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LISTEN TO HER: GENDER DIFFERENCES IN INFORMATION DIFFUSION WITHIN THE HOUSEHOLD

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ABSTRACT

We study how economic information diffuses within the household, leveraging an information-provision experiment with a representative sample of households from Germany. A random sample of household members received information on their household's position in the income distribution. When provided with information directly, there are no gender differences in how individuals update their beliefs. However, we observe significant gender disparities in the diffusion of information within the household. Specifically, when only the husband receives the information, it influences the wife's beliefs; however, when only the wife receives the information, it does not affect the husband's beliefs.

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1 Introduction

About half a century ago, the UK government changed the allocation of subsidies for families with children, directing them towards mothers instead of fathers. According to the economic models of the time, such a policy should have no impact. The central tenet of these models was that the household functions as an integrated unit in which preferences are aligned, and information is available to all household members (Samuelson, 1956; Becker, 1981). These basic assumptions, however, has been criticized as unrealistic. Empirical evidence supports this criticism: providing the child allowance to mothers rather than fathers led to spending patterns more in line with the policy's intention of covering necessities, like clothing, for the family's children (Lundberg and Pollak, 1996; Lundberg, Pollak and Wales, 1997; Ward-Batts, 2008). Subsequent results have corroborated the view that households do not necessarily function as an integrated unit with common preferences over monetary resources – and that women's spending choices are deemed to be more beneficial to the family's children than men's (e.g., Dizon-Ross and Jayachandran, 2023). This is a prominent reason that cash transfer programs to the poor, more often than not, target women as the beneficiaries (Duflo, 2003; Almås et al., 2018; Armand et al., 2020; Field et al., 2021).

There has been a growing interest in economic research aimed at understanding how households function in the real world (e.g., Lundberg and Pollak, 1996; Ashraf, 2009; Chiappori and Mazzocco, 2017). The focus has been on how households manage resources such as consumption goods or money. A highly relevant question that has received comparably little attention so far is how households manage *information*. Arguably, information is as crucial a resource as money given that limitations on information accessible to spouses can impact their decision-making. The importance of information in intra-household decision-making has long been emphasized in the sociological literature (e.g., Dwyer and Bruce, 1988; Zelizer, 2005), while the common assumption in economics is that households pool their information, in particular when interests are aligned (Chiappori, 1992; Lundberg and Pollak, 1996). In this paper, we challenge this assumption and provide novel evidence on gender differences in how economic information diffuses within the household.

Studying diffusion of information within the household presents some empirical challenges: we need a setting in which we can, first, observe both spouses independently and repeatedly in their natural environment and, second, manipulate decision-relevant information exogenously. For this purpose, we leverage existing data from a two-year survey experiment with a representative sample

of Germans (Fehr, Mollerstrom and Perez-Truglia, 2022).[1] Our survey revolves around perceptions about the household's relative position in the income distribution. This setting is well-suited for studying learning because of widely-documented evidence that individuals hold significant misperceptions about their relative income (Cruces, Perez-Truglia and Tetaz, 2013; Engelhardt and Wagener, 2017; Karadja, Mollerstrom and Seim, 2017; Fehr, Mollerstrom and Perez-Truglia, 2022) and because perceptions about relative income are important for households in natural settings. For example, perceived relative income has been shown to affect preferences for redistribution (Cruces, Perez-Truglia and Tetaz, 2013; Engelhardt and Wagener, 2017; Karadja, Mollerstrom and Seim, 2017; Fehr, Mollerstrom and Perez-Truglia, 2022), subjective well-being (Perez-Truglia, 2020), and a wide range of decisions such as where to live (Bottan and Perez-Truglia, 2022) and whether to continue working at a company (Card et al., 2012; Cullen and Perez-Truglia, 2022).

In our baseline survey, we first elicited respondents' beliefs about their household's rank on the national and global income scale in an incentivized way. All adult members of a household were interviewed by professional interviewers in private, without the possibility of communicating with each other, so respondents could not share any information during the baseline survey even if they wanted to. After collecting the prior beliefs, half of the respondents received accurate information about their household's income rank. We randomized this information provision at the individual level to create variation within households. Thus, this resulted in households where both spouses, only the wife or husband, or nobody received the information, enabling us to explore how respondents acquire knowledge through direct information provision and indirectly through the diffusion of information within the household.

A year later, we conducted a follow-up survey with the same respondents, where we again asked incentivized questions about the household's income rank. While there was no opportunity for spouses to communicate during the interviews, they had ample opportunity to discuss the information about income ranks in the year that passed between the two survey waves if they chose to do so. Importantly, we did not provide any explicit incentives to share the information with other household members, and we did not inform respondents that we would be asking questions about relative income again a year later. As a result, information sharing evolved endogenously and naturally, with respondents freely choosing to share information with other household members, or to refrain from doing so.

[1] In the original study, we measure how beliefs about relative income affect preferences for redistribution (Fehr, Mollerstrom and Perez-Truglia, 2022). In this follow-up work, we further analyze the data to explore gender differences in the diffusion of information.

We start by documenting how individuals learn from information directly (i.e., when they receive it themselves). When spouses directly receive information about their true income ranks, the information has a significant and persistent effect on beliefs even after a whole year has passed. More importantly, men and women seem to incorporate the information to a similar degree when it is given directly to them. After one year, the learning rate is around 0.2 and does not differ statistically between women and men. More precisely, for each percentage point shock in the information given directly to a respondent, the perceived income rank as measured a year later is higher by about 0.22 percentage points for women and 0.16 for men (difference p-value = 0.391).

In contrast, we find stark differences by gender in how information diffuses within the household, with the pass-through of information from wives to husbands being substantially lower than from husbands to wives. If husbands received information about the true income rank directly whereas their wives did not, we observe a pass-through to their wife's belief that is about as strong as if the wife received the information directly. However, if a wife receives the information directly, we see no effect on her husband's belief. The difference in the rates of indirect learning (0.19 for women vs. -0.01 for men) is large and statistically significant (p-value = 0.040). Our findings further indicate that this asymmetry is not due to different communication and information acquisition patterns of women and men, nor to gender differences in the interest in information about relative income. Our evidence indicates that this phenomenon is specific to the household context, in that men are equally likely to incorporate information given to them directly by male versus female interviewers.

We contribute to an emerging literature on information flows within households. The bulk of this literature is concerned with decision situations in which incentives are *non-aligned* and preferences differ, such as fertility decisions (Ashraf, Field and Lee, 2014; Apedo-Amah, Djebbari and Ziparo, 2020; Ashraf et al., 2022).[2] The evidence from these experiments shows that information in such settings only sometimes flows freely and that information barriers can result in inefficient behavior (e.g., Ashraf, 2009; Ashraf, Field and Lee, 2014; Ashraf et al., 2022). For instance, Ashraf et al. (2022) conducted an information intervention in which they either informed husbands or wives about maternal health risks. Consistent with our findings, they find that the information spills over from husbands to wives but not in the other direction. We contribute to this literature by studying a real-world situation in which incentives are *aligned*, which is arguably one of the

[2]More generally, there are some studies exploring gender differences in how information flows outside households (e.g., Beaman and Dillon, 2018; Cullen and Perez-Truglia, 2023; BenYishay et al., 2020).

more common settings in practice yet one that has received little attention. A notable exception is a study by Conlon et al. (2022) that focuses, as we do, on a situation with aligned preferences. In their laboratory experiment with 400 married couples from Chennai, India, the husband or wife gets signals about the number of differently colored balls in an urn. They can pass this information on to their spouse, and the spouse can subsequently use it to make an optimal guess about the color of the ball that is drawn next. Despite the explicit incentives to share this information, and consistent with our own findings, Conlon et al. (2022) document pronounced gender differences in information diffusion: while wives took the information discovered by their husbands into full consideration, husbands failed to do the same with information revealed to their wives.

We complement the work of Conlon et al. (2022) in several important ways. First, different from their stylized setting (participants received information by drawing balls from an urn), our setting is one of endogenously and naturally-occurring information diffusion over a long time. Our subjects could naturally share the information in their daily lives over the span of a whole year, but we did not provide any explicit incentives to do so. Second, rather than studying beliefs about an abstract object (the colors of balls from an urn), we study a belief that households arguably care about above and beyond the context of our experiment: their relative income. Third, together Conlon et al. (2022) and our paper show that the gender differences in information diffusion hold across very different cultural and economic contexts. Gender norms are, for example, less pronounced in Germany than in India: according to the World Values Survey, 52% of Indians agree with the statement that men should have more rights to a job than women if jobs are scarce, while only 15% of Germans agree with the same statement. There are also significant differences in education between the two countries: according to World Bank Data, less than 30% of the Indian population enrolls in tertiary education, for example, compared to over 70% in Germany.[3] In summary, while Conlon et al. (2022) have a more controlled setting, it is also more artificial. On the other hand, our work is set in a natural field setting which, naturally, comes with somewhat less control but arguably with higher external validity. However, when taken together, these two studies paint a consistent picture that even in environments with aligned interests, gender barriers to information flow exist and are robust across different cultural and economic contexts.

[3]This education gap is also reflected in the two study samples. The average years of schooling are 7.99 among the Chennai couples in Conlon et al. (2022) and 11.64 in our German sample.

