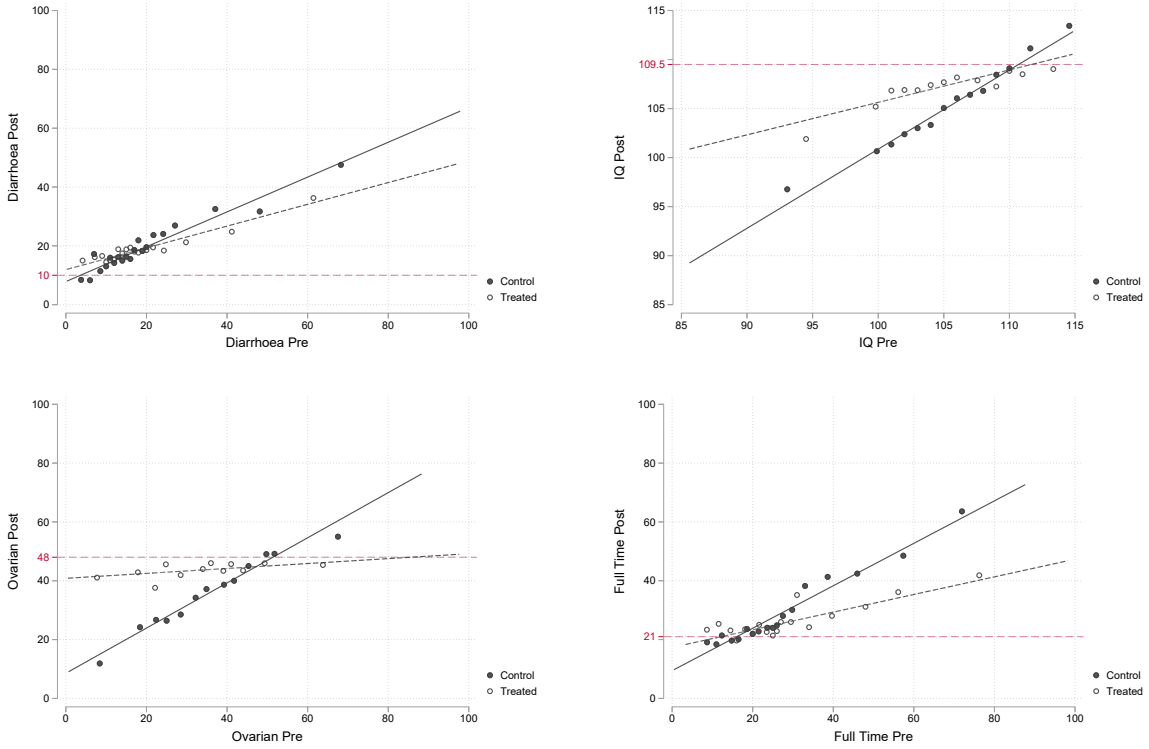
Table 3. Treatment effects on beliefs (ANCOVA)

	Diarrhoea (1)	IQ (2)	Ovarian cancer (3)	Full-time employment (4)
Treatment effect	0.015	0.595***	0.567***	-0.237***
	(0.046)	(0.041)	(0.046)	(0.046)
Prior belief control	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Prior mean	17.809	104.563	35.718	27.206
Prior SD	13.228	4.592	15.331	15.932
Observations	1337	1337	1337	1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. OLS estimates of equation (1). Prior and posterior beliefs are standardized separately in the full analysis sample. The treatment indicator equals one for respondents assigned to the information treatment. Each specification controls for the corresponding prior belief and individual characteristics (age, age squared, race, marital status, education, employment status, and income). Prior means and standard deviations are reported in the original outcome units.

Figure 2 complements the regression table by showing how posterior beliefs vary with baseline beliefs across the full support of priors. In each panel, the horizontal axis is the pre-treatment belief and the vertical axis is the posterior belief. The solid line traces the fitted relationship for the control group and the dashed line for the treated group. The vertical separation between the lines visualizes the treatment effect conditional on baseline beliefs. The horizontal line marks the reference value used in our analysis. For diarrhoea, this is the UK breastfeeding comparison, not the gastrointestinal-infection rates shown in the treatment; for IQ, it is the promotion-trial benchmark rather than the precise breastfeeding counterfactual. The information treatment weakens the relationship between prior and posterior beliefs for most outcomes: the regression line is flatter for the treatment group than for the control group, and for IQ, ovarian cancer, and full-time employment it is also visibly pulled toward the benchmark across the support of priors. Diarrhoea is again the exception: the treated line remains close to the control line and far above the benchmark.

Figure 2. Belief updating: outcomes elicited pre and post treatment



Notes: Panels show binned scatter plots of posterior beliefs against prior beliefs for diarrhoea, IQ, ovarian cancer, and full-time employment. Solid lines are fitted values for the control group and dashed lines are fitted values for the treatment group. The horizontal line marks the analytical reference value (10 for diarrhoea, 109.5 for IQ, 48 for ovarian cancer, and 21 for full-time employment). The diarrhoea reference was not supplied in the information treatment, and the IQ reference is the promotion-trial intervention-group mean; see Section 3.

4.3 Does Information Improve Accuracy?

A central question is whether the information treatment not only shifts posterior beliefs but also brings them closer to the evidence. Table 4 reports estimates of equation (3), where the outcome is the absolute distance between posterior beliefs and the reference value. For diarrhoea, ovarian cancer, and employment, the reference values provide reasonable empirical benchmarks for the outcomes elicited in our questions. For IQ, the promotion-trial benchmark plausibly understates the outcome under the breastfeeding scenario we ask about. To the extent that this is the case, upward revisions from below the benchmark move beliefs toward the underlying outcome as well as toward the benchmark. A decline in our distance measure therefore indicates improved accuracy, although for IQ it need not capture the full remaining error.

Treatment substantially reduces distance from the benchmarks for IQ, ovarian cancer, and full-time employment. For child IQ, the reduction is 2.93 points, approximately 53% of the pre-treatment mean distance of 5.51 points; this percentage refers to benchmark distance, not necessarily to the underlying belief error. For ovarian cancer, the reduction is 9.14 cases per 100,000 – about 59% of the pre-treatment mean distance of 15.45. For full-time employment,

the distance falls by 5.90 percentage points, approximately 53% of its pre-treatment mean. The effect on diarrhoea is small and statistically insignificant, consistent with near-zero belief updating for this domain.

Table 4. Treatment effects on belief accuracy

	Diarrhoea (1)	IQ (2)	Ovarian cancer (3)	Full-time employment (4)
Treatment Effect	-0.124	-2.930***	-9.139***	-5.899***
	(0.544)	(0.159)	(0.513)	(0.572)
Controls	Yes	Yes	Yes	Yes
Pre-mean	8.965	5.513	15.446	11.236
N	1337	1337	1337	1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. OLS estimates of equation (3). The dependent variable is the absolute difference between posterior beliefs and the reference value. A negative coefficient indicates reduced distance from the benchmark. Specifications control for the corresponding prior distance and individual characteristics (age, age squared, race, marital status, education, employment status, and income).

Heterogeneity by prior underestimation. Table 5 examines how treatment effects vary with prior beliefs, using equation (4) and domain-specific standardized underestimation measures. Unlike Table 3, belief updates are oriented so that positive values indicate greater perceived benefits of breastfeeding: the signs are therefore reversed for diarrhoea and ovarian cancer. As discussed in Section 3, centering and standardizing the prior gaps removes the constant reference values. These estimates therefore capture heterogeneity by relatively less favorable prior beliefs and do not depend on our assumptions about the numerical benchmarks. The treatment interaction is positive and statistically significant in all four domains. Respondents with less favorable priors thus respond more positively to information about breastfeeding. Belief updates are standardized to have mean zero and standard deviation one in the full analysis sample. In three of the four domains, a one-standard-deviation increase in prior underestimation raises the treatment effect on updating toward greater perceived benefits by roughly half to two-thirds of a standard deviation: 0.538 for IQ, 0.697 for ovarian cancer, and 0.447 for employment. The corresponding increase for diarrhoea is smaller, at 0.229 standard deviations.

At zero standardized underestimation, the treatment coefficient describes the response at the sample-average prior, not at the reference value. At this point, information increases perceived cognitive benefits but reduces perceived ovarian-cancer protection and expected maternal employment, and it has no effect on diarrhoea beliefs. The negative ovarian-cancer and employment coefficients reflect the position of the average prior relative to the evidence: the average prior incidence of ovarian cancer (35.7 cases) lies below the benchmark of 48, and the average prior full-time employment rate (27.2%) lies above the benchmark of 21%, so moving toward the evidence means perceiving less protection and a larger career cost.

Column 5 summarizes these patterns with two indices: the average of the four standardized belief updates and the average of the four standardized prior gaps, each re-standardized to have mean zero and standard deviation one. At the average prior, information does not change the overall favorability of beliefs about breastfeeding (−0.064, not statistically significant). Yet a one-standard-deviation increase in overall underestimation raises the treatment effect on the index by 0.590 standard deviations. The information thus rebalances perceived benefits and costs rather than producing a uniformly more favorable view of breastfeeding, and it shifts beliefs in a favorable direction for women who start out most pessimistic.

