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KEEPING UP WITH THE JANSENS: CAUSAL PEER EFFECTS ON HOUSEHOLD SPENDING, BELIEFS AND HAPPINESS

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Abstract: How does perceived relative income affect household behavior and well-being? We use a randomized information experiment in a representative panel of Dutch households to generate exogenous variation in beliefs about peers' income. Lower perceived relative standing shifts expenditure toward durable goods but we find little evidence of an increase in total spending. Higher perceived relative income increases socializing and substantially raises happiness, while lower relative income increases subsequent employment. Relative standing also shapes views about inequality and redistribution. A complementary vignette experiment provides supporting evidence on mechanisms. Overall, peer comparisons matter for behavior and well-being even when total spending responses are small.

JEL Codes: D1, D3, D6, E21

Keywords: Peer effects, randomized controlled trial, surveys

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“Never keep up with the Joneses. Bring them down to your level instead. It’s much cheaper.” Quentin Crisp

1. Introduction

Do our beliefs about how much our peers earn relative to our own earnings matter? An extensive literature suggests that they may matter along many dimensions. In development economics, the Easterlin paradox states that average happiness levels of countries are not increasing with income levels above a certain threshold and that only relative income levels within a population matter for happiness (Easterlin 1974, Deaton 2008). Similarly, in macroeconomics and finance, theoretical and empirical work has argued that “keeping up with the Joneses” effects—in which spending depends in part on how much peers are consuming, especially on conspicuous items—are strong and can account for empirical patterns like consumption inertia and asset pricing puzzles (Campbell and Cochrane 1999, 2000, Smets and Wouters 2007). In labor, experimental evidence suggests that learning that one’s peers make more money than expected tends to reduce job satisfaction and increase job search (Card et al. 2012) while in political economy, some have found that higher relative income may decrease the desire for redistribution (Cruces et al. 2013). One common challenge in quantifying peer effects is the difficulty of separating causality from correlations. In Manski’s (1993) terminology, one needs to rule out “correlated effects” and the reflection problem; our design identifies a “contextual (or exogenous) effect” of perceived peer income on own behavior.

In this paper, we provide new *causal* evidence on the role of perceived relative income standing for a range of individual choices, beliefs, and measures of well-being using a randomized controlled trial (RCT) to exogenously change respondents’ perceptions of the income of their peers. To this end, we fielded special-purpose surveys in successive waves of a representative survey of households in the Netherlands. A key feature of the experiment is that it generates measurable exogenous variation in perceived peer income and hence in respondents’ perceived relative standing. We use this variation to study spending, socializing and happiness, employment and expected income, and beliefs about equity and redistribution.

To organize these outcomes, we develop a simple framework in which relative income can affect household behavior through several distinct channels. A broad “keeping-up” channel makes the utility obtained from consumption depend on relative standing, while a conspicuous-consumption channel allows relative income to change the desired composition of spending between durable and nondurable goods. Relative standing can also affect the utility from socializing as well as utility directly. Finally, over longer horizons households may respond to their relative position by changing work and own income, thereby attenuating or amplifying the initial shock to relative standing. This framework is useful because it clarifies that a small response of total expenditure need not imply that relative income is

unimportant more generally: different spending channels can offset one another, while relative standing may simultaneously have substantial effects on social interaction, labor supply, or utility. It also distinguishes the short-run response to a change in perceived peer income, when own income is predetermined, from the longer-run response once households can adjust their own income.

We report five main findings. First, perceived relative income affects the composition of household spending but has little effect on total expenditure. Across all respondents we find no clear average response of nondurable spending, although spending on nondurables and services falls among respondents receiving bad news. At the same time, higher perceived peer income increases both durable spending and the probability of making a durable purchase, with these effects also concentrated among the bad-news group. Thus, a deterioration in relative standing generates a shift toward more visible durable goods, while the net effect on total expenditure is small over the months following the experiment. Second, higher perceived relative income increases the amount of time individuals spend socializing with peers, albeit with less statistical precision than for some of our other outcomes. Third, believing that one earns more relative to peers causally and meaningfully increases self-reported happiness. The magnitude of this effect is large even though total spending moves little, consistent with relative income affecting utility through social interactions and status as well as through consumption. Fourth, when individuals learn that their peers earn more than they thought, they become more likely to be employed in subsequent months. Expected household income also rises with perceived peer income, although the estimate is sufficiently imprecise that we cannot determine how much of the initial change in relative standing ultimately persists. Finally, higher perceived relative income makes respondents less likely to view income and wealth differences as excessive and less supportive of making incomes more equal. It also makes them more likely to view income differences as providing incentives for individual effort.