2 Research Design and Data

We implemented two tailor-made survey modules in the Innovation Sample of the German Socio-Economic Panel (SOEP-IS). The SOEP-IS is a comprehensive longitudinal study that, once a year, surveys a representative sample of the German population on a wide range of topics. It is the ideal test-bed for our research question and offers several advantages over other survey modes: First, all household members above 16 are interviewed by professional interviewers in computer-assisted interviews conducted in person. Second, we can follow up with little attrition a year later. Third, the face-to-face interviews provide significant control, minimize non-response and allow us to instantly clarify misunderstandings. Important for our purposes, it also prevents information look-up and communication between household members *during* and *between* the interviews *within a wave* because the interviews were conducted in private with each member of a household. Fourth, through SOEP we have access to a rich set of measures of socio-economic indicators. Fifth, the SOEP team implements various safeguards to ensure high data quality, such as pre-testing new items and conducting plausibility and consistency checks after data collection (for more details, see Goebel et al., 2019).

Baseline Survey: At the beginning of the baseline survey, we asked respondents to assess their household rank in the income distribution. Specifically, we asked respondents to state their perceived rank both in the national (i.e., German) and the global income distribution, in randomized order. Because estimates of the global income distribution are only available on the per-capita, pre-tax level, we explained and informed all respondents about their per-capita pre-tax household income based on their stated absolute household income. They then stated their rank in the national and global income distributions, respectively, on a scale from 0 (poorest percentile) to 100 (richest percentile) in private (i.e., without the interviewer seeing the tablet screen – this was done in order to avoid social desirability bias to potentially impact answers). We incentivized both assessments of income rank to ensure that it was optimal for respondents to answer truthfully, and each assessment that was correct to the closest percentile was rewarded with €20. About 10-15 minutes later in the baseline survey, after respondents had answered several questions unrelated to our research, we randomized half of the respondents into a treatment providing them with accurate information about their true income rank in the national and global income distributions. The information briefly explained the source of the information, and then revealed the share of people

that are poorer at the national and global levels. This information was read out by the interviewer, who additionally visualized this information with customized graphs to ease understanding (see Appendix Figure A1 for a screenshot). The other half of the respondents received no information.

Follow-up Survey: One year later, we implemented our second survey module with the same sample of respondents. The setup of the follow-up survey closely followed the setup of the baseline survey. That is, we first collected information on household income and the number of household members and explained the concept of per-capita household income. We then asked respondents to state their rank in the national and global income distributions in private and to further assess how certain they are about their statements. Again, we rewarded accurate predictions (this time, we paid €10 for each accurate prediction). The main difference to the baseline survey was that we did not provide information on the true income rank in either context in the follow-up survey. Instead, we elicited respondents' willingness to pay (WTP) for information about their true rank in the national and global income distributions using a list-price version of the Becker-DeGroot-Marschak method (Becker, DeGroot and Marschak, 1964).[4] Finally, we asked *treated* respondents whether they had shared the information on the true income rank that they received in the baseline survey with anyone in the household during the past year. We asked *all* respondents whether they had looked for information about the distribution of national and global income.

Data: Our data contains the two survey modules that we implemented in the 2017 and 2018 waves of the SOEP-IS. A total of 1,392 respondents took part in the baseline survey, while 1,144 participated in the second survey (82% of the 1,392 respondents in the baseline survey). We focus our analysis on single and 2-person, mixed-gender households as explained in Section 3 below. This restriction results in a sample of 1,164 respondents in the baseline survey and 989 respondents in the follow-up survey (85% of the 1,164 respondents in the baseline survey). The lower participation rate in the follow-up survey may raise a concern that the experiment's information provision could have affected the decision to participate in the follow-up survey. This is, however, is not the case as there is no significant difference in the attrition rates between the control and treatment neither in the *full sample* (17% vs. 19%, p-value=0.392 for t-test of proportions) nor in the *restricted sample*

[4]For both pieces of information, we presented five scenarios in which respondents had to decide between receiving information about their true rank in the income distribution and receiving a monetary reward that progressively increased from 10 cents to 10 euros. Respondents made their decision in private, and we informed them that one randomly selected decision for each piece of information (national and global) would be implemented. Possible payments, and information provisions, were made at the end of the survey.

(14% vs. 17%, p-value=0.289 for t-test of proportions). In Appendix Tables A1-A4, we present several specifications showing that treatment status does not predict participation in the follow-up survey (for the restricted and full sample). Moreover, and as expected, the observable pre-treatment characteristics are balanced across treatment and control groups. Appendix Tables A5 and A6 present the results for the full and restricted sample and also split the samples for men and women (for more details, see Section A.4 in the Appendix).

3 Empirical Strategy

We want to estimate the direct and indirect impact of information provision on beliefs about income ranks one year later. For this purpose, we do not distinguish between beliefs about national and global income ranks, as explained below. We define T_i as a treatment indicator variable taking on the value 1 if a respondent received direct information on their households' income rank in the baseline survey and 0 otherwise. Similarly, $T_i^{indirect}$ is an indicator variable that takes on the value 1 if the respondent did not receive the information directly, but another member of their household did, and 0 otherwise. Let r_i^{prior} denote the perceived income rank in the baseline survey (i.e., the prior belief before receiving information) and r_i^{info} denote the information about the income rank that could be shown to the respondent. Consequently, $r_i^{info} - r_i^{prior}$ is the potential treatment: i.e., the misperception about the income rank. A positive difference indicates underestimation and a negative difference indicates an overestimation of the income rank. The direct information shock is given by $(r_i^{info} - r_i^{prior}) \cdot T_i$, while the indirect information shock is given by $(r_i^{info} - r_i^{prior}) \cdot T_i^{indirect}$. We use the following specification to estimate the direct and indirect rates of learning:

$$\begin{aligned} r_i^{posterior} = \alpha^{direct}(r_i^{info} - r_i^{prior}) \cdot T_i + \alpha^{indirect}(r_i^{info} - r_i^{prior}) \cdot T_i^{indirect} \\ +\beta_1(r_i^{info} - r_i^{prior}) + \beta_2 X_i + \epsilon_i \end{aligned} \tag{1}$$

The dependent variable, $r_i^{posterior}$, is the posterior belief about the income rank in the follow-up survey. The coefficients α^{direct} and $\alpha^{indirect}$ tell us how correcting misperceptions—directly or indirectly through the husband or wife—affect beliefs one year later. The parameter α^{direct} measures the direct learning rate, i.e., the effect of an additional percentage point of information shock given directly to individual i on their posterior belief. The parameter $\alpha^{indirect}$ measures the indirect rate of learning, i.e., the rate of pass-through between the information provided to respondent

i's spouse and respondent i's belief one year later. X_i is a vector of control variables that include the demographic characteristics of the respondent and the household. We estimate equation (1) separately for female and male respondents, and cluster standard errors at the household level.

For our baseline specification, we restrict our sample to single-member households and households consisting of two adult partners ($n = 989$). We include single-member households to improve the statistical power in the analysis of direct learning. We exclude households in which other adult household members besides the spouses were interviewed to avoid dealing with cases in which information can be transmitted from multiple household members (e.g., adult children, grandparents). We further restrict the sample to mixed-gender partners – same-sex households are a negligible share of the sample, and thus we do not have enough data to study them separately. Finally, we observe beliefs about each respondent's income rank at the national and global levels. In the analysis, we pool these two responses, as differentiating between the two belief statements is inessential for our purposes. This gives us two income rank observations for each respondent, resulting in a total of $n = 1,978$ observations. All of the regressions report standard errors clustered at the household level. In the Appendix Section A.5, we show that our results do not depend on any of the specification choices listed above.

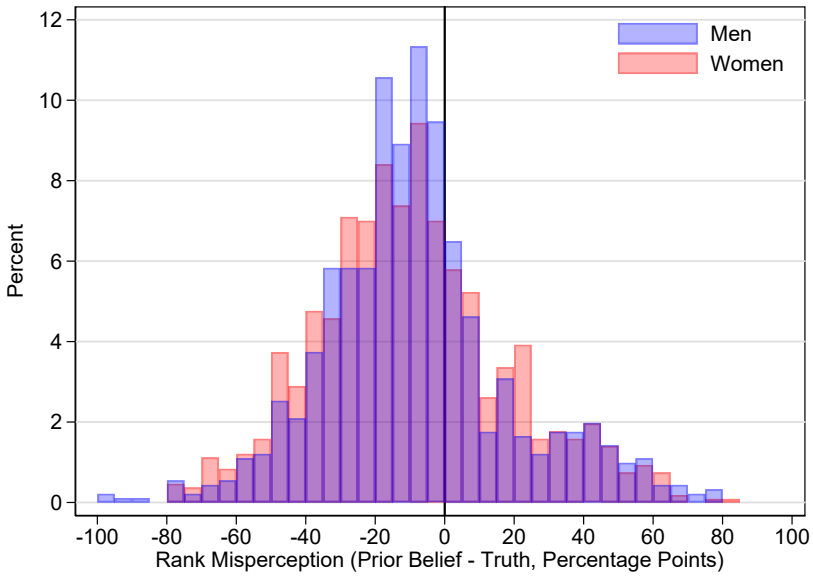
4 Results

4.1 Misperceptions about Income Ranks

Misperceptions of own household income rank are common among both women and men. Figure 1 shows the distribution of misperceptions (measured as perceived minus actual percentile) at baseline, separated by gender; the difference in the distribution of misperceptions between women and men is statistically insignificant (Kolmogorov-Smirnov test, p-value=0.126). For instance, women underestimate their rank by about 9 percentage points and men by about 10 percentage points, a difference that is small and statistically insignificant (p-value=0.411).