Table 5. Heterogeneity by prior underestimation

	Diarr. (1)	IQ (2)	Ov. Ca (3)	FT Emp (4)	Index (5)
Treat. Effect	-0.014	0.640***	-0.523***	-0.235***	-0.064
	(0.045)	(0.042)	(0.037)	(0.043)	(0.042)
Treat. Effect × Underest.	0.229**	0.538***	0.697***	0.447***	0.590***
	(0.100)	(0.070)	(0.057)	(0.066)	(0.059)
Controls	Yes	Yes	Yes	Yes	Yes
Mean underest.	7.809	4.937	-12.282	-6.206	-0.000
SD	13.228	4.592	15.331	15.932	1.000
N	1337	1337	1337	1337	1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. OLS estimates of equation (4), using domain-specific standardized prior gaps and standardized belief updates. Positive updates indicate greater perceived breastfeeding benefits: prior minus posterior for diarrhoea and ovarian cancer, and posterior minus prior for IQ and employment. Higher prior gaps indicate relatively less favorable beliefs; zero standardized underestimation denotes the sample-average prior gap. The reported prior-gap means and SDs are in original units. All specifications include individual controls.

Generalization to related outcomes. Table 6 reports estimates of equation (2) for four outcomes elicited only after the information treatment: vomiting, GCSE performance, breast-cancer evidence, and labor earnings. Because the information treatment did not directly address these outcomes, they test whether respondents generalize what they learned to related outcomes within the same conceptual domain.

The estimates reveal within-domain spillovers for cognition and maternal health. For GCSE performance, the treatment increases posterior beliefs by 0.250 standard deviations, significant at the 1% level, indicating that respondents extend the IQ evidence to a related educational outcome. For breast-cancer evidence beliefs, the treatment effect is 0.257 standard deviations, also significant at the 1% level. Unlike the reduction in perceived ovarian-cancer protection, information increases the perceived evidence that breastfeeding protects against breast cancer. This contrast is consistent with greater scope for favorable updating on breast cancer: control respondents report about 46 of 98 studies finding a protective association, substantially below the reference of 89. The effects on vomiting and labor earnings are small

and statistically insignificant, though the earnings effect is directionally consistent with the employment result.

Table 6. Treatment effects: post-only outcomes

	Vomit	GCSE	Positive effect on breast cancer	Earnings
	(1)	(2)	(3)	(4)
Treatment Effect	0.012	0.250***	0.257***	-0.086
	(0.055)	(0.054)	(0.054)	(0.054)
Controls	Yes	Yes	Yes	Yes
Control mean	12.755	54.685	46.277	1108.000
Control SD	10.991	12.698	29.708	1425.303
N	1337	1337	1337	1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. OLS estimates of equation (2) for outcomes elicited only after the information treatment (vomiting, GCSE performance, breast-cancer evidence beliefs, and labor earnings). Outcomes are standardized. The treatment indicator equals one for respondents assigned to the information treatment. All specifications include individual controls. Because these outcomes are elicited only after the treatment, no pre-treatment mean exists for them; the table therefore reports the mean and standard deviation of the control group, in the original units of each outcome, which give the level the outcome takes absent the information.

Table 7 examines whether these spillovers also bring beliefs closer to the reference values described in Section 2.5. Information reduces absolute benchmark distance by 3.30 percentage points for GCSE attainment, 7.47 studies for breast-cancer evidence, and £128.31 for monthly earnings. The effect for vomiting is small and statistically insignificant. Thus, even though the average earnings effect in Table 6 is statistically insignificant, distance from the earnings benchmark declines.

Table 7. Treatment effects on accuracy: post-only outcomes

	Vomit	GCSE	Positive effect on breast cancer	Earnings
	(1)	(2)	(3)	(4)
Treatment effect	-0.411	-3.298***	-7.468***	-128.310**
	(0.548)	(0.518)	(1.492)	(56.271)
Individual controls	Yes	Yes	Yes	Yes
Control mean distance	6.091	16.896	43.913	367.711
Observations	1337	1337	1337	1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. OLS regressions of absolute posterior distance from the reference value on treatment and individual controls. References are 8% for vomiting, 69.5% for GCSE attainment (six to less than twelve months of breastfeeding), 89 of 98 studies for evidence of lower breast-cancer risk, and £880 for monthly earnings. The earnings reference is the calibration used in the survey. Outcomes and control-group mean distances are in original units. A negative coefficient indicates improved accuracy relative to the benchmark. No corresponding prior outcome was elicited.

4.4 Treatment Effects on Breastfeeding Intentions

Table 8 applies the ANCOVA approach in equation (1) to breastfeeding intentions and labor-market plans: being very likely to breastfeed, the top category of the five-point likelihood scale (Very Likely); intended breastfeeding duration in months, constructed by mapping the duration categories to mean durations in Understanding Society (Duration); and planning to work full-time after birth (Full Time). Each specification controls for the corresponding pre-treatment response and individual characteristics. Panel A reports average effects, Panel B examines upward and downward revisions, and Panel C estimates effects separately by motherhood status. In Panel B, the outcomes are indicators for revising the five-category likelihood response (Intention), the duration category, or full-time work plans upward (+) or downward (−) relative to the pre-treatment response. Probability effects are reported on the 0–1 scale, and duration is measured in months in Panels A and C.

The main finding, in Panel A, is that the information treatment significantly increases intended breastfeeding duration. Treated respondents intend to breastfeed approximately 0.67 months longer than controls, about 8% of the post-treatment sample mean of 8.35 months. By contrast, the treatment has no statistically significant effect on the probability of being very likely to breastfeed. The treatment also reduces the probability of planning to work full-time after birth by 1.6 percentage points, consistent with respondents internalizing information about labor-market costs.

Panel B shows that information increases the probability of an upward revision in breastfeeding intentions by 2.8 percentage points and the probability of increasing intended duration by 10.7 percentage points, both significant at the 1% level. Neither outcome shows a significant effect on downward revisions. For full-time work plans, the adjustment runs in the opposite direction: information raises the probability of revising from full-time to non-full-time work by 1.6 percentage points.

While belief updating is broadly similar across motherhood status, the intention responses differ markedly. Women without children increase intended duration by 1.02 months, compared with 0.30 months among mothers, and reduce planned full-time employment by 2.5 percentage points, compared with an insignificant 0.8-point reduction among mothers. This pattern is consistent with information having greater scope to shape duration and work plans before women become mothers. We cannot rule out that part of the stronger duration response among women without children is mechanical: 58.8% of mothers, compared with 34.8% of women without children, already select the highest duration category (seven months or more) before treatment, leaving mothers less scope for recorded upward revisions in duration. However, the ceiling does not fully account for the difference: among women below the highest category at baseline, estimated duration effects remain larger for non-mothers (1.58 months) than for mothers (0.80 months), with the difference significant at the 5% level ($p = 0.032$). The extensive margin is more nuanced: mothers become 3.4 percentage points more likely to report being very likely to breastfeed, whereas the effect among women without children is negative and statistically insignificant. Thus, women

Table 8. Treatment effects on breastfeeding intentions (ANCOVA)

Panel A: Average Effects						
	Very Likely		Duration (months)		Full Time	
Treatment	0.007		0.667***		-0.016**	
	(0.013)		(0.107)		(0.007)	
Mean outcome	0.565		8.346		0.473	
N	1337		1337		1337	
Panel B: Directional Changes						
	Intention		Duration revision		Full Time	
	+	−	+	−	+	−
Treatment	0.028***	0.012	0.107***	-0.002	-0.000	0.016***
	(0.009)	(0.013)	(0.014)	(0.007)	(0.003)	(0.006)
N	1337	1337	1337	1337	1337	1337
Panel C: Results by Motherhood Status						
	Very Likely		Duration (months)		Full Time	
	No child	Has child	No child	Has child	No child	Has child
Treatment	-0.017	0.034**	1.020***	0.297***	-0.025**	-0.008
	(0.021)	(0.016)	(0.188)	(0.109)	(0.012)	(0.007)
Mean outcome	0.492	0.646	7.669	9.105	0.550	0.379
N	666	622	666	622	666	622

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. Panels A and C report OLS ANCOVA estimates controlling for the corresponding baseline intention and individual characteristics. Baseline breastfeeding likelihood and work plans enter as categorical controls; baseline duration enters in months. Very Likely denotes being very likely to breastfeed and Full Time denotes planning full-time work after birth. Their coefficients, standard errors, and outcome means are on the 0–1 scale. Duration in Panels A and C is in months. Panel B reports effects on the 0–1 scale on the probability of an upward (+) or downward (−) revision, with the same baseline controls; Intention refers to the five-category likelihood response. Full Time revisions indicate entering (+) or leaving (−) planned full-time work; changes between part-time work and non-employment are not counted. Panel C splits the sample into women without children ($N = 666$) and mothers ($N = 622$); its combined sample of 1,288 excludes 49 respondents with missing motherhood status. Mean outcome refers to the post-treatment outcome in the respective sample. All specifications include age, age squared, race, marital status, education, employment status, and income.

without children respond more in duration and work plans, but mothers respond more on the very-likely margin.