To establish these results, we fielded several survey waves in 2020. In the first wave, respondents were asked about their demographic and economic characteristics as well as questions about how much they thought their peers earned. A randomly selected subset of these households was provided two weeks later with information about the actual average income of households with similar observable characteristics, defined using age, education, and marital status. Importantly, respondents themselves had previously been asked about the incomes of the people with whom they actually associate, such as friends, neighbors, acquaintances, or coworkers. We then measure whether information about objectively similar households changes respondents' beliefs about the income of this self-defined peer group. In follow-up waves, respondents were asked additional questions about

their beliefs, choices, and outcomes, allowing us to trace the effects of experimentally induced changes in perceived relative standing over the subsequent months.

Key to our identification, therefore, is the randomized information treatment, which we show was successful in generating meaningful revisions in respondents' beliefs about their peers' income. In relying on an RCT to assess the effects of new information on beliefs and outcomes, we follow a growing literature that has applied this technique in macroeconomics (Roth and Wohlfart 2020; Cavallo et al. 2017), finance (Beutel and Weber 2026), political economy (Cruces et al. 2013; Stantcheva 2020), and labor economics (Jager et al. 2024), among others. We differ from much of this work in that the information treatment is tailored to the individual's characteristics, as in Hvidberg et al. (2023). This yields an additional testable implication from Bayesian learning, namely that the coefficients on the provided individual-specific signal and the prior belief should be related through the weight respondents place on the signal, a prediction borne out in the data. In another important methodological contribution, we estimate causal peer effects by exogenously changing perceptions about peers who potentially matter for own behavior: peers are defined by respondents themselves rather than solely by place of work, residence, or an externally imposed reference group. Although we do not observe a respondent's actual social circle, we show that information about objectively similar households systematically moves perceptions about the incomes of the people respondents themselves identify as their peers.

As is common in RCTs of information provision, the resulting estimates should be interpreted as the total causal effects of changing beliefs about relative income. Learning that one's peers earn more or less may induce several channels simultaneously, including a desire to keep up with peers, concerns about status, changes in perceived financial opportunities, or inferences about the broader income distribution. To provide complementary evidence on these mechanisms, we also fielded a separate vignette experiment on Prolific in 2026. Respondents were randomly assigned to imagine that their household income was either 20 percent above or 20 percent below that of households with whom they associate, and were then asked how they would respond along the same margins studied in the field experiment. For one randomly selected margin, respondents also provided an open-ended explanation. We use this supplementary exercise primarily to assess the direction of behavioral responses and the mechanisms respondents associate with relative standing rather than to compare magnitudes directly with the Dutch experiment.

Our first application is to spending decisions. "Keeping up with the Joneses" is an integral component of many macroeconomic (e.g., Smets and Wouters 2007; Huang et al. 2022) and finance models (e.g., Campbell and Cochrane 1999, 2000), but direct causal evidence on this mechanism is limited. Much of the existing evidence relies on aggregate or regional consumption dynamics and often

points toward a high pass-through from others' spending to individual spending decisions (e.g., Korniotis 2010). Other work documents that households exposed to richer peers or neighbors increase conspicuous spending (Bertrand and Morse 2016; Agarwal et al. 2020, 2021; Kuhn et al. 2011). Our follow-up surveys include questions about recent spending on nondurables and services as well as the extensive and intensive margins of durable purchases. This allows us to distinguish a response of total spending from a change in its composition. We find that when individuals learn that their peers earn more than they thought, durable spending rises, consistent with a conspicuous-consumption response to a deterioration in relative standing, while nondurable spending shows little average response and declines among respondents receiving bad news. Estimating total spending directly yields an elasticity of -1.98 (s.e. 1.24), with a 95 percent upper bound on a positive pass-through of 0.45; the corresponding weak-instrument-robust upper bound is 0.7. The resulting total-spending estimates therefore provide little support for a large net short-run external-habit response.

Our second set of results concerns social interaction and happiness. We provide novel, albeit tentative, evidence that as individuals perceive themselves as earning more relative to their peers, they increase the amount of time spent outside of work with friends, neighbors, and other peers. We do not find evidence that this occurs through increased participation in political or other civic organizations but instead find suggestive evidence of more participation in sports and other social clubs. To the best of our knowledge, we are the first to identify a causal link between perceived relative income and time spent socializing with peers. The supplementary vignette yields the same directional result: respondents asked to imagine that they earn less than their peers report wanting to socialize less, whereas those asked to imagine earning more report wanting to socialize more. This social margin is potentially important because peer effects need not operate solely through consumption choices.