Next, we compare perceptions between husbands and wives. Panel (a) of Figure 2 shows a binned scatterplot of misperceptions about the income rank, with wives on the y-axis and husbands on the x-axis. If husbands and wives have similar levels of misperception regarding their households' income rank, their misperceptions would align along the 45-degree line. However, misperceptions do not perfectly align in this way, suggesting significant disagreement about income ranks between spouses. While the rank misperceptions within a household are correlated, the

Figure 1: Misperceptions of Income Ranks, by Gender



Notes: Distribution of misperceptions about income rank in the baseline survey for female (red) and male respondents (blue). Misperceptions are calculated as the difference between prior beliefs about income rank and true income rank. Positive (negative) differences correspond to overestimation (underestimation) of own income rank. Data from baseline, i.e., before the respondent (or their spouse) actually received any information ($n = 1,978$).

correlation is far from perfect ($\rho = 0.55$). In other words, husbands and wives tend to harbor rather different misperceptions.

A potential concern is that the differences in misperceptions about *relative* income are a mechanic result from disagreements about the *absolute* income. To address this concern, panel (b) of Figure 2 shows a binned scatterplot of the stated absolute household income for wives (y-axis) and their husbands (x-axis). In contrast to misperceptions of relative income, the stated absolute incomes line up almost perfectly on the 45-degree line, with a correlation coefficient of $\rho = 0.95$. This suggests that spouses largely agree about their absolute income, and that misperceptions about relative income can not be attributed to disagreement about absolute income.

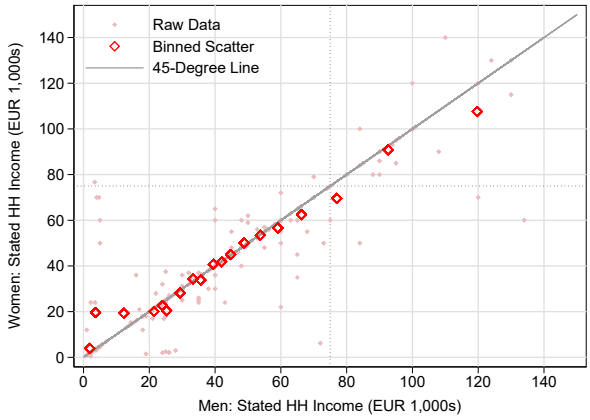
4.2 Direct and Indirect Effects of Information on Posterior Beliefs

Table 1 shows the direct and indirect effects of information provision on beliefs within households from estimating regression equation (1). The top-panel estimates in columns (1) and (2) illustrate that information about income ranks moves perceptions one year later, and shows that there is substantial diffusion of information within the household. The learning rate for direct information

Figure 2: Misperceptions of Income Ranks and Stated Household Income within Households

(a) Misperceptions

(b) Stated Household Income



Notes: **Panel (a)** shows the correlation between misperceptions about the income rank of women and men (within the household), and **panel (b)** shows the correlation of the stated household income of women and men (within the household). Misperceptions are calculated as the difference between prior beliefs about income rank and true income rank. Stated household income is the yearly gross household income measured in 1,000 Euros. Both figures show scatter plots of the raw data (light red) and binned scatterplots (red diamonds). For the binned scatterplot, we group the variables on the x-axis into 20 equally-sized bins and calculate the mean of the x and y variable within each bin. Both figures use data from the baseline survey, and we restrict the sample to 2-person, mixed-gender households ($n = 1,132$).

is 0.16 (p-value < 0.001, column (1)), i.e. for each percentage point shock in the information given directly to the respondent, the perceived income rank a year later is higher by about 0.16 percentage points. This direct learning rate does not differ between women and men (0.17 vs. 0.16 percentage points, from columns (3) and (5)). Column (2) shows the indirect learning effects in the pooled sample (women *and* men). The coefficient of 0.11 implies that for each percentage point shock in the information given to a respondent's spouse, the posterior belief of the respondent a year later is 0.11 percentage points closer to the actual ranks. The indirect effect of information pass-through also illustrates the importance of accounting for information diffusion within households – the coefficient estimate of 0.195 for the direct learning rate suggests that we underestimate the direct impact substantially if the indirect effects are not taken into account.

Figure 3 presents coefficient plots of our main result (see also Table 1, columns (4) and (6)): the indirect information diffusion effect is entirely driven by women whose husbands directly received information about the true income ranks of the household. Panel (a) of Figure 3 reveals that the coefficient for direct learning is 0.22 percentage points for women and 0.16 percentage points for men. That is, for each percentage point that we correct a respondent's misperception by directly

Table 1: Direct and Indirect Effects of Information Provision on Beliefs

	Posterior Belief 2018					
	(1) Pooled	(2) Pooled	(3) Women	(4) Women	(5) Men	(6) Men
Income Rank: Treatment*(Feedback - Prior)	0.164	0.195	0.170	0.219	0.162	0.159
	(0.034)	(0.037)	(0.046)	(0.049)	(0.052)	(0.056)
Income Rank: Indirect Treatment*(Feedback - Prior)		0.110		0.191		-0.009
		(0.054)		(0.074)		(0.079)
Observations	1,978	1,978	1,070	1,070	908	908
	Prior Belief 2017					
	(1) Pooled	(2) Pooled	(3) Women	(4) Women	(5) Men	(6) Men
Income Rank: Treatment*(Feedback - Prior)	-0.005	-0.015	0.030	0.014	-0.054	-0.060
	(0.025)	(0.030)	(0.029)	(0.031)	(0.042)	(0.049)
Income Rank: Indirect Treatment*(Feedback - Prior)		-0.033		-0.031		-0.020
		(0.030)		(0.035)		(0.046)
Observations	1,978	1,978	1,070	1,070	908	908

Notes: OLS regressions estimating the direct and indirect effects of information provision on beliefs for women and men. The top panel shows the main result (posterior beliefs), and the bottom panel shows a falsification test using prior beliefs. Standard errors are clustered at the household level, and the sample is restricted to single households and households with two mixed-gender adult partners. The direct learning rate – Income Rank: Treatment*(Information - Prior) – corresponds to the pass-through of information on true income rank within a household, i.e., the effect of providing direct information to a respondent on their beliefs about income rank one year after the intervention. Correspondingly, the indirect learning rate – Income Rank: Indirect Treatment*(Information - Prior) – is the effect of providing indirect information through a respondent's partner on a respondent's posterior beliefs. Regressions control for respondent's income, the number of household members, the prior belief about the income rank, the change in the true income rank between the two surveys, an indicator for beliefs about national income rank, and the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party, and East Germany.

providing them with information on the actual income ranks, this respondent updated their posterior belief by between 0.16 and 0.22 percentage points. Importantly, the difference between these two estimates (0.16 and 0.22) is not only small but also statistically insignificant (p-value=0.391).[5] The observed direct learning rates are sizable, considering that we measure posteriors about a year later. Generally, the learning rate should be lower than the perfect pass-through rate (i.e., $\alpha < 1$), even if measured immediately after the information provision. Firstly, from a Bayesian perspective, respondents form posterior beliefs by taking a weighted average between the signal provided to them and their prior beliefs. Thus, if respondents find the information untrustworthy or feel very sure about their prior beliefs, they should update only partially. Moreover, evidence shows that the effect of information on beliefs can decline substantially even over the course of a few months (e.g., Cavallo, Cruces and Perez-Truglia, 2017; Bottan and Perez-Truglia, 2022), so substantial dilution a full year after the information was provided should be expected.

While there is no difference in how men and women treat information that was revealed to them directly, the information provided to their spouses generates a different picture. When a wife received the information about the actual income ranks through her husband, the effect on her belief about income rank one year later was substantial (0.19 percentage points, p-value=0.010) and almost as strong as if she was directly informed. In strong contrast, when a husband was not directly informed about the true household income rank but his wife was, he did not adjust his beliefs one year later (-0.01 percentage points, p-value=0.906). The difference in indirect learning rates between wives and husbands is both sizable (0.19 vs. -0.01) and statistically significant (p-value=0.040).[6]

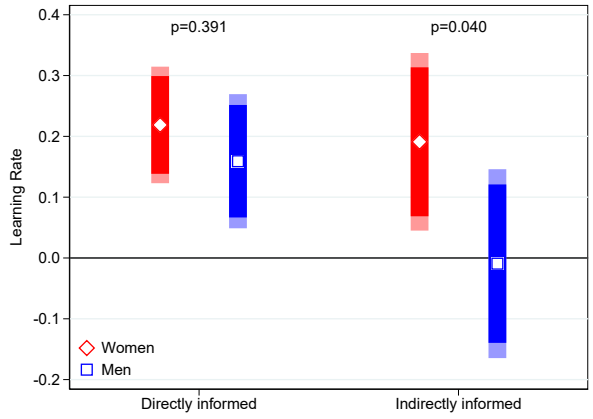
In panel (b) of Figure 3, we test the robustness of these results through a falsification test. We measure the effect of direct and indirect information provision on prior beliefs about household income rank. Given that we elicited these beliefs before the information experiment, we expect to observe no effect of the information on these prior beliefs. This is exactly what we find: the direct and indirect placebo learning rate is close to zero, statistically insignificant, and precisely estimated in all specifications (see also bottom panel of Table 1). In the Appendix, we further show that our results are robust to (i) not pooling the beliefs about national and global income ranks (Appendix Table A8), (ii) using the full sample (Appendix Table A9), and (iii) focusing only on

[5]To test for the difference in learning rates across gender, we present estimates from interacting all relevant variables with gender in Appendix Table A7.