4.5 Prior Beliefs and Intention Responses

Table 9 examines whether the effects of information depend on respondents' prior beliefs. We include all four standardized prior underestimation measures and their interactions with treatment jointly, alongside the baseline intention and individual controls. Because these beliefs are measured before random assignment, the interactions identify heterogeneity in treatment effects by prior beliefs. They do not identify causal mediation through subsequent belief changes.

Cognitive priors are particularly relevant for intended duration: a one-standard-deviation increase in IQ underestimation raises the treatment effect by 0.280 months, over 40% of the

0.673-month effect at the mean priors. Employment priors matter for both duration and the direction of intention revisions. Women who initially expect lower employment under breastfeeding, and therefore anticipate a larger employment penalty, show a 0.278-month larger duration effect per standard deviation of underestimation. They also show a 3.8-percentage-point lower treatment effect on negative intention revisions, while the interaction for positive revisions is small and statistically insignificant. Thus, the likelihood response is associated with fewer downward revisions rather than more upward revisions. Neither diarrhoea nor ovarian-cancer priors show statistically significant interactions.

For full-time work plans, the employment-prior interaction is positive but imprecisely estimated in the full sample: 0.9 percentage points, compared with a treatment coefficient of −1.6 percentage points at the mean priors. Column (5) focuses on women initially planning full-time work, who have scope to revise their plans downward. The interaction rises to 2.8 percentage points and is significant at the 10% level, against a treatment coefficient of −2.9 percentage points. Women who already anticipate a larger employment penalty therefore reduce their work plans less in response to information. Taken together, these results point to cognitive and employment priors as relevant predictors of intention responses, without requiring us to condition on reported belief changes after treatment.

Table 9. Treatment effects on intentions by prior underestimation

	Positive revision	Negative revision	Duration (months)	Full Time	FT conditional on Prior FT = 1
Treatment	0.027***	0.008	0.673***	-0.016***	-0.029**
	(0.009)	(0.013)	(0.106)	(0.006)	(0.011)
Treatment × diarrhoea underest.	0.006	-0.005	-0.154	-0.004	-0.009
	(0.008)	(0.016)	(0.129)	(0.007)	(0.012)
Treatment × IQ underest.	0.011	0.010	0.280**	0.001	-0.009
	(0.008)	(0.018)	(0.126)	(0.009)	(0.012)
Treatment × ovarian cancer underest.	-0.006	-0.006	0.016	-0.002	0.009
	(0.010)	(0.016)	(0.118)	(0.007)	(0.012)
Treatment × employment underest.	0.006	-0.038**	0.278**	0.009	0.028*
	(0.010)	(0.017)	(0.118)	(0.011)	(0.016)
Observations	1337	1337	1337	1337	643

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. Each column reports OLS ANCOVA estimates including all four standardized prior underestimation measures, their treatment interactions, the corresponding baseline intention, and individual controls. Column (5) restricts the sample to women initially planning full-time work. Underestimation measures retain their full-sample standardization; higher values denote relatively less favorable prior beliefs. The treatment coefficient is the effect when all four measures equal zero, and each interaction measures how that effect changes with a one-SD increase in the corresponding measure, holding the others fixed. Positive and negative revisions indicate an increase or decrease on the five-category breastfeeding-likelihood scale. Revision probabilities and FT are on the 0–1 scale; duration is in months. Baseline likelihood and work plans enter as categorical controls, and baseline duration enters in months. Individual controls are as in Table 8. Underestimation main effects are included but not displayed.

4.6 Heterogeneity

Table 10 examines heterogeneity by motherhood, current full-time work status, confidence in prior beliefs (Sure), and higher education. For each outcome, the first specification interacts treatment with motherhood alone, and the second adds the remaining interactions jointly. Mothers show a smaller duration response and a lower treatment effect on negative intention revisions; both differences persist in the joint specification. Their response in positive revisions is not significantly different from that of women without children. None of the additional interactions is statistically significant at conventional levels. Motherhood therefore emerges as the most consistent source of heterogeneity among the characteristics examined, with substantially weaker evidence for the other moderators.

Table 10. Heterogeneity in treatment effects on intentions

	Positive revision		Negative revision		Duration (months)	
	Motherhood	Joint	Motherhood	Joint	Motherhood	Joint
Treatment	0.031**	0.054*	0.056***	0.010	1.017***	1.108***
	(0.014)	(0.028)	(0.021)	(0.039)	(0.186)	(0.327)
Treatment × Has child	-0.014	-0.020	-0.085***	-0.069**	-0.723***	-0.700***
	(0.017)	(0.018)	(0.027)	(0.028)	(0.216)	(0.219)
Treatment × Full-time		-0.015		0.041		-0.017
		(0.020)		(0.027)		(0.223)
Treatment × Sure		-0.002		-0.001		-0.186
		(0.019)		(0.028)		(0.220)
Treatment × Higher edu.		-0.012		0.017		0.014
		(0.021)		(0.033)		(0.241)
Observations	1288	1288	1288	1288	1288	1288

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. Positive and negative revisions are binary indicators of an increase or decrease on the five-category breastfeeding-likelihood scale; duration is in months. Within each outcome pair, Motherhood includes a treatment interaction with motherhood status, and Joint additionally includes interactions with current full-time employment, confidence in prior beliefs (Sure: somewhat sure or higher), and higher education. All moderator main effects are included. Specifications control for baseline likelihood categories or baseline duration, as appropriate, and individual characteristics. The sample excludes 49 respondents with missing motherhood status. Each interaction is the differential treatment effect relative to the indicated group's complement.

We also examine heterogeneity across demographic groups in Appendix Table A.6. The results are broadly consistent across groups: the treatment increases intended duration in every subgroup and has no effect on the probability of being very likely to breastfeed. Point estimates for duration are larger for younger women (0.87 versus 0.47 months), but this subgroup difference is borderline significant and differences for the other subgroups are imprecisely estimated.

Attention checks. The results are also robust to restricting the sample to respondents who pass the initial numerical attention check or report paying close attention at the end of the

survey (Appendix Table A.7). Treatment effects on beliefs and intentions remain close to the full-sample estimates.

4.7 Obfuscated Follow-up

To assess whether the intention responses persist beyond the original survey setting and whether they reflect experimenter demand, we conduct an obfuscated follow-up one week later, following Haaland et al. (2023). If treated respondents reported stronger intentions only because they inferred what the researchers wanted to hear, the effects should weaken or disappear once respondents no longer link the questions to the original experiment. This test complements the mixed-direction design of the information, which already makes it harder for respondents to infer a desired answer. Breastfeeding questions are therefore embedded among questions about school-inspection ratings, tobacco policy, and saving for a future child, with framing designed to reduce the connection to the original experiment. Screenshots of the follow-up survey are provided in Appendix C.2. Of the 1,337 respondents in the analysis sample, 890 provide follow-up responses (66.6%). Response rates are similar across treatment arms: 67.0% in control and 66.2% in treatment.

The follow-up questionnaire included a separate branch for mothers, asking whether they were currently breastfeeding and how long they planned to continue, rather than repeating the questions about breastfeeding intentions for a future child. Unfortunately, this design choice means that their follow-up responses are not comparable to their baseline intentions, so we cannot use mothers to assess persistence. We therefore restrict this analysis to women without children, a particularly relevant population for information provision because they have yet to make their first breastfeeding decision.

Table 11 compares immediate and follow-up treatment effects among non-mothers. For each outcome, both estimates use the same respondents and control for the corresponding baseline outcome and individual characteristics. Positive and negative revisions compare responses with the pre-treatment likelihood of breastfeeding; duration uses the same category-to-month mapping as the main analysis.

Treatment raises the probability of a positive revision at follow-up by 5.3 percentage points and intended duration by 0.81 months, compared with 1.14 months immediately after treatment in the same sample. The follow-up effect on negative revisions is small and statistically insignificant. The duration effect at follow-up retains about 70% of its immediate size, and the effect on positive revisions is larger than immediately after treatment. That these effects persist in a setting less directly connected to the original experiment makes it less likely that experimenter demand is the main driver of the intention responses among women without children. We cannot, however, rule out demand effects entirely: some respondents may still have linked the follow-up to the original survey. Moreover, stated intentions need not translate into subsequent behavior.