Relative standing has an even larger effect on happiness. An extensive literature has studied the link between income and happiness following the Easterlin paradox, the fact that higher-income people within a country tend to be happier than lower-income people while average happiness within a country need not rise proportionately with income (Easterlin 1974; Deaton 2008). One possible explanation is the relative income hypothesis, according to which happiness depends on how our income compares with that of our peers, not simply on its absolute level. Some prior work documents strong correlations between relative income and happiness (e.g., Ferrer-i-Carbonell 2005; Luttmer 2005), although other work has questioned the importance of this channel (Stevenson and Wolfers 2008). Previous studies have been creative in seeking exogenous variation (e.g., Perez-Truglia 2020; Natali et al. 2018; Pischke 2011), but direct causal evidence from changes in perceived peer income is limited. We generate such variation experimentally and find that higher perceived relative income sharply increases self-reported happiness.

Our IV estimate is roughly three times the corresponding OLS estimate, and controlling for ex-post changes in spending, socializing, or beliefs about inequality reduces the effect only modestly. The supplementary vignette produces a similarly large difference in reported happiness between respondents assigned to higher and lower relative-income positions. Taken together with the small response of total spending, these findings suggest that relative income matters for utility through channels beyond desired expenditure alone.

Third, we consider how households adjust work and income in response to relative standing. Card et al. (2012) and Jager et al. (2024) find that when workers learn that their peers earn more, they engage in more job search, while Breza et al. (2018) and Cullen and Perez-Truglia (2022) document effects of peer pay information on worker morale and behavior. Evidence on actual employment outcomes is more limited. Because our data have a panel structure, we are able to assess how exogenous variation in beliefs about peers' incomes affects subsequent employment. We find an asymmetric response: among respondents who learn that their peers earn more than they thought, lower relative standing leads to a substantial increase in the probability of being employed in subsequent months. The supplementary vignette points in the same direction, with respondents assigned to the lower-relative-income scenario reporting that they would work almost three hours per week more relative to respondents assigned to the higher-relative-income scenario. This labor response matters not only as an outcome in its own right but also for interpreting the persistence of the experimentally induced change in relative standing. If a deterioration in relative standing induces additional work and raises own income, part of the initial relative-income shock will eventually be offset. Our estimate of the response of expected household income is positive but imprecise, with a confidence interval that includes both no adjustment and an approximately one-for-one response.

Finally, we consider beliefs about inequality and redistribution. Prior work using information experiments has found mixed effects of relative income information on the demand for redistribution. Cruces et al. (2013) and Hvidberg et al. (2023) find that those who receive information indicating that their relative income is lower than they thought demand more redistribution, whereas Hoy and Mager (2021) find the opposite response. Karadja et al. (2017) instead find that those receiving good news about their relative income demand less redistribution, while Fehr et al. (2022) find little effect in the context of global inequality. We find that as perceived relative income rises, individuals are less likely to think that income and wealth differences are too large and less supportive of making incomes more equal. We also provide evidence on why relative income may affect these views. Higher perceived relative income does not make respondents more likely to believe that success reflects hard work rather than luck. Instead, it makes them more likely to view income differences as providing incentives for individual effort. This distinction is consistent with the framework, in which relative standing can

affect both a household's private stake in redistribution and its perceived efficiency costs. The vignette experiment also shows a sizable effect of relative standing on perceived fairness, while the accompanying open-ended responses point to heterogeneous considerations including merit, structural differences across households, and altruistic concerns. Our experiment identifies the combined effect of these considerations rather than separately isolating them.

The paper is organized as follows. Section 2 describes how the survey and information experiment were implemented, discusses the representativeness of the sample, and presents our empirical framework. Section 3 develops a theoretical framework for organizing the different channels through which relative income can affect household behavior. Section 4 documents the effects of the information treatment on respondents' beliefs about their peers' income and establishes the exogenous variation used for identification. Section 5 presents the causal effects of perceived relative income on spending, socializing and happiness, employment and expected income, and beliefs about equity and redistribution. Section 6 concludes.

2. Data and Empirical Framework

In this section, we first describe how the survey was implemented, then discuss the representativeness of the survey and some validations of the data.

2.1 The Survey of Dutch Households

We use data from the CentER Internet panel, which is sponsored by the Dutch National Bank (DNB) and maintained by CentERdata at Tilburg University.¹ The members of the panel are recruited through face-to-face or telephone interviews. Centerdata provides respondents who are selected for participation in the panel but who do not have a computer with Internet access with the necessary equipment (for more details on the CentERdata panel, see Teppa and Vis 2012). The baseline survey is conducted annually and collects detailed information on a range of demographic and economic characteristics for a representative sample of Dutch-speaking households. In addition to the baseline survey, respondents participate in special-purpose surveys during the course of the year.