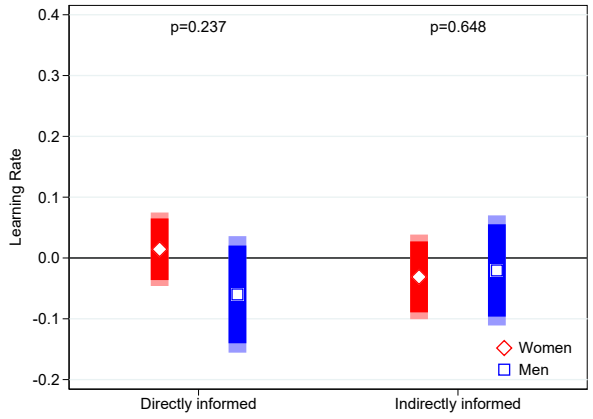
[6]Consistent with these results, we find that direct information raises confidence in belief statements for both women and men, and some indication that receiving indirect information has a stronger impact on women's confidence in belief statement compared to men (see Appendix Table A12).

Figure 3: Direct and Indirect Learning from the Information Shocks

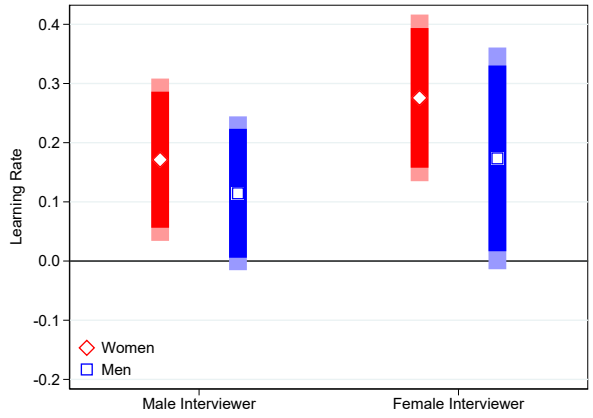
(a) Posterior Beliefs



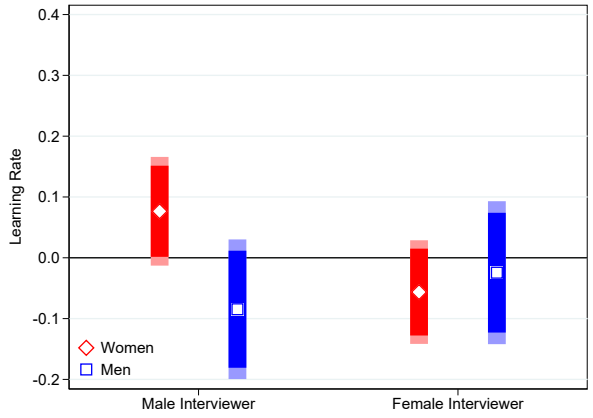
(b) Falsification Test: Prior Beliefs



(c) Direct Learning – Interviewer Gender



(d) Falsification Test: Interviewer Gender



Notes: Coefficient plots of learning rates from OLS regressions estimating the effect of information provision on beliefs about income rank as outlined in equation (1) in Section 3. The sample is restricted to single and two-person, mixed-gender households, and standard errors are clustered at the household level. Bands around coefficient estimates indicate 90% (light color) and 95% (intense color) confidence intervals. **Panel (a)** shows the effect of providing direct information to a respondent (α^{direct}) or indirect information through a respondent's partner ($\alpha^{indirect}$) on this respondent's beliefs about income rank one year after the intervention (posteriors). We estimate (α^{direct}) and ($\alpha^{indirect}$) separately for women, displayed in red, and men, displayed in blue. **Panel (b)** shows a falsification test estimating equation (1) using beliefs about income rank in the same year (prior beliefs). **Panel (c)** shows the effect of providing direct information to a respondent (α^{direct}) on their beliefs about income rank one year after the intervention (posteriors) by interviewer gender. **Panel (d)** shows a falsification test estimating the direct effect of providing direct information to a respondent (α^{direct}) on their beliefs about income rank in the same year (prior beliefs) by interviewer gender.

2-person households (Appendix Table A10).

While the presented evidence consistently points to pronounced gender differences in information diffusion, a question is whether these differences are a specific feature of the household context or whether they occur more generally. To examine this, we use the fact that the information treatment was delivered by a female or male interviewer. The interviewer read out the information and showed the respondent a customized graph on their tablet visualizing the information treatment (as discussed in Section 2 above). As interviewers are randomly assigned to households, it is also random whether households are interviewed by a male or female interviewer. Further, the gender composition of interviewers is quite balanced (55% male vs. 45% female). Focusing on direct information provision, we provide suggestive evidence that there is no difference in the reaction of men: they update their beliefs in a similar fashion regardless of whether the interviewer is female or male (0.17 vs. 0.11, see panel (c) of Figure 3 and Appendix Table A11). The difference between the two coefficients is small and insignificant (p-value=0.696). This suggests that our findings are more likely the result of within-household dynamics than of a more general phenomenon where men neglect to incorporate information they receive from any women.[7]

Our preferred interpretation of these findings is that wives are more likely to incorporate the information shared by their husbands than husbands are to incorporate the information shared by their wives. This interpretation is consistent with the findings from Conlon et al. (2022), who designed a laboratory experiment to precisely disentangle this mechanism. In our field setting, it is more difficult to control and observe how household members share information, so it is more challenging to rule out alternative stories. However, we provide some suggestive evidence against alternative channels below.

Given the long time span between the baseline and the follow-up surveys, one potential concern is that women may have obtained the information about income ranks not from their husbands, but from other sources. To address this concern, we leverage data on a question from the follow-up survey, in which we asked respondents whether they sought information on their own in-between the baseline and follow-up surveys. The results are presented in Figure 4, which focuses on 2-person households. Panel (a) of Figure 4 shows that only a small share of respondents reported having looked for rank information on their own (5%). Also, if anything, this share is

[7] For the sake of completeness, panel (d) of Figure 3 shows the corresponding falsification exercise to estimate the direct effect on prior beliefs. All coefficients are, as expected, close to zero. Despite being small, one of the coefficients (for female respondents with a male interviewer) is marginally statistically significant (p-value=0.093). However, given the large number of falsification tests conducted in this study, we should expect some significant coefficients.

lower for women than for men (2% vs. 7%, difference p-value=0.002). Thus, seeking information from other sources is unlikely to play a significant role in explaining the gender differences in information diffusion.

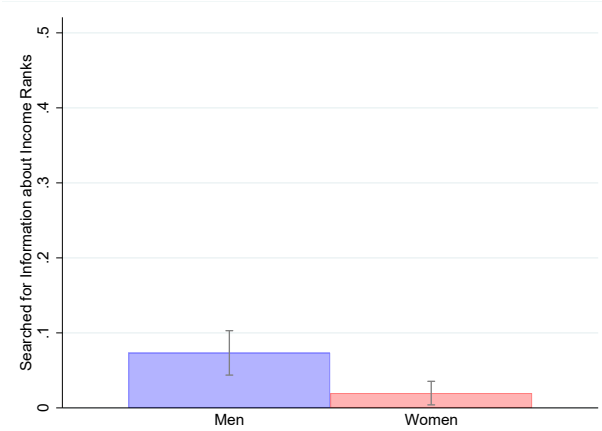
A natural channel that could help explain our findings is gender differences in communication patterns. For example, if a wife does not communicate the information, her husband would be unable to learn from her. Although we cannot completely exclude this channel, there is substantial evidence against its significance. The most direct evidence uses a question from the follow-up survey that asked all respondents who directly received information (in the baseline survey) whether they could recall having shared the income rank information with other household members. These data are, of course, merely a proxy for information sharing: Most importantly, responses are likely to be subject to substantial recall bias. When individuals are asked whether they did something a year ago, their ability to recall these events is highly imperfect. Hence, we expect these responses to systematically under-estimate the share of individuals who respond affirmatively (see e.g., Schacter, 1999; Bound, Brown and Mathiowetz, 2001). In addition to the recall bias, respondents may have been reluctant to admit sharing the information because the interview protocol was rather strict in preventing communication *during* the interview, so respondents may have worried that they were also not supposed to share the information after the interview. In addition, sharing the experimental information is not a necessary condition for social learning as spouses may discuss the issue more generally and, for example, share their (updated) beliefs. With those caveats in mind, panel (b) of Figure 4 shows that a non-negligible share of respondents said they shared the information within the household. Most importantly, we find no evidence that wives and husbands differ in the propensity of having shared the information: 21% vs. 22% (p-value=0.899, test of proportions).

Alternatively, men may be more likely to share the information with their spouses because they are more interested in the topic of income ranks or, more generally, in financial matters. On the contrary, we provide two pieces of evidence that men and women have similar levels of interest in relative income information. First, we leverage data on prior beliefs and document that the difference by gender in the average misperception is small and insignificant (husbands 21.2 vs. wives 22.4, p-value=0.239). This evidence suggests that, prior to our baseline survey, husbands and wives had acquired similar levels of information about income ranks.