Table 11. Treatment effects in the obfuscated follow-up: non-mothers

	Positive revision		Negative revision		Duration (months)	
	Main	Follow-up	Main	Follow-up	Main	Follow-up
Treatment	0.024	0.053**	0.064**	-0.029	1.135***	0.809***
	(0.017)	(0.026)	(0.028)	(0.036)	(0.228)	(0.254)
Observations	464	464	464	464	464	464

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. ANCOVA estimates for women without children. Main and Follow-up columns use identical samples within each outcome pair. Positive and negative revisions are indicators of an increase or decrease relative to the pre-treatment five-category likelihood response; coefficients are in fractions. Duration is in months, using the Understanding Society category means. All regressions include baseline likelihood categories or baseline duration, as appropriate, and individual controls. Mothers are excluded because their follow-up questions elicited current breastfeeding behavior rather than intentions for a future child.

5 Conclusion

This paper documents economically meaningful misperceptions about breastfeeding's effects on child and maternal outcomes. Baseline prior beliefs reveal systematic directional errors relative to the evidence we provide: women underestimate breastfeeding's protective effects on child health (gastroenteritis incidence) and cognitive development (IQ), overestimate its protective effect on maternal ovarian-cancer risk, and underestimate its negative association with maternal full-time employment. These misperceptions are not merely random noise but are correlated with stated breastfeeding intentions.

Women's beliefs prove surprisingly responsive to carefully presented evidence. Information leads to significant updating across three domains – child cognitive development (IQ), maternal health (ovarian cancer), and maternal employment (full-time work) – while the effect on diarrhoea beliefs is negligible. Moreover, this updating is not narrowly confined to exactly-shown outcomes; respondents generalize learned insights within conceptual domains, revising beliefs about related outcomes they did not directly encounter. Importantly, information also improves belief accuracy: treated respondents' posterior beliefs are substantially closer to the evidence, with absolute belief errors falling by roughly 50–60% across the three updating domains.

Belief updating translates into action-relevant intention shifts. The information treatment increases intended breastfeeding duration by 0.67 months on average, approximately 8.5% of the pre-treatment mean of 7.89 months, driven by upward revisions, although it does not raise the probability of being very likely to breastfeed. Respondents also become cognizant of labor-market tradeoffs, with stated work intentions adjusting downward in acknowledgment of employment costs.

The heterogeneity in treatment response illuminates who benefits most from information. Prior beliefs are the primary moderator of belief updating: women who entered the survey with less favorable beliefs about breastfeeding show the largest displacement, with treatment interactions ranging from 0.23 to 0.70 standard deviations across domains. This is consistent

with information providing the greatest incremental value where the gap between prior beliefs and the evidence is widest.

For breastfeeding intentions, motherhood status emerges as the key source of heterogeneity, although the two groups respond on different margins. Women without children, who can still form breastfeeding intentions unconstrained by a first birth experience, increase intended duration by approximately one month (1.02 months), about one and a half times the average treatment effect. Women who are already mothers show a smaller but statistically significant increase in duration (0.30 months), part of which reflects that many already report the longest duration category at baseline. Mothers, however, are 3.4 percentage points more likely to report being very likely to breastfeed, while women without children show no such response. Information thus appears to lengthen planned breastfeeding among women who have not yet given birth, and to strengthen the commitment to breastfeed among women who already have. Public health messaging may therefore be valuable both before the first birth and between births, with different expected effects in each case.

Two important caveats qualify interpretation of magnitudes. First, our survey data capture stated beliefs and intentions, not realized behavior. Second, some information treatments rely on evidence from non-UK contexts (e.g., the PROBIT trial was conducted in Belarus). While the magnitude of effects may differ in context-specific applications, the core finding, that evidence-based information can correct misperceptions and shift intentions, is likely to generalize across settings.

Information provision can be delivered at low cost through multiple channels: prenatal care appointments, public-health campaigns, digital platforms, and worker onboarding programs. Unlike structural policies (extended maternity leave, childcare subsidies, workplace lactation facilities), information provision involves no direct fiscal costs and scales easily. The results suggest that integrating evidence-based communication about breastfeeding benefits and costs into routine maternal and child health messaging can help women make more informed feeding and work-planning decisions. Testing information provision in clinical or policy-implementation settings would strengthen evidence on scalability and cost-effectiveness relative to traditional interventions.

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Appendix

A	Additional Tables
B	Departures from the Pre-Registration
C	Survey Screenshots

A Additional Tables

Table A.1. Summary Statistics

	Treated	Control	T–C	p-value	Overall
Age	31.47	31.50	-0.04	0.882	31.49
University	0.77	0.73	0.03	0.151	0.75
White	0.79	0.78	0.01	0.651	0.79
Employed	0.89	0.87	0.02	0.282	0.88
Full time	0.63	0.62	0.01	0.667	0.62
Weekly Hours Worked	30.72	29.55	1.16	0.124	30.13
Income £0-1500	0.26	0.30	-0.04*	0.093	0.28
Income £1500-2000	0.21	0.20	0.02	0.489	0.20
Income £2000-2500	0.24	0.23	0.01	0.688	0.24
Income £2500-3000	0.16	0.13	0.03	0.115	0.14
Income £3000-5000+	0.13	0.14	-0.01	0.477	0.13
Has a child	0.49	0.48	0.02	0.583	0.48
Own Mother Breastfed	0.53	0.50	0.03	0.218	0.51
Joint orthogonality test	$F(12, 1275) = 1.11$, $p = 0.351$, $N = 1288$				
Participants	668	669			1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports individual characteristics: columns 1 and 2 give means by treatment arm and column 5 the mean in the pooled estimation sample, while column 3 gives the treated-control difference and column 4 the p-value of the two-sample t-test of that difference. The joint orthogonality row reports a test in which treatment status is regressed on all characteristics in the table simultaneously (with heteroskedasticity-robust standard errors, and the lowest income band as the omitted category since the five income indicators are mutually exclusive and exhaustive) and the reported F-statistic tests that all coefficients are jointly zero. Its sample size is smaller than the full sample because the indicator for having a child is missing for some respondents.

Table A.2. Sample Representativeness

	Prolific Sample	APS 2025
Age	31.49	31.21
University	0.75	0.54
White	0.79	0.80
Employed	0.88	0.80
Full time	0.62	0.53
Weekly Hours Worked	30.13	28.65
Income £0-1500	0.17	0.28
Income £1500-2000	0.22	0.21
Income £2000-2500	0.28	0.21
Income £2500-3000	0.17	0.15
Income £3000-5000+	0.16	0.15
Participants	1,337	20,178

Notes: The first column reports the descriptive statistics from the survey, the second column reports corresponding figures computed from the Annual Population Survey (APS), January–December 2025 [Office for National Statistics. Social Survey Division. (2025). Annual Population Survey, 2025. SN: 9599], restricted to women aged 23–39 to match the survey's eligibility criteria and weighted using the person weight to give population-representative estimates. University, White, Employed, Full time, and Weekly Hours Worked are defined analogously to the Prolific sample. Income is net personal monthly income, matching the survey's own income question; because the underlying pay variable is only recorded for employees, the income row restricts both columns to respondents in paid employment (excluding the self-employed), whereas all other rows use the full sample in each column. Participants for the general population reflects the unweighted APS sample size, not the (much larger) weighted population estimate.

Table A.3. Standardized outcomes balance

	Treated	Control	T–C	Overall
Diarrhoea belief, pre	-0.00	0.00	-0.01	0.00
Diarrhoea belief, post	0.00	-0.00	0.00	0.00
Child IQ belief, pre	-0.03	0.03	-0.06	0.00
Child IQ belief, post	0.28	-0.28	0.56***	0.00
Ovarian cancer belief, pre	-0.03	0.03	-0.05	0.00
Ovarian cancer belief, post	0.27	-0.27	0.54***	0.00
Full-time employment belief, pre	0.06	-0.06	0.13**	-0.00
Full-time employment belief, post	-0.09	0.09	-0.17***	0.00
Vomiting belief, post	0.00	-0.00	0.01	-0.00
GCSE belief, post	0.13	-0.13	0.25***	-0.00
Breast cancer belief, post	0.13	-0.13	0.26***	0.00
Earnings belief, post	-0.04	0.04	-0.08	0.00
Log earnings belief, post	0.05	-0.05	0.10*	0.00
Breastfeeding intention, pre	0.03	-0.03	0.05	-0.00
Breastfeeding intention, post	0.04	-0.04	0.08	-0.00
Breastfeeding duration, pre	0.02	-0.02	0.03	0.00
Breastfeeding duration, post	0.08	-0.08	0.16***	-0.00
Participants	668	669		1337

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table reports standardized outcomes by treatment status. Each outcome is standardized to have mean zero and standard deviation one in the full analysis sample. The first two columns report treated and control means; T–C is the treated-minus-control difference. Stars refer to two-sided two-sample t-tests. Prior outcomes assess baseline balance; posterior differences may reflect the information treatment. Duration is standardized from the original survey coding.