We designed a sequence of special purpose surveys. We administered these surveys and the information experiment to every panel participant aged 18 and older in January 2020. Participants in special purpose surveys are generally invited to take the survey in the first week after receiving the questionnaire. Those who do not respond receive a reminder invitation and can fill in the survey in the

¹ Respondents are not told that DNB is sponsoring/using these surveys but are told that these surveys are for research purposes.

follow-up week. Given that the survey allows for contacting respondents at a high frequency, we repeated parts of the survey in March (wave 2) and May (wave 3) 2020 to track changes in beliefs and behavior in the post-treatment period. All surveys were conducted online. Our dataset includes 2,671 adult respondents from 2,201 households.²

The first wave was split into two parts to minimize survey fatigue. The first part (fielded in the second half of January 2020) collected information about own household net income and debts as well as respondents' (prior) perceptions about the average income of their peers. Specifically, for the income (net of taxes) of their peers, participants were asked:

“How much do you think was the average net annual income of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work in 2019?”

The income question is presented as an open-ended question such that respondents can provide an answer in euro or select “I do not know.” If they choose the “I do not know” option, then they are presented with a follow-up screen with unfolding brackets (see Appendix B for specific ranges). These follow-up questions reduce non-response.

Apart from these questions, the first part of wave 1 (wave 1A) included questions on their own economic situation and attitudes towards spending and borrowing, their happiness, and fairness views on inequality. For instance, respondents were asked to indicate to what extent they agreed with statements like “Differences in incomes in the Netherlands are too large”; “Incomes should be made more equal”; “Differences in income are an incentive for individual effort”; “In the long run, hard work usually brings a better life”; “Hard work does not generally bring success – it is more a matter of luck and connections”; “Cheating on taxes if you have a chance can never be justified”; “Differences in wealth in the Netherlands are too large”. To assess how often people participate in social activities, the survey asks respondents how often in the last month, they have “met with friends, neighbors, acquaintances, or people at work (after working hours)”; “Gone to a sport, social or other kind of club”; or “Taken part in a political or community-related organization/activities”. Respondents were also asked about their level of happiness through the following question: “All in all, to what extent do you consider

² Parts of the follow-up survey waves overlap with the COVID-19 outbreak. The February follow-up preceded the outbreak in the Netherlands, and the March follow-up was fielded from February 28 to March 17, 2020, with the great majority of responses collected before national measures were announced on March 12. Wave 3 (May) occurred during the work-from-home period. We explored heterogeneity by southern-region residence and urbanity, but the resulting subsamples are too small and the first stage too weak to draw useful conclusions. Because the pandemic could plausibly have dampened social interaction while also shifting households toward some durable purchases, we do not interpret the estimates as lower bounds. The supplementary Prolific vignette, fielded in 2026 under ordinary conditions, reproduces the direction of the socializing and work-hour responses and therefore provides complementary evidence on external validity.

yourself a happy person?” with answers provided on a scale of 1 (very unhappy) to 5 (very happy). Responses to each socializing item are reported in intervals and converted to midpoint counts of 0, 1.5, 3, 7, and 12.5 for “0 times,” “1-2 times,” “3-4 times,” “5-9 times,” and “10 times or more,” respectively.

The second part of the first wave (wave 1B), was fielded in the two weeks (first half of February) following the first part. We first reminded respondents about their own net income levels (reported in the first part) and allowed them to amend their response if necessary. Subsequently, we provided information treatments constructed from the information collected in the first part. Each respondent was randomly assigned to treatment/control groups. The treatment group was shown the following statement:

*“Before we proceed, we would like to share the following information with you. According to recent representative statistical data, the **total net annual household income of a typical household of similar age and education like yours is: X euro**”*

We refer to this group (N=824) as having been treated with peer income. The control group (N=825) receives no such information.³ When respondents in the treated group are shown the statements on the household income of typical households of similar age and education, the screen simultaneously displays a bar chart visualizing the provided information. This chart contains two bars showing their own self-reported level of total household income versus the income of a typical household like them and helps them to conceptualize their income standings relative to their peers.⁴

When providing these information treatments, we follow Hvidberg et al. (2023) and focus on peer groups that are narrowly defined. Specifically, the value of X shown to each respondent depends on the age of the respondent (age categories: <36, 36-50, 50-65, 66+), the education of the respondent (education categories: less than high school, high school, college, university), whether the respondent has a partner. Appendix B reports the values shown to each subgroup.