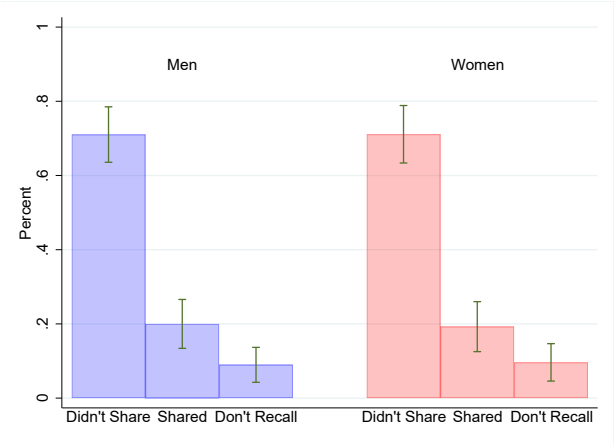
Second, using data from the follow-up survey on the willingness to pay for information indicate no difference in the interest in information about relative income. The average WTP for

Figure 4: Information Search, Information Sharing and Willingness to Pay for Information

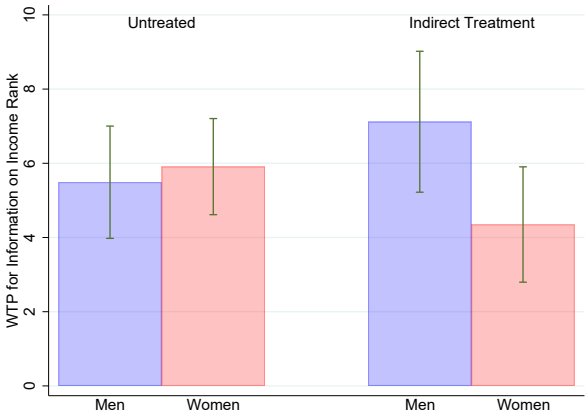
(a) Information Search



(b) Information Sharing w/in Households



(c) WTP for Information



Notes: **Panel (a)** shows the share of women and men in 2-person households who said they searched for information about income ranks after the baseline survey ($n = 604$, test of proportions, p-value=0.002). **Panel (b)** shows the fraction of treated women and men in 2-person households who (i) did not share their baseline rank information within their household after the baseline survey, (ii) shared this information, and (iii) did not recall information sharing ($n = 280$, Fisher's exact test, p-value=1.000). **Panel (c)** shows the average WTP for rank information for untreated women and men in 2-person households ($n = 290$, p-value=0.607) and indirectly treated women and men ($n = 300$, p-value=0.027).

information was 6 Euro for the national and global ranks each. This is substantial given that the maximum WTP is 10 Euro, and is also high compared to other studies that elicit WTP for other types of information (e.g., Khattak, Yim and Prokopy, 2003; Angulo, Gil and Tamburo, 2005; Allcott and Kessler, 2019; Fuster et al., 2022).[8] Part of this demand for information may be introduced artificially through our experiment.[9] However, there are reasons to believe that subjects can be genuinely interested in the topic due to its instrumental value. [10]

We also find no evidence that men are more interested than women in the information on relative income. Panel (c) of Figure 4 compares the average WTP for information between men and women. Looking at respondents in untreated households (i.e., households in which nobody received information), we see that the willingness to pay (WTP) for information does not differ much between women and men (5.9 Euro vs. 5.5 Euro, p-value=0.607). In turn, if we look at uninformed respondents in households with one informed member, we see that uninformed women have a significantly lower WTP than uninformed men (4.4 Euro vs. 7.1 Euro, p-value= 0.027). The lower WTP for women in households in which only their husbands were informed suggests that they may be less interested in the information because they already received it from their husbands.

5 Conclusions

Using a representative sample of German households in a naturally-occurring context, we document significant gender differences in information diffusion. Women adjust their beliefs when they receive information directly, and they also update their beliefs when the information is only given to their husbands. Men also adjust their beliefs when they are informed directly, but do not change their beliefs when the information is given only to their wives. This behavior cannot be traced to differences in information-sharing and acquisition patterns between women and men and also not to differential interest in income ranks. Our findings further suggest that this pattern is unique to the household setting.

[8] For a more detailed comparison, see Fehr, Mollerstrom and Perez-Truglia (2022, p. 258).

[9] In both the baseline and follow-up surveys, we incentivized the elicitation of beliefs about income ranks. While we did not tell respondents that we would elicit this information in the following year, some respondents may nevertheless expect this opportunity and thus express interest in the information.

[10] For example, individuals seek out information on incomes when it becomes publicly available due to transparency policies (Perez-Truglia, 2020). Other evidence suggests that employees are interested in learning about the salaries of their peers and that this information has a significant effect e.g. on whether to stay with a company (Card et al., 2012; Cullen and Perez-Truglia, 2023, 2022).

Our study documents gender-specific barriers to information flow within households but does not provide a comprehensive account of when and why these exist. While we do offer some insights into the boundaries and underlying reasons for the barriers, there is still important future research to be conducted. Exploring additional potential factors contributing to gendered differences in information flows, such as spousal communication patterns (see e.g., Bjorkman Nyqvist, Jayachandran and Zipfel, 2023), would be a valuable avenue for investigation. Additionally, it is necessary to extend our examination beyond the beliefs addressed in this study (e.g., to inflation expectations, effectiveness and safety of vaccines, etc.) and explore other contexts, including different developed and developing countries. Ultimately, we need to ascertain whether the gender differences in belief updating we investigate here also translate into variations in high-stakes behavior.

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Appendix – For Online Publication Only

Listen to Her: Gender Differences in Information Diffusion within the Household

Dietmar Fehr, Johanna Mollerstrom, and Ricardo Perez-Truglia

A.1 Data

This study is based on data from SOEP INNOVATION SAMPLE (soep-is.2020; 10.5684/soep.is.2020), which is available for the research community as scientific use file (SUF). To get access to the SUF you have to sign a data distribution contract with the SOEP. For more details, see the website of the Research Data Center SOEP by visiting the following URL: https://www.diw.de/en/diw_01.c.601584.en/data_access.html (for questions, you can reach out to soepmail@diw.de). Once your contract is approved you will receive a link to an online form to request the data. Here you request the latest SOEP Innovation Sample 20xx (2020 as of the time this README file was written). You will then receive an individualized download link for the SUF (and passwords for the data on your mobile phone).

A.2 Replication Package

The replication package is available at: https://doi.org/10.17605/OSF.IO/3A9C5

Setup: We used STATA (version 17) to prepare and analyze the data and we provide two do-file to reproduce the analysis. If you have a newer version of Stata, you may want to add "version 17" at the beginning to ensure compatibility. There are some commands used in the code, "coefplot" and "estout," that do not come pre-installed with Stata. If you are connected to the Internet, you can install these two commands by entering "ssc install coefplot" and "ssc install estout" in the Stata command window.

Instructions: First, download all the data files referenced above and put all the data files (*.dta) and do-files (*.do) into the same folder. The data we use in our analysis is contained in the following data files (you may want to delete the other files that come with the data distribution).

- inno.dta: this contains the data from our tailor-made survey modules.
- p.dta: person-related information
- pgen.dta: person-related information generated from the answers in the personal questionnaire.
- pbrutto.dta: person-related information generated by the interviewers during fieldwork.
- hbrutto.dta: household-related information generated by the interviewers during fieldwork.
- h.dta: household-related information
- intv.dta: interviewer-related information

Second, we provide two do-files:

- prepare_working_file.do: this file generates the variables used in the analysis from the raw data
- Figures&Tables.do: this file creates all figures and tables in the paper and replication package

To prepare the "working file" you should first open and run the "prepare_working_file.do" in Stata. This do-file will generate the variables used in the analysis from the raw data and create a new data file that will be saved in the same folder: "working_file_intra_aux.dta." This data file will be used to produce all figures and tables in the main text and the analysis in this replication package. The Stata code for this analysis is contained in "Figures&Tables.do," which also includes comments indicating which portion of the code generates which table/figure. The mapping of the code outcome to the figures and tables in the manuscript (and appendix) is as follows. Figures are named "Fig_description," where "description" refers to the content, e.g., misperceptions_pooled_female_vs_male. Tables are named "Table_description," where "description" refers to the content, e.g., Pooled_Peer_Treatment_Gender. The programs were last run top to bottom on June 25, 2023.

A.3 Information Treatment

Appendix Figure A1 provides an example screenshot (translated from German) of the treatment providing information about actual income rank at the national level. The interviewer first read out some general information on the data sources and then told the respondent the share of people in Germany with less per-capita gross household income. In addition, the interviewer showed and explained a visualization of this information. Information about actual global income rank was presented analogously.

A.4 Attrition and Balance Checks

In the analysis, we measure the effects of treatment on beliefs about income rank one year later (i.e., in the follow-up survey). A potential concern is that the treatment may have affected the decision to participate in the follow-up survey. In Appendix Tables A1–A4, we provide further assurances that the attrition was random in the full and restricted sample. Starting with the full sample, Appendix Table A1 examines whether treatment status predicts participation in the follow-up survey. Column (1) shows that this is not the case. As it is possible that some household members are treated while others are not, we also control for the indirect treatment. The results are displayed in column (2) and indicate that it neither affects participation in the follow-up survey. In columns (3) and (4), we show the effect of treatment intensity on follow-up survey participation. Again, the coefficient estimates are small and insignificant. In Appendix Table A2, we repeat this exercise with the restricted sample. Again, we find no evidence that treatment status predicts participation in the follow-up survey except for indirectly informed respondents about the global income rank (i.e., respondents who indirectly learn that they are richer than thought globally). We repeat this exercise for treatment effects for women and men separately. We do this in the full sample (Appendix Table A3) and the restricted sample (Appendix Table A4). Again, there is no indication that the treatments had an effect on participation in the follow-up survey. Finally, we show that observable characteristics are balanced across treatment and control groups for men and women together, and men and women separately. Appendix Table A5 shows the result for the full sample and Appendix Table A6 shows the result for the restricted sample.