Table A.4. Post-treatment Beliefs and Intentions

	Mean / share	SD	Min	Max	N
Infant diarrhoea (%)	18.46	12.19	0	100	1337
Child IQ (points)	105.87	4.37	85	115	1337
Ovarian cancer (per 100,000)	40.04	13.77	0	100	1337
Maternal full-time employment (%)	27.08	14.84	0	96	1337
Infant vomiting (%)	12.80	10.53	0	100	1337
GCSE attainment (%)	56.22	12.09	0	100	1337
Breast cancer (studies)	50.09	29.38	0	98	1337
Monthly earnings (£)	1062.30	1077.65	0	35000	1337
Very likely to breastfeed	0.57	0.50	0	1	1337
Intended breastfeeding duration (months)	8.35	4.51	0	12.52	1337
Intends full-time work after birth	0.47	0.50	0	1	1337

Notes: The table reports descriptive statistics for beliefs and intentions as elicited after the information treatment, in their original units and pooled across treatment arms, with intended duration expressed in months using the Understanding Society category means; their pre-treatment counterparts are reported in Table 1.

Table A.5. Treatment effects on beliefs by motherhood status

	Diarrhoea (1)	IQ (2)	Ovarian cancer (3)	Full-time employment (4)
Panel A: Women without children				
Treatment effect	0.009	0.662***	0.376***	-0.294***
	(0.063)	(0.056)	(0.061)	(0.060)
Prior belief control	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Prior mean	17.670	104.318	37.596	26.898
Prior SD	12.436	4.443	14.681	15.856
Observations	666	666	666	666
Panel B: Mothers				
Treatment effect	0.061	0.506***	0.842***	-0.166**
	(0.071)	(0.062)	(0.067)	(0.073)
Prior belief control	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Prior mean	17.923	104.826	33.159	27.299
Prior SD	14.007	4.767	15.416	15.571
Observations	622	622	622	622

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. OLS estimates of equation (1) separately for women without children ($N = 666$) and women with at least one child ($N = 622$). Respondents with missing motherhood status are excluded. Prior and posterior beliefs are standardized separately in the full analysis sample, retaining a common scale across groups. Each specification controls for the corresponding prior belief and the individual characteristics listed in Table 3. Prior means and standard deviations are in the original outcome units and refer to the respective subgroup.

Table A.6. Heterogeneity in treatment effects on intentions by demographic characteristics

	Non-W. (1)	White (2)	Not Married (3)	Married (4)	Mo. no BF (5)	Mo. BF (6)	Age < p50 (7)	Age ≥ p50 (8)	Low Inc. (9)	High Inc. (10)
VL	-0.003	0.014	0.001	0.017	-0.003	-0.001	0.012	0.003	0.003	0.007
	(0.036)	(0.014)	(0.019)	(0.017)	(0.017)	(0.019)	(0.019)	(0.018)	(0.020)	(0.017)
Duration	0.532**	0.697***	0.765***	0.500***	0.484***	0.692***	0.872***	0.466***	0.604***	0.751***
	(0.270)	(0.115)	(0.157)	(0.140)	(0.148)	(0.157)	(0.171)	(0.135)	(0.148)	(0.158)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	285	1052	734	603	516	688	659	678	652	685

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. Each column reports OLS estimates of the treatment effect on VL (probability of being very likely to breastfeed) and Duration (intended months) from a separate regression on the indicated subsample. Subgroups: Non-W. = non-white; Single = not married; Mo. no BF/Mo. BF = respondent's own mother did not breastfeed / did breastfeed her (respondents answering "I don't know" are excluded from both columns); Age $</\geq$ p50 = below/above median age; Low/High Inc. = monthly income below/above £2000. All specifications include individual controls.

Table A.7. Robustness: attention-check subsamples

	Diarr.	IQ	Ov.Ca	FT Emp	VL	Duration
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Begin-survey attention check						
Treatment Effect	0.016	0.594***	0.568***	-0.240***	0.007	0.667***
	(0.046)	(0.041)	(0.046)	(0.046)	(0.013)	(0.107)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	1336	1336	1336	1336	1336	1336
Panel B: End-survey attention check						
Treatment Effect	0.010	0.610***	0.557***	-0.242***	0.009	0.674***
	(0.047)	(0.040)	(0.046)	(0.046)	(0.013)	(0.109)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	1294	1294	1294	1294	1294	1294

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses. Panel A restricts the sample to respondents who correctly entered 1719 in the beginning-of-survey attention check. Panel B restricts to respondents who reported paying "Very closely" or "Extremely closely" attention at the end of the survey. Columns 1–4 report OLS ANCOVA estimates of equation (1) with standardized posterior beliefs as dependent variables (Diarr., IQ, Ov.Ca, FT Emp). Columns 5–6 report treatment effects on breastfeeding intentions: VL (probability of being very likely to breastfeed) and Duration (intended months). All specifications include individual controls.

B Departures from the Pre-Registration

The study was pre-registered in the AsPredicted registry (#262970; `https://aspredicted.org/9bf4ad.pdf`). This appendix lists the ways in which the analysis departs from the pre-registered plan and explains the reasons for each change. Point numbers refer to the questions in the pre-registration.

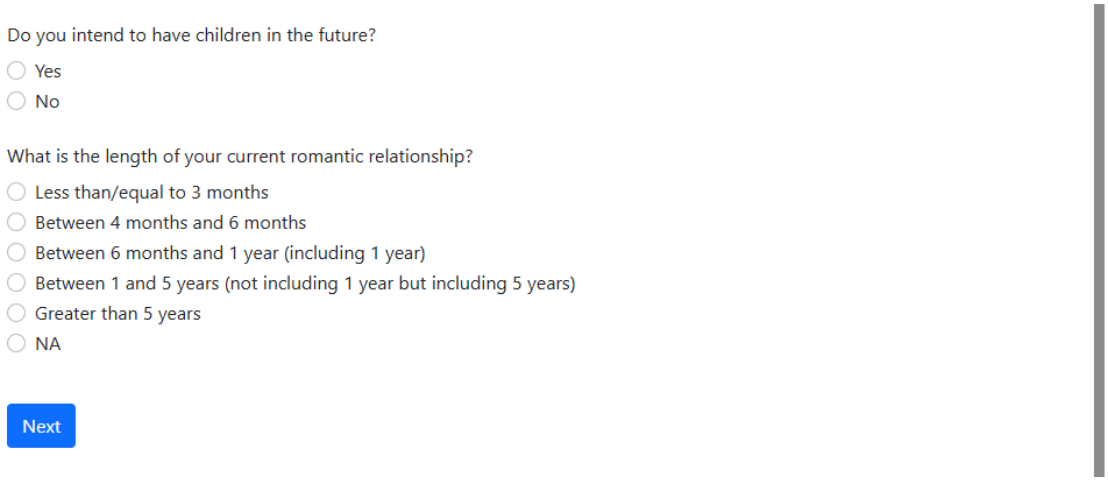
Estimating equation (Points 3, 5a, and 8). The pre-registration specifies a within-individual first-difference model, in which the change in beliefs or intentions is regressed on the treatment indicator. We instead estimate the ANCOVA specification in equation (1), which regresses the post-treatment outcome on the treatment indicator and controls for the pre-treatment outcome. Unless pre- and post-treatment outcomes are perfectly correlated, ANCOVA yields a more efficient estimate of the treatment effect (McKenzie, 2012). Estimating the coefficient on the pre-treatment outcome, rather than imposing a coefficient of one, also makes the specification robust to mean reversion arising from measurement error in elicited beliefs. Results from the first-difference model are qualitatively similar.