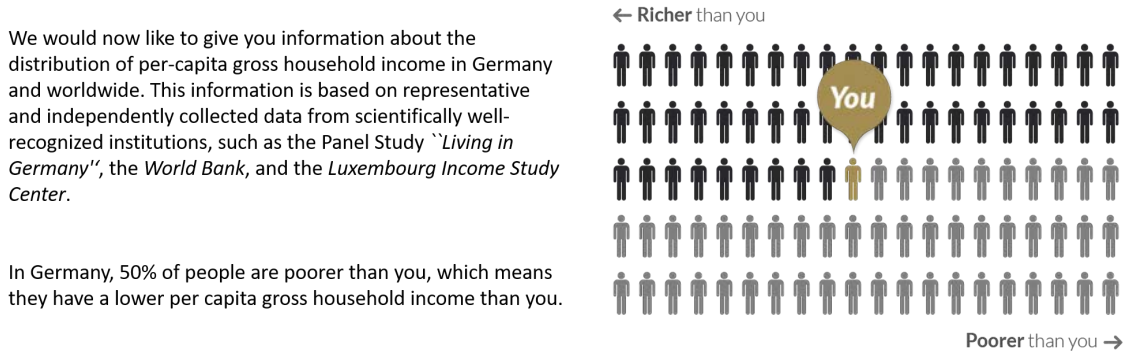
A.5 Alternative Specifications of the Main Result

In Appendix Table A7, we estimate the direct and indirect learning rate in a model in which we interact all relevant variables with respondent gender. The results show that the difference in indirect learning is significant between women and men. In Section 3, we outlined several sample restrictions. We now present evidence that our results do not depend on any of these choices to restrict the study sample. Appendix Tables A8–A10 show these alternative specifications for our main result. First, in Appendix Table A8, we show that our results do not depend on pooling beliefs. We see a positive indirect learning rate of women for national and global income rank information. In contrast, the indirect learning rate for men is, in both cases, close to zero. Second, Appendix Table A9 replicates the results from our baseline specification (Table 1) for the full sample. The information pass-through from husbands to wives is larger than in our baseline specification (0.24 vs. 0.19) and significant. Note that the likelihood of someone receiving information indirectly increases in households larger than two. Therefore, assignment to $T^{indirect}$ (indirect treatment) is random only after conditioning on the number of respondents in the household who could have been assigned to the direct information treatment. Third, in Appendix Table A10, we show that our results also hold if we restrict the sample to 2-person households only. Though the effect is smaller than in our baseline specification (0.15 vs. 0.19) due to the much smaller sample size.

A.6 Additional Results

Appendix Tables A11–A13 present additional results. In particular, in Appendix Table A11, we show that men react to directly delivered information from female and male interviewers in the same way. Appendix Table A12 shows that receiving information directly increases confidence in the posteriors for both women and men and that receiving information indirectly only has some effects on women but not men. Appendix Table A13 shows gender differences in the willingness to pay for information in the control and treatment group as well as in the group with indirect information.

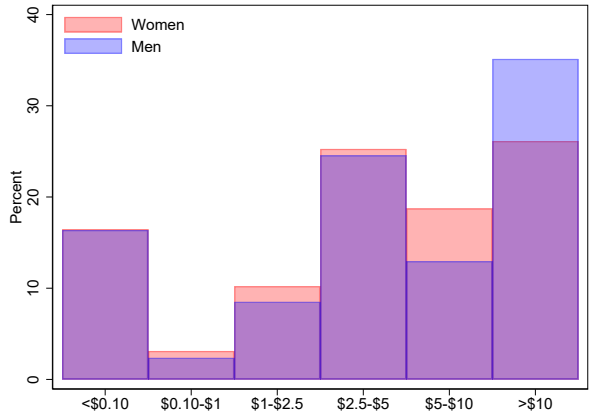
Figure A1: Screenshot of a Sample of the Information Treatment



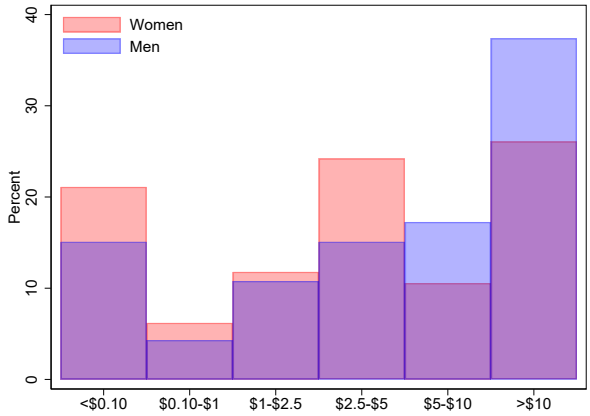
Notes: Visualization of the information treatment providing information about actual income rank at the national level (information about actual global income rank was presented analogously). Translated from German. Respondents received first some general information on the data sources and then learned the share of people in Germany with less per-capita gross household income. The information was illustrated using customized graphs that indicated the relative position to make it easier to understand and digest.

Figure A2: Distribution of WTP for Information

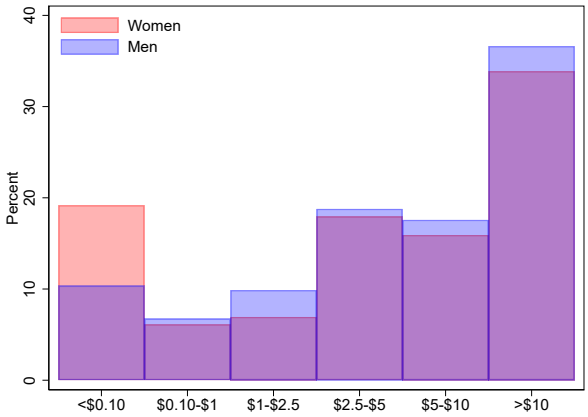
(a) Control Treatment



(b) Indirect Treatment



(c) Direct Treatment



Notes: Histograms of cutoff values in the WTP elicitation task. **Panel (a)** displays the distribution of cutoff values in the control treatment (respondents neither received direct nor indirect information). **Panel (b)** shows the distribution of cutoff values in the indirect treatment (only one respondent in a household received information). **Panel (c)** shows the distribution of cutoff values in the direct treatment (all respondents in a household received direct information).

Table A1: Effects of Information Provision on Response Rate to the Follow-Up Survey (Selective Attrition) – Full Sample

	Responded to Follow-Up Survey			
	(1)	(2)	(3)	(4)
Treatment	-0.020	-0.009		
	(0.020)	(0.026)		
Indirect Treatment		0.031		
		(0.032)		
National Rank: Treatment*(Feedback - Prior)			-0.022	-0.068
			(0.097)	(0.113)
National Rank: Indirect Treatment*(Feedback - Prior)				-0.133
				(0.146)
Global Rank: Treatment*(Feedback - Prior)			-0.151	-0.095
			(0.098)	(0.120)
Global Rank: Indirect Treatment*(Feedback - Prior)				0.151
				(0.134)
Observations	1,392	1,392	1,364	1,364

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions with standard errors clustered at the household level in parentheses using data from both surveys. The dependent variable is an indicator whether a respondent took part in the second survey one year later. Analysis conditional on number of household members and HH gross income.

Table A2: Effects of Information Provision on Response Rate to the Follow-Up Survey (Selective Attrition) – Restricted Sample

	Responded to Follow-Up Survey			
	(1)	(2)	(3)	(4)
Treatment	-0.021	-0.007		
	(0.021)	(0.027)		
Indirect Treatment		0.047		
		(0.033)		
National Rank: Treatment*(Feedback - Prior)			0.003	-0.070
			(0.099)	(0.118)
National Rank: Indirect Treatment*(Feedback - Prior)				-0.236
				(0.147)
Global Rank: Treatment*(Feedback - Prior)			-0.139	-0.057
			(0.101)	(0.125)
Global Rank: Indirect Treatment*(Feedback - Prior)				0.250**
				(0.127)
Observations	1,187	1,187	1,164	1,164

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions with standard errors clustered at the household level in parentheses using data from both surveys. The dependent variable is an indicator whether a respondent took part in the second survey one year later. Analysis conditional on number of household members and HH gross income. Sample restricted to single and two-person, mixed-gender households.

Table A3: Effects of Information Provision on Response Rate to the Follow-Up Survey (Treatment Effect on Attrition) – Full Sample

	All	Women	Men
	(1)	(2)	(3)
National Rank: Treatment*(Feedback - Prior)	-0.013	-0.154	0.127
	(0.096)	(0.142)	(0.146)
Global Rank: Treatment*(Feedback - Prior)	-0.134	-0.091	-0.159
	(0.100)	(0.133)	(0.166)
P-value Nat.=Glob.	0.506	0.806	0.329
Observations	1,364	745	619

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the effect of treatment status on participation in the second survey using data from baseline survey. Standard errors clustered at the household level in parentheses. The dependent variable is an indicator whether a respondent took part in the second survey one year later. The control variables used in the analysis are the prior misperceptions about the national and global income rank, and the following demographic characteristics: age and dummies for gender, education, disability, unemployment, retirement, self-employment, political party and East Germany.

Table A4: Effects of Information Provision on Response Rate to the Follow-Up Survey (Treatment Effect on Attrition) – Restricted Sample

	All	Women	Men
	(1)	(2)	(3)
National Rank: Treatment*(Feedback - Prior)	0.006	-0.170	0.172
	(0.100)	(0.155)	(0.148)
Global Rank: Treatment*(Feedback - Prior)	-0.144	-0.085	-0.206
	(0.101)	(0.146)	(0.168)
P-value Nat.=Glob.	0.419	0.762	0.201
Observations	1,164	640	524

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the effect of treatment status on participation in the second survey using data from baseline survey. Standard errors clustered at the household level in parentheses. The dependent variable is an indicator whether a respondent took part in the second survey one year later. The control variables used in the analysis are the prior misperceptions about the national and global income rank, and the following demographic characteristics: age and dummies for gender, education, disability, unemployment, retirement, self-employment, political party and East Germany. Sample restricted to single and two-person, mixed-gender households.