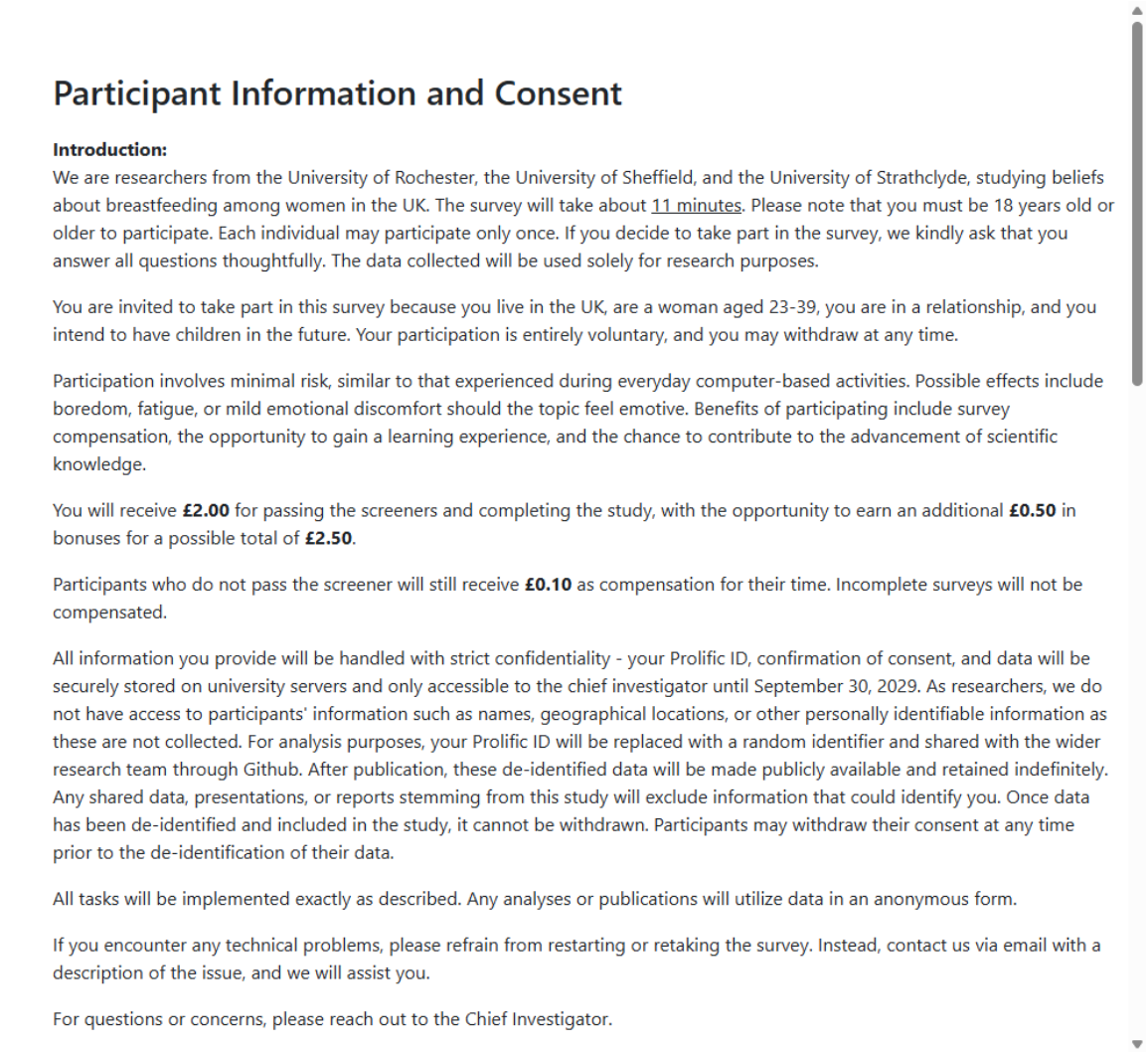
Sample (Point 7). The pre-registration targets women who have been in a relationship for at least one year, intend to have children, and do not yet have children. We extended the sample to women who already have children and intend to have more, for three reasons. First, Prolific's screening filters did not fully exclude women with children. Second, restricting the sample to women without children would have reduced the available pool considerably. Third, women who already have children and intend to have more are themselves informative: their beliefs and intentions are shaped by experience with breastfeeding, and they face a future breastfeeding decision. We report results separately by motherhood status throughout.

Belief changes and intentions (Point 8). The pre-registration specifies regressions of breastfeeding intentions and labor-market plans on changes in beliefs about maternal labor-market outcomes, maternal health, and children's health and cognitive skills, together with the standard set of baseline controls. We replace this specification with the one reported in Table 9, which interacts the treatment indicator with standardized measures of prior underestimation. Belief changes have a different meaning in the treatment and control groups: in the treatment group they reflect the response to information, while in the control group they reflect re-elicitation noise and reconsideration. Belief changes are also measured after treatment and are therefore themselves outcomes of the experiment, so conditioning on them does not identify a causal effect. Prior beliefs, by contrast, are measured before random assignment, so their interactions with treatment identify heterogeneity in treatment effects.

C Survey Screenshots

C.1 Main Survey





Participant Information and Consent

Introduction:
We are researchers from the University of Rochester, the University of Sheffield, and the University of Strathclyde, studying beliefs about breastfeeding among women in the UK. The survey will take about 11 minutes. Please note that you must be 18 years old or older to participate. Each individual may participate only once. If you decide to take part in the survey, we kindly ask that you answer all questions thoughtfully. The data collected will be used solely for research purposes.

You are invited to take part in this survey because you live in the UK, are a woman aged 23-39, you are in a relationship, and you intend to have children in the future. Your participation is entirely voluntary, and you may withdraw at any time.

Participation involves minimal risk, similar to that experienced during everyday computer-based activities. Possible effects include boredom, fatigue, or mild emotional discomfort should the topic feel emotive. Benefits of participating include survey compensation, the opportunity to gain a learning experience, and the chance to contribute to the advancement of scientific knowledge.

You will receive **£2.00** for passing the screeners and completing the study, with the opportunity to earn an additional **£0.50** in bonuses for a possible total of **£2.50**.

Participants who do not pass the screener will still receive **£0.10** as compensation for their time. Incomplete surveys will not be compensated.

All information you provide will be handled with strict confidentiality - your Prolific ID, confirmation of consent, and data will be securely stored on university servers and only accessible to the chief investigator until September 30, 2029. As researchers, we do not have access to participants' information such as names, geographical locations, or other personally identifiable information as these are not collected. For analysis purposes, your Prolific ID will be replaced with a random identifier and shared with the wider research team through Github. After publication, these de-identified data will be made publicly available and retained indefinitely. Any shared data, presentations, or reports stemming from this study will exclude information that could identify you. Once data has been de-identified and included in the study, it cannot be withdrawn. Participants may withdraw their consent at any time prior to the de-identification of their data.

All tasks will be implemented exactly as described. Any analyses or publications will utilize data in an anonymous form.

If you encounter any technical problems, please refrain from restarting or retaking the survey. Instead, contact us via email with a description of the issue, and we will assist you.

For questions or concerns, please reach out to the Chief Investigator.

Chief Investigator details:
Agnese Romiti
University of Strathclyde
Email: agnese.romiti@strath.ac.uk

This study has been approved by the Department of Economics Committee of the University of Strathclyde.

For questions, concerns, or to contact an independent person for further information, please contact:

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Consent Form:

I confirm that I have read and understood the Participant Information Sheet and the researchers have answered any queries satisfactorily.
I confirm that I have read and understood the Privacy Notice for Participants in Research Projects, including how my personal information will be used, and for how long.
I understand that my participation is voluntary, and I can withdraw at any time without consequences by contacting the chief investigator Agnese Romiti.
I understand that I can request the withdrawal from the study of some personal information and researchers will comply where possible.
I understand that de-identified data cannot be withdrawn once included in the study.
I understand that all information recorded in the research will remain confidential, and my identity will not be publicly disclosed.
I consent to being a participant in the project.

By clicking "Yes, I consent to participate in the study," you confirm that you are at least 18 years, have read and understand the above information, and you agree to participate in this online study.

- ○ Yes, I consent to participate in the study
- ○ No, I would not like to participate in the study (you will exit the study now).

What is your unique Prolific ID?

Next

Symbol £ denotes questions that are candidate for the monetary incentive.

Some of the questions that follow will ask you to make estimates and will be marked with a £ sign. One of these questions will be randomly selected for payment. If your answer in the selected question is within 2 percent of the truth, then you will receive a £0.50 bonus. Therefore, it is in your best interest to provide your best guess.

Next

In surveys like ours, some participants may not thoroughly read each question and instead quickly navigate through the survey. This behaviour can lead to random responses, compromising the results of research studies. To confirm that you are reading our questions attentively, please enter 1719 as your response to this question: what is your favourite number?

Next

Which of the following best describes your intended work plan after childbirth?

- ○ I plan to take maternity leave and return to my current job full-time.
- ○ I plan to take maternity leave and return to full time work with a different employer.
- ○ I plan to take maternity leave and return to my current job part-time.
- ○ I plan to take maternity leave and return to part-time work with a different employer.
- ○ I plan to stop working temporarily and return to work later.
- ○ I plan to stop working permanently.

How likely are you to breastfeed when you have a child (breastfeeding also includes expressing milk)?

○ Very unlikely ○ Somewhat unlikely ○ Neither likely nor unlikely ◉ Somewhat likely ○ Very likely

Your answer to the previous question is somewhat likely and above, for how many months do you think you would breastfeed?

Next

Some of the questions in this survey require basic numeracy and an understanding of probability. Please answer the following question as a short practice exercise:

If the chance of getting a disease is 10 percent, how many people out of 1,000 would be expected to get the disease?

Next

We now provide a hypothetical example, which may serve as an exercise for you to get familiar with the structure of the next questions.

According to a study, 12% of commuters miss their train when they arrive at the platform 2 minutes before the scheduled departure time.

What do you think the percentage would be if these commuters arrived 5 minutes before the scheduled departure time?

The slider below is currently set to 12%.

If you think that arriving earlier reduces the likelihood of missing the train, then this percentage should be lower than 12%, and you should move the slider accordingly.

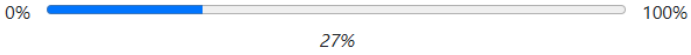
0% 100%

12%

Next

£ In the UK, 27% of infants aged 6 months who have not been breastfed report one or more episodes of diarrhoea (a common symptom of gastroenteritis).