Table A5: Randomization Balance – Full Sample

	All			Women			Men		
	(1) Control	(2) Treat	(3) p-val	(4) Control	(5) Treat	(6) p-val	(7) Control	(8) Treat	(9) p-val
HH Gross Income (EUR 1,000s)	43.64	43.54	0.97	42.80	39.76	0.33	44.64	48.15	0.51
	(1.91)	(2.28)		(2.37)	(2.00)		(3.10)	(4.42)	
No. of Household Members	2.34	2.28	0.35	2.30	2.25	0.54	2.38	2.31	0.48
	(0.04)	(0.05)		(0.06)	(0.06)		(0.07)	(0.07)	
Age	54.58	56.44	0.06	55.28	56.11	0.53	53.76	56.83	0.04
	(0.71)	(0.69)		(0.97)	(0.91)		(1.02)	(1.06)	
Female (=1)	0.54	0.55	0.79						
	(0.02)	(0.02)							
Education: upper secondary (=1)	0.63	0.60	0.23	0.62	0.63	0.76	0.64	0.56	0.03
	(0.02)	(0.02)		(0.02)	(0.02)		(0.03)	(0.03)	
Education: college (=1)	0.22	0.23	0.61	0.19	0.17	0.63	0.26	0.30	0.23
	(0.02)	(0.02)		(0.02)	(0.02)		(0.02)	(0.03)	
Disabled (=1)	0.13	0.15	0.18	0.12	0.13	0.69	0.14	0.18	0.13
	(0.01)	(0.01)		(0.02)	(0.02)		(0.02)	(0.02)	
Unemployed (=1)	0.03	0.04	0.50	0.03	0.04	0.52	0.04	0.04	0.76
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	
Self employed (=1)	0.07	0.05	0.21	0.05	0.04	0.89	0.09	0.06	0.13
	(0.01)	(0.01)		(0.01)	(0.01)		(0.02)	(0.01)	
Retired (=1)	0.34	0.35	0.72	0.35	0.34	0.74	0.34	0.37	0.38
	(0.02)	(0.02)		(0.02)	(0.02)		(0.03)	(0.03)	
East Germany (=1)	0.23	0.23	0.99	0.24	0.23	0.74	0.22	0.23	0.71
	(0.02)	(0.02)		(0.02)	(0.02)		(0.02)	(0.02)	
SPD Supporter (=1)	0.13	0.16	0.14	0.12	0.14	0.35	0.14	0.18	0.23
	(0.01)	(0.01)		(0.02)	(0.02)		(0.02)	(0.02)	
CDU/CSU Supporter (=1)	0.22	0.24	0.30	0.22	0.24	0.43	0.21	0.24	0.51
	(0.02)	(0.02)		(0.02)	(0.02)		(0.02)	(0.02)	
FDP Supporter (=1)	0.02	0.02	0.94	0.01	0.02	0.31	0.03	0.03	0.54
	(0.01)	(0.01)		(0.00)	(0.01)		(0.01)	(0.01)	
Gruene Supporter (=1)	0.06	0.08	0.28	0.07	0.08	0.46	0.05	0.07	0.42
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	
Linke Supporter (=1)	0.04	0.03	0.35	0.04	0.02	0.15	0.04	0.04	0.91
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	
AfD/Right Supporter (=1)	0.04	0.03	0.43	0.02	0.02	0.81	0.06	0.05	0.44
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	
Joint F-Test			0.26			0.87			0.11
Observations	705	687		383	378		322	309	

Notes: Mean and standard deviation (in parentheses) of control variables, separated for treatment and control in the baseline survey (full sample). p-val is the p-value from testing for the difference between treatment and control. Joint F-test reports the p-value from an F-test based on regressing treatment status on all controls. Columns (1) through (3) display the results for respondents in the full sample. Columns (4) through (6) separate by female respondents, and Columns (7) through (9) by male respondents. All control variables are defined as binary variables except household income, number of household members, and age.

Table A6: Randomization Balance – Restricted Sample

	All			Women			Men		
	(1) Control	(2) Treat	(3) p-val	(4) Control	(5) Treat	(6) p-val	(7) Control	(8) Treat	(9) p-val
HH Gross Income (EUR 1,000s)	45.27	45.05	0.96	42.88	39.39	0.38	48.08	51.66	0.62
	(2.46)	(3.13)		(3.00)	(2.60)		(4.03)	(6.04)	
No. of Household Members	2.12	2.07	0.41	2.09	2.03	0.52	2.16	2.11	0.61
	(0.04)	(0.05)		(0.06)	(0.07)		(0.06)	(0.07)	
Age	56.62	59.04	0.03	57.30	58.63	0.37	55.81	59.52	0.02
	(0.78)	(0.76)		(1.08)	(0.99)		(1.11)	(1.17)	
Female (=1)	0.54	0.54	0.94						
	(0.02)	(0.02)							
Education: upper secondary (=1)	0.65	0.61	0.25	0.65	0.63	0.62	0.64	0.59	0.25
	(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)	
Education: college (=1)	0.24	0.25	0.68	0.19	0.19	0.96	0.29	0.31	0.62
	(0.02)	(0.02)		(0.02)	(0.02)		(0.03)	(0.03)	
Disabled (=1)	0.13	0.15	0.30	0.13	0.12	0.92	0.14	0.19	0.12
	(0.01)	(0.02)		(0.02)	(0.02)		(0.02)	(0.03)	
Unemployed (=1)	0.03	0.03	0.59	0.03	0.04	0.40	0.03	0.02	0.83
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	
Self employed (=1)	0.06	0.06	0.99	0.04	0.06	0.23	0.09	0.07	0.31
	(0.01)	(0.01)		(0.01)	(0.01)		(0.02)	(0.02)	
Retired (=1)	0.39	0.40	0.89	0.39	0.38	0.72	0.39	0.42	0.55
	(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)	
East Germany (=1)	0.24	0.24	0.80	0.26	0.25	0.89	0.21	0.24	0.58
	(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)	
SPD Supporter (=1)	0.14	0.16	0.26	0.13	0.14	0.58	0.15	0.18	0.30
	(0.02)	(0.02)		(0.02)	(0.02)		(0.02)	(0.03)	
CDU/CSU Supporter (=1)	0.23	0.24	0.80	0.24	0.24	0.94	0.23	0.24	0.65
	(0.02)	(0.02)		(0.03)	(0.03)		(0.03)	(0.03)	
FDP Supporter (=1)	0.02	0.02	0.88	0.01	0.02	0.22	0.04	0.03	0.50
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	
Gruene Supporter (=1)	0.06	0.08	0.37	0.07	0.09	0.38	0.06	0.07	0.73
	(0.01)	(0.01)		(0.01)	(0.02)		(0.02)	(0.02)	
Linke Supporter (=1)	0.05	0.03	0.28	0.04	0.02	0.11	0.05	0.05	0.94
	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	
AfD/Right Supporter (=1)	0.04	0.03	0.33	0.03	0.02	0.87	0.06	0.04	0.27
	(0.01)	(0.01)		(0.01)	(0.01)		(0.02)	(0.01)	
Joint F-Test			0.38			0.81			0.26
Observations	510	479		276	258		234	221	

Notes: Mean and standard deviation (in parentheses) of control variables, separated for treatment and control in the baseline survey (restricted sample – single and two-person, mixed-gender households). p-val is the p-value from testing for the difference between treatment and control. Joint F-test reports the p-value from an F-test based on regressing treatment status on all controls. Columns (1) through (3) display the results for respondents in the restricted sample. Columns (4) through (6) separate by female respondents, and columns (7) through (9) by male respondents. All control variables are defined as binary variables except household income, number of household members, and age.

Table A7: Alternative Specification – Model with Interactions

	Posterior Belief 2018		Prior Belief 2017	
	(1)	(2)	(3)	(4)
Income Rank: Treatment*(Feedback - Prior)	0.160***	0.162***	-0.058	-0.065
	(0.051)	(0.055)	(0.044)	(0.051)
Female=1 × Income Rank: Treatment*(Feedback - Prior)	0.006	0.063	0.089	0.074
	(0.069)	(0.073)	(0.054)	(0.059)
Income Rank: Indirect Treatment*(Feedback - Prior)		0.004		-0.025
		(0.078)		(0.047)
Female=1 × Income Rank: Indirect Treatment*(Feedback - Prior)		0.221**		-0.063
		(0.107)		(0.060)
Observations	1,978	1,978	1,978	1,978

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the direct and indirect effects of information provision on beliefs using a specification that interacts all relevant variables with an indicator for gender. Columns (1) and (2) show the effect on posterior beliefs, and columns (3) and (4) show a falsification test using prior beliefs. Standard errors are clustered at the household level, and the sample is restricted to single households and households with two mixed-gender adult partners. The direct learning rate corresponds to the pass-through of information on true income rank within a household, i.e., the effect of providing direct information to a respondent on their beliefs about income rank one year after the intervention – Income Rank: Treatment*(Information - Prior). Correspondingly, the indirect learning rate is the effect of providing indirect information through a respondent's partner on a respondent's posterior beliefs – Income Rank: Indirect Treatment*(Information - Prior). Regressions control for respondent's income, the number of household members, the prior belief about the income rank, the change in the true income rank between the two surveys, an indicator for beliefs about national income rank, and the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party, and East Germany.

Table A8: Alternative Specification – Separated by National and Global Income Ranks

	Posterior Belief 2018 - Women				Posterior Belief 2018 - Men			
	(1) National	(2) National	(3) Global	(4) Global	(5) National	(6) National	(7) Global	(8) Global
National Rank: Treatment*(Feedback - Prior)	0.108*	0.186***			0.207***	0.207***		
	(0.056)	(0.059)			(0.058)	(0.064)		
National Rank: Indirect Treatment*(Feedback - Prior)		0.291***				-0.001		
		(0.085)				(0.076)		
Global Rank: Treatment*(Feedback - Prior)			0.144***	0.206***			0.089	0.098
			(0.053)	(0.057)			(0.068)	(0.074)
Global Rank: Indirect Treatment*(Feedback - Prior)				0.204**				0.023
				(0.089)				(0.089)
Observations	614	614	606	606	523	523	516	516
	Prior Belief 2017 - Women				Prior Belief 2017 - Men			
	(1) National	(2) National	(3) Global	(4) Global	(5) National	(6) National	(7) Global	(8) Global
National Rank: Treatment*(Feedback - Prior)	0.025	0.010			0.007	-0.017		
	(0.032)	(0.038)			(0.038)	(0.044)		
National Rank: Indirect Treatment*(Feedback - Prior)		-0.052				-0.076		
		(0.049)				(0.047)		
Global Rank: Treatment*(Feedback - Prior)			-0.008	0.003			-0.046	-0.048
			(0.028)	(0.026)			(0.042)	(0.048)
Global Rank: Indirect Treatment*(Feedback - Prior)				0.038				-0.004
				(0.052)				(0.046)
Observations	656	656	613	613	566	566	521	521

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the direct and indirect effects of information provision on national and global income ranks on beliefs for women and men. The top panel shows the main result (posterior beliefs), and the bottom panel shows a falsification test using prior beliefs. Standard errors are clustered at the household level, and the sample includes all households. The direct learning rate corresponds to the pass-through of information on true income rank within a household, i.e., the effect of providing direct information to a respondent on their beliefs about income rank one year after the intervention – National (Global) Rank: Treatment*(Information - Prior). Correspondingly, the indirect learning rate is the effect of providing indirect information through a respondent's partner on a respondent's posterior beliefs – National (Global) Rank: Indirect Treatment*(Information - Prior). Regressions control for respondent's income, the number of household members, the prior belief about the income rank, the change in the true income rank between the two surveys, and the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party, and East Germany.

Table A9: Alternative Specification – Full Sample

	Posterior Belief 2018					
	(1) Pooled	(2) Pooled	(3) Women	(4) Women	(5) Men	(6) Men
Income Rank: Treatment*(Feedback - Prior)	0.138***	0.178***	0.129***	0.195***	0.156***	0.156***
	(0.033)	(0.035)	(0.044)	(0.046)	(0.049)	(0.053)
Income Rank: Indirect Treatment*(Feedback - Prior)		0.128**		0.235***		0.001
		(0.050)		(0.071)		(0.069)
Observations	2,259	2,259	1,220	1,220	1,039	1,039
	Prior Belief 2017					
	(1) Pooled	(2) Pooled	(3) Women	(4) Women	(5) Men	(6) Men
Income Rank: Treatment*(Feedback - Prior)	-0.005	-0.008	0.018	0.022	-0.048	-0.060
	(0.024)	(0.028)	(0.027)	(0.029)	(0.040)	(0.047)
Income Rank: Indirect Treatment*(Feedback - Prior)		-0.009		0.015		-0.036
		(0.032)		(0.042)		(0.046)
Observations	2,259	2,259	1,220	1,220	1,039	1,039

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the direct and indirect effects of information provision on beliefs for women and men. The top panel shows the main result (posterior beliefs), and the bottom panel shows a falsification test using prior beliefs. Standard errors are clustered at the household level, and the sample includes all households. The direct learning rate corresponds to the pass-through of information on true income rank within a household, i.e., the effect of providing direct information to a respondent on their beliefs about income rank one year after the intervention – Income Rank: Treatment*(Information - Prior). Correspondingly, the indirect learning rate is the effect of providing indirect information through a respondent's partner on a respondent's posterior beliefs – Income Rank: Indirect Treatment*(Information - Prior). Regressions control for respondent's income, the number of household members, the prior belief about the income rank, the change in the true income rank between the two surveys, an indicator for beliefs about national income rank, and the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party, and East Germany.

Table A10: Alternative Specification – 2-Person Households

	Posterior Belief 2018					
	(1) Pooled	(2) Pooled	(3) Women	(4) Women	(5) Men	(6) Men
Income Rank: Treatment*(Feedback - Prior)	0.115***	0.140***	0.122**	0.197***	0.102	0.072
	(0.043)	(0.053)	(0.059)	(0.071)	(0.064)	(0.075)
Income Rank: Indirect Treatment*(Feedback - Prior)		0.050		0.148*		-0.061
		(0.065)		(0.088)		(0.092)
Observations	1,203	1,203	608	608	595	595
	Prior Belief 2017					
	(1) Pooled	(2) Pooled	(3) Women	(4) Women	(5) Men	(6) Men
Income Rank: Treatment*(Feedback - Prior)	0.022	-0.000	0.048	0.047	-0.035	-0.059
	(0.031)	(0.046)	(0.036)	(0.047)	(0.050)	(0.065)
Income Rank: Indirect Treatment*(Feedback - Prior)		-0.044		-0.002		-0.050
		(0.042)		(0.046)		(0.059)
Observations	1,203	1,203	608	608	595	595

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the direct and indirect effects of information provision on beliefs for women and men. The top panel shows the main result (posterior beliefs), and the bottom panel shows a falsification test using prior beliefs. Standard errors are clustered at the household level, and the sample is restricted to households with two mixed-gender adult partners. The direct learning rate corresponds to the pass-through of information on true income rank within a household, i.e., the effect of providing direct information to a respondent on their beliefs about income rank one year after the intervention – Income Rank: Treatment*(Information - Prior). Correspondingly, the indirect learning rate is the effect of providing indirect information through a respondent's partner on a respondent's posterior beliefs – Income Rank: Indirect Treatment*(Information - Prior). Regressions control for respondent's income, the number of household members, the prior belief about the income rank, the change in the true income rank between the two surveys, an indicator for beliefs about national income rank, and the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party, and East Germany.

Table A11: Direct and Indirect Effect of Information Provision on Beliefs for Women and Men by Interviewer Gender

	Posterior Belief 2018 - Women		Posterior Belief 2018 - Men	
	(1) Male Interviewer	(2) Female Interviewer	(3) Male Interviewer	(4) Female Interviewer
Income Rank: Treatment*(Feedback - Prior)	0.171**	0.276***	0.114*	0.173*
	(0.070)	(0.071)	(0.066)	(0.095)
Observations	485	406	426	325
	Prior Belief 2018 - Women		Prior Belief 2018 - Men	
	(1) Male Interviewer	(2) Female Interviewer	(3) Male Interviewer	(4) Female Interviewer
Income Rank: Treatment*(Feedback - Prior)	0.076*	-0.056	-0.085	-0.016
	(0.045)	(0.043)	(0.058)	(0.062)
Observations	485	406	426	325

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the direct effect of information provision on beliefs for women and men separated by interviewer gender. The top panel shows the main result (posterior beliefs), and the bottom panel shows a falsification test using prior beliefs. Standard errors are clustered at the household level, and the sample is restricted to single households and directly treated 2-person households. The direct learning rate corresponds to the pass-through of information on true income rank within a household, i.e., the effect of providing direct information to a respondent on their beliefs about income rank one year after the intervention – Income Rank: Treatment*(Information - Prior). Regressions control for respondent's income, the number of household members, the prior belief about the income rank, the change in the true income rank between the two surveys, an indicator for beliefs about national income rank, and the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party, and East Germany.

Table A12: Effects of Information Provision on Belief Certainty One Year Later

	Certainty Posterior Belief					
	(1) Pooled	(2) Pooled	(3) Women	(4) Women	(5) Men	(6) Men
Treatment	0.533***	0.669***	0.432**	0.611***	0.650***	0.730***
	(0.139)	(0.169)	(0.206)	(0.234)	(0.193)	(0.216)
Indirect Treatment		0.426*		0.577*		0.246
		(0.229)		(0.343)		(0.299)
Observations	1,983	1,983	1,074	1,074	909	909

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. OLS regressions estimating the effect of information provision on confidence in posterior beliefs. Standard errors are clustered at the household level in parentheses. The dependent variable is the confidence in stated posterior beliefs measured on a 1–10 scale emulating steps of 10 percent. "Treatment" is an indicator for treatment information on income ranks, and "Indirect Treatment" takes the value 1 if the respondent did not receive the information but another member of her household and 0 otherwise (i.e., if the respondent received the information or if none of the household members received the information). Regressions control for respondent's income, the number of household members, the prior belief about the income rank, the change in the true income rank between the two surveys, an indicator for beliefs about national income rank, and the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party and East Germany.

Table A13: Gender Differences in WTP for Information on Actual Income Ranks

	WTP For Information		
	(1) Control	(2) Treatment	(3) Indirect
Female (=1)	0.420	-0.430	-2.765**
	(0.818)	(0.904)	(1.252)
Mean WTP males	5.487***	7.110***	7.118***
	(0.772)	(0.699)	(0.969)
Observations	290	531	300

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Interval regressions estimating gender differences in willingness to pay (WTP) for information on actual income ranks for husbands and wives. "Control" refers to respondents who neither received direct nor indirect information, "Treatment" refers to directly informed respondents, and "Indirect" refers to indirectly informed respondents. Standard errors are clustered at the household level in parentheses. The dependent variable is the WTP for information, measured as the switch point from receiving information to receiving money in the list-price format. Regressions control for the following demographic characteristics: age and indicator variables for education, disability, unemployment, retirement, self-employment, political party, and East Germany.