What do you think the percentage would be if these children were breastfed for 4 months or longer?



Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

£ IQ is the most widely used measure of intelligence. For adults, the average IQ is 100, and 9 out of 10 people have an IQ between 85 and 115. According to a UK study, the average IQ of children aged 8.5 who were not breastfed or were breastfed for less than one month is 101.

What do you think the average IQ of these children would be if there were breastfed for at least 6 months?

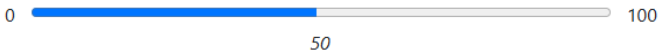


Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

£ A large UK study of around 800,000 women aged 50 and older shows that among those who never breastfed, there are about 50 cases of ovarian cancer per 100,000 women per year.

How many women out of 100,000 who breastfeed do you think would develop ovarian cancer?



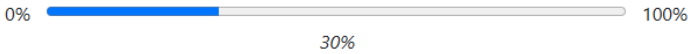
Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

£ Using data from a UK Survey, we calculate that among mothers who were working full-time before the birth of their first child and chose not to breastfeed, 30% are working full-time 3 to 7 years after the birth of their child.

We then computed the same statistics for women with similar income and education levels who breastfed for 7 months or longer.

What percentage of those mothers breastfeeding 7 months or longer do you believe is working full-time?



Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

How confident are you in the answers you provided before?

○ Very unsure ○ Unsure ○ Somewhat sure ○ Sure ○ Very sure

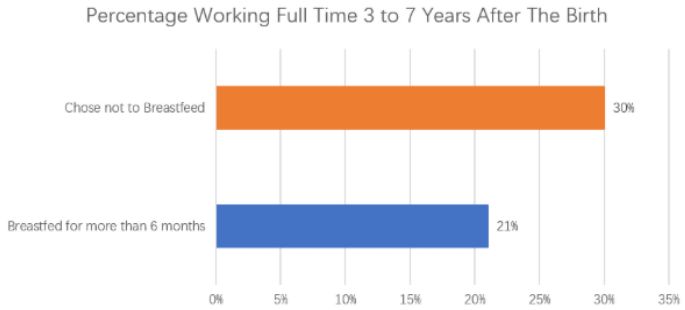
Next

Now, we are going to provide you with a few facts derived from research papers published in well-known scientific journals or estimates we have obtained from high-quality UK-based data.

Next

Using data from a UK Survey*, we calculate that among those mothers who were **working full time** before the birth of their first child and chose to breastfeed for *more than 6 months*, only **21%** are working full time between 3 to 7 years after the birth of their child. We then computed the same statistics for women with similar income and education levels who *chose not to breastfeed*, **30%** of those are working full time 3 to 7 years after the birth.

This means that mothers who chose to breastfeed have a full-time employment rate that is **9** percentage points **lower** than similar mothers who chose not to breastfeed.



Next (18s)

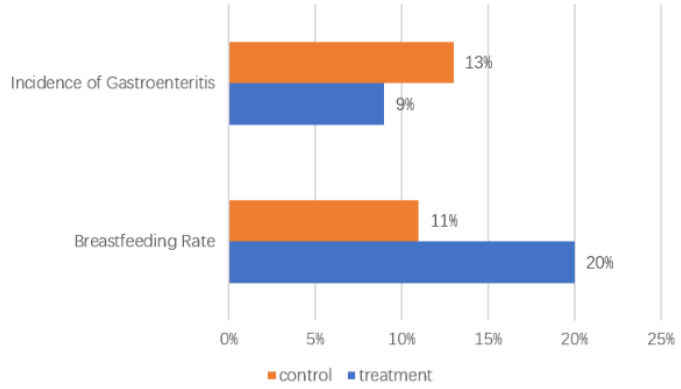
* Understanding Society and its Pregnancy and Early Childhood datafile (PEACH)

Source: University of Essex, Institute for Social and Economic Research. (2024). Understanding Society: Pregnancy and Early Childhood (PEACH), 2009-2022. [data collection]. 3rd Edition. UK Data Service. SN: 9075, DOI: http://doi.org/10.5255/UKDA-SN-9075-3

While studies for the UK do not exist, evidence from Belarus shows that increasing breastfeeding during the first year of life reduces the incidence of gastroenteritis (*i.e.*, **stomach bugs**).

In this study, over 17,000 mothers were randomly assigned to two groups. One group received additional support to start and continue breastfeeding (the *treatment group*), while the other received standard infant-feeding guidance (the *control group*).

At 12 months, **20%** of mothers in the *treatment group* were still breastfeeding, compared with **11%** in the *control group*. In their first year of life, **9%** of babies in the *treatment group* developed gastroenteritis compared with **13%** in the *control group*.



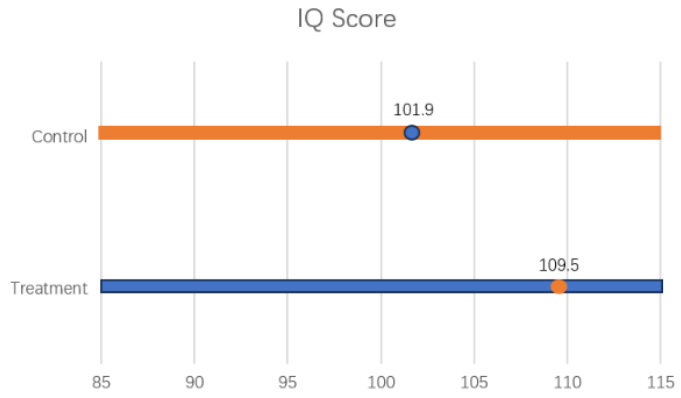
Next

* Source: Kramer, M.S., Chalmers, B., Hodnett, E.D., Sevkovskaya, Z., Dzikovich, I., Shapiro, S., Collet, J.P., Vanilovich, I., Mezen, I., Ducruet, T. and Shishko, G., 2001. Promotion of Breastfeeding Intervention Trial (PROBIT): a randomized trial in the Republic of Belarus. *Jama*, 285(4), pp.413-420.

A different study utilising the same experiment from Belarus we have discussed in the previous page shows that higher breastfeeding rates lead to better **cognitive outcomes** at age 6.5.

As before, over 17,000 mothers were randomly assigned to two groups. The *treatment group* received additional support to start and continue breastfeeding, while the *control group* received standard infant-feeding guidance.

At age 6.5, children in the *treatment group* scored higher on IQ tests (average **109.5**) than those in the *control group* (average **101.9**).



Next

* Source: Kramer, M.S., Aboud, F., Mironova, E., Vanilovich, I., Platt, R.W., Matush, L., Igumnov, S., Fombonne, E., Bogdanovich, N., Ducruet, T. and Collet, J.P., 2008. Breastfeeding and child cognitive development: new evidence from a large randomized trial. *Archives of general psychiatry*, 65(5), pp.578-584.

The large UK study on breastfeeding and **ovarian cancer** mentioned earlier, which analysed about 800,000 women, found that mothers who breastfed had a **lower** risk of developing ovarian cancer. Among those who breastfed, there are about **48** cases of ovarian cancer per 100,000 women per year compared to **50** cases among those who never breastfed.

Next

* Source: Gaitskell, K., Green, J., Pirie, K., Barnes, I., Hermon, C., Reeves, G.K., Beral, V. and Million Women Study Collaborators, 2018. Histological subtypes of ovarian cancer associated with parity and breastfeeding in the prospective Million Women Study. *International journal of cancer*, 142(2), pp.281-289.

We would now like to ask you some additional questions about breastfeeding, children, and maternal outcomes - some are new, while others may repeat questions from earlier sections. Please answer all of them.

Next

£ In the UK, 27% of infants aged 6 months who have not been breastfed report one or more episodes of diarrhoea (a common symptom of gastroenteritis).

What do you think the percentage would be if these children were breastfed for 4 months or longer?

0% 100%

27%

Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

£ The UK study examining infants we described before shows that 16% of them who have not been breastfed experience one or more episodes of vomiting (a common symptom of gastroenteritis).

What do you think the percentage would be if these children were breastfed for 4 months or longer?

0% 100%

16%

Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

£ As a reminder, according to the UK study we mentioned earlier, the average IQ of children aged 8.5 who were not breastfed or breastfed for less than one month is 101.

What do you think the average IQ of these children would be if there were breastfed for at least 6 months?

85 115

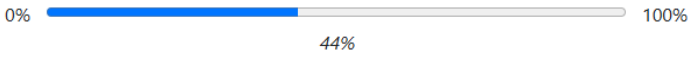
101

Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

According to a UK study examining about 5,000 children, 44% of children who were never breastfed passed 5 or more GCSEs[1] with a mark of 5 or higher.

What is this percentage for these children if they were breastfed for 6 months?

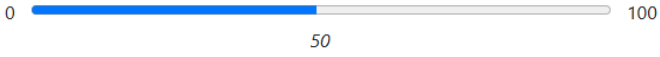


Next

[1] Passing five GCSEs at grade 5 or above is a metric commonly used as key benchmark for academic achievement at the end of secondary school in England, Northern Ireland, and Wales. This metric is roughly equivalent to achieving five National 5s at grades A to C in the education system in Scotland.

£ The large UK study of around 800,000 women we mentioned shows that among those who never breastfed, there are about 50 cases of ovarian cancer per 100,000 women per year.

How many women out of 100,000 who breastfeed do you think would develop ovarian cancer?



Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

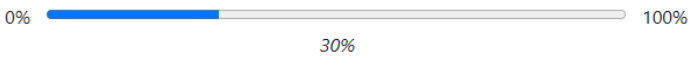
£ Researchers have conducted numerous studies to assess the link between breastfeeding and breast cancer. A comprehensive review examined 98 studies from various countries. Out of these 98 studies, how many indicate that women who have ever breastfed have a lower risk of breast cancer?

Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

£ As mentioned before, in the UK we calculate that among mothers who were working full-time before the birth of their first child and did not breastfeed, 30% are working full-time between 3 and 7 years after the birth of their child.

What do you think this percentage would be for women with similar income and education levels who breastfed for 7 months or longer?



Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

£ Using data from a UK survey, we estimate that the average monthly gross (before tax) labour earnings, between 3 and 7 years after the birth of their child, are around £1,080 for mothers who decided not to breastfeed their child.

What do you think the labour earnings would be for these mothers if they had breastfed for 7 months or longer?

Next

* The symbol **£** denotes questions that are candidates for the monetary incentive.

Which of the following best describes your intended work plan after childbirth?

- ○ I plan to take maternity leave and return to my current job full-time.
- ◉ I plan to take maternity leave and return to full time work with a different employer.
- ○ I plan to take maternity leave and return to my current job part-time.
- ○ I plan to take maternity leave and return to part-time work with a different employer.
- ○ I plan to stop working temporarily and return to work later.
- ○ I plan to stop working permanently.

How likely are you to breastfeed when you have a child?

○ Very unlikely ◉ Somewhat unlikely ○ Neither likely nor unlikely ○ Somewhat likely ○ Very likely

Your answer to the previous question is somewhat unlikely and below. What are the factors that drive this decision:

- ○ Job-related factors.
- ○ Lack of support (e.g., lactation consultants.)
- ○ I don't think that breastfeeding brings any benefit.
- ○ Other reasons/prefer not to say.

Next

What is your age?

What is your gender?

- ○ Female
- ○ Other
- ○ Prefer not to say

To which of these ethnic groups do you consider you belong?

- ○ Asian or Asian British
- ○ Black, Black British, Caribbean or African
- ○ Mixed or multiple ethnic groups
- ○ White
- ○ Other ethnic group

Which is the highest qualification you have?

- ○ No Qualification
- ○ Other Qualification
- ○ GCSE or equivalent
- ○ A-levels or equivalent
- ○ Degree or Higher

What is your current marital status?

- ○ Married
- ○ Living as a couple
- ○ Civil partnership
- ○ Other

Are you currently pregnant?

- ○ Yes
- ○ No
- ○ Prefer not to say

Do you have any children?

- Yes
- No

If yes, how many children do you have? Please select "None" if you do not have any children.

Which of this best describes your current employment situation?

- Self-employed
- Employed
- Unemployed
- Retired
- Family care or home
- Full-time student
- Long-term sick or disabled
- On apprenticeship
- Other

Do you work part-time or full-time?

- Full-time (usually 35 hours or more per week)
- Part-time (usually less than 35 hours per week)
- Not Applicable

How many hours do you typically work per week?

What was your (main) job in the week ending last Sunday? Please write your job title below.

Which of the following best describes the person(s), organisation(s) or client(s) you do work for this job?

Please select the industry sector of your current job? If you hold more than one job, please refer to the main job.

What is your personal typical monthly net income?

Which of this best describes your partner's current employment situation?

- Self-employed
- Employed
- Unemployed
- Retired
- Family care or home
- Full-time student
- Long-term sick or disabled
- On apprenticeship
- Other

Does your partner work part-time or full-time?

- Full-time (usually 35 hours or more per week)
- Part-time (usually less than 35 hours per week)
- Not Applicable

How many hours does your partner typically work per week?

What was your partner's (main) job in the week ending last Sunday? Please write your partner's job title below.

Which of the following best describes the person(s), organisation(s) or client(s) your partner does work for this job?

Please select the industry sector of your partner's current job? If your partner hold more than one job, please refer to the main job.

What is your partner's typical monthly net income?

Thinking back to when you were less than 11 years old, was your mother working?

- Full-time
- Part-time
- No, not working at all
- I do not know

To the best of your knowledge, was your mother breastfeeding you?

- Yes
- No
- I do not know

Next

To what extent did you pay close attention to the instructions throughout the survey? Your response to this question will not affect your earnings.

○ Not at all ○ Slightly ○ Moderately ○ Very closely ○ Extremely closely

Next

Can you say in a few words what you think the goal of this survey was?

Next

When do you approximately plan to have children in the future?

Next

C.2 Obfuscated Follow-up

Participant Information and Consent

Before you begin:
This research is being carried out by a team of researchers from two UK universities and one university in the United States. It involves reading some information and answering a few short questions. You have been invited because you live in the UK, are a woman aged 23-39, you are in a relationship, and you intend to have children in the future. The survey takes approximately 2 minutes to complete.

Participation is entirely voluntary. You may withdraw at any time before finishing the survey by closing your browser, without providing a reason and without any consequences. You will receive **£0.50** as a base participation payment. Please note that no payment will be provided for partially completed responses.

The potential risks of taking part include boredom, fatigue, or mild distress if you find the topic emotive. The potential benefits include the participation payment, the opportunity to reflect on the topic, and contributing to scientific research.

We do not collect any identifying information, and your data will remain confidential. Data linked to your Prolific ID will be stored securely until **30 September 2029** and, for the purpose of analysis, your ID will be replaced with a random identifier for analysis. Data without your ID may be shared after publication for the purpose of replication and retained indefinitely. Any shared data, presentations, or reports about the study will not identify you personally. Once anonymised (i.e. stripped of information that could identify you), your data cannot be withdrawn.

Thank you for reading this information. If you have any questions, please ask before starting the survey. Your data will be handled in accordance with data protection legislation.

After completing the survey, you will be provided with details about the Chief Investigators, the research ethics committee, and the study title.

Consent Form
By answering the consent question below, you confirm that:

- You have read and understood the Participant Information Sheet and the researchers have answered any queries satisfactorily.
- You confirm that you have read and understood the Privacy Notice for Participants in Research Projects, including how your personal information will be used, and for how long.
- You understand that your participation is voluntary, and you can withdraw at any time without consequences by contacting the chief

investigator.

- You understand that you can request the withdrawal from the study of some personal information and researchers will comply where possible.
- You understand that de-identified data cannot be withdrawn once included in the study.
- You understand that all information recorded in the research will remain confidential, and your identity will not be publicly disclosed.
- You consent to being a participant in the project.

If you are 18 years of age or older, have read and understand the information above, and freely consent to participate in the study, please click on the "I agree" button to begin the study.

- ○ I agree.
- ○ I disagree.

What is your unique Prolific ID?

Next

This survey asks for your views on a range of issues related to family life, health, and public policy.

Schools and early-years childcare providers in the UK are regularly inspected and rated by national education bodies, for example, Ofsted in England, Education Scotland, Estyn in Wales, and the Education and Training Inspectorate in Northern Ireland (e.g., Outstanding, Good, Requires Improvement, Inadequate).

Thinking about where you might live when you have children, how much influence would these inspection ratings have on your choice of neighbourhood or local area?

○ Not at all important ○ Slightly important ○ Moderately important ○ Very important ○ Extremely important

Next

To what extent do you support or oppose the government's plan to ban the sale of tobacco to anyone born on or after 1 January 2009, with the aim of creating a 'smoke-free generation'?

○ Fully disagree ○ Disagree ○ Neither agree nor disagree ○ Agree ○ Fully agree

Next

If you were to have a child in the future, how likely would you be to open a savings account for them?

○ Very unlikely ○ Somewhat unlikely ○ Neither likely nor unlikely ○ Somewhat likely ○ Very likely

Next

Do you have any children?

- (●) Yes
- ( ) No

Are you breastfeeding your child(ren)?

- (●) Yes
- ( ) No

Until what age of your child(ren) do you plan to continue breastfeeding your child(ren)?

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3 months or less
4-6 months
7 months or more

Do you have any children?

- ( ) Yes
- (●) No

How likely are you to breastfeed when you have a child?

( ) Very unlikely ( ) Somewhat unlikely ( ) Neither likely nor unlikely (●) Somewhat likely ( ) Very likely

Your answer to the previous question is somewhat likely and above, until what age of your child do you plan to continue breastfeeding?

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3 months or less
4-6 months
7 months or more

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