The treatment was followed by repeating the question asking for an estimate of the average net annual income in 2019 of households that respondents frequently associate with. This allows us to measure the effect of the information treatments on beliefs about peers’ income. Over the course of two subsequent waves (waves 2 and 3), many of the questions that were asked in the first wave before the treatment were repeated. Hence, we can measure whether treatments affected the beliefs of respondents about the need for redistribution, the extent to which they socialize with others, etc. In addition,

³ The original experiment also included a third group receiving information about peer debt with the aim to influence relative debt standing (see van Rooij et al. 2024). Because the present paper focuses on relative income, we do not analyze the peer debt treatment in the main text; Appendix Table A6 reports the joint updating results.

⁴ Appendix Figure A1 shows an example of such a chart. Appendix Table A12 verifies that respondent characteristics are balanced across treatment arms. Treatment status is not correlated with participation in subsequent waves.

respondents were asked questions about how much they had spent in the last month on non-durables and services, whether they had made any large durable purchases and how much they had spent on these, as well as a question about whether they had taken out any new loans. We use these questions in Section 5 to assess whether beliefs about peers' income levels affect their beliefs and decisions.

As a complementary exercise, we fielded a separate vignette survey of Dutch households on Prolific in 2026 designed to shed additional light on the mechanisms underlying responses to relative income (the questionnaire is in Appendix D). Respondents were randomly assigned to a scenario in which their household income was described as either 20 percent higher or 20 percent lower than the average income of households with whom they associate. The vignette therefore varies relative standing directly, without providing an absolute income amount or information about the broader income distribution. Respondents were then asked how they would feel or behave along margins closely related to those studied in our field experiment, including happiness and fairness, hours worked and time spent socializing, durable and nondurable spending, saving and borrowing, and beliefs about the incomes of people outside their social circle. For one randomly selected margin, respondents were also asked to explain their answer in an open-ended response. We use these vignette responses as complementary evidence on the direction of behavioral responses and on the mechanisms respondents associate with changes in relative standing.

2.2 Characteristics of Respondents and Representativeness of the Survey

Descriptive statistics about respondents are provided in Table 1. The average respondent is 56 years old, and about half are women.⁵ More than a third of the respondents have either a college or higher vocational education degree. The average net annual household income is €38,764 (median income: €35,500) with a total debt value of €90,460, although the variance across respondents is very large for both. These numbers are reasonably close to the official population-wide figures gathered by Statistics Netherlands, which for 2019 point to an average net household income of €45,700 (median income: €36,700) and to an average household debt of €102,800 (of which mortgage debt on housing: €88,900). Spending on durables is lumpy: 47 percent of respondents report a durable purchase in the previous month, and the average spending over the previous month conditional on buying a durable good is €1,663 while the median is €350.

2.3 Empirical framework

⁵ The respondents are household heads and their partners who are 18 years or older. As a result, the average age in the sample is higher than the average age of the population, which includes children.

Our survey design aims to address several identification issues in peer-effect studies. To the extent that peer effects matter for own behavior, *perceptions* about peers (and thus relative standing) are a key driving force. It does not matter if these perceptions are objectively accurate or not as people often make decisions based on misperceptions or incomplete information. Despite their key role in analyzing peer effects, these perceptions are rarely collected in surveys and are not available in administrative data (e.g., on labor contracts). Instead, it is common to quantify peer effects by estimating the effects of the behavior of the peer group on own behavior. In this context, one has to address two challenges: a) define/know the characteristics of the peer group; and b) consider a behavior that is typically visible (e.g., conspicuous consumption, car type, etc.). Challenge a) raises identification concerns because people choose their peers non-randomly and there may be a correlation between unobserved characteristics of the respondent or respondent's environment and those of peers (correlated effects). Challenge b) is an issue because regressing, e.g., own spending on peers' conspicuous spending suffers from the 'reflection' problem (Manski 1993). We address these various concerns by *directly* eliciting respondent-specific perceptions about their actual peers. In this setup, there is no need to ask respondents to define the characteristics of their social circle. Section 2.4 describes survey evidence on the perceived income levels of peers.

One remaining challenge, however, is that perceived relative standing likely also correlates with unobserved respondent characteristics that may be time-varying (e.g., optimism). As a result, even if such perceptions are recorded in successive surveys one cannot rely on standard panel data techniques (e.g., fixed effects) to consistently estimate the effect of relative standing on behavior. In view of this, we implement a randomized controlled trial which, as we discuss in Section 4, generates exogenous variation in the perceptions about peers as defined by a respondent. In this framework, we can credibly estimate the causal effect of perceptions about peers' income on own beliefs and behavior. The supplementary vignette also allows us to examine whether learning about one's relative position generates inferences beyond the immediate peer group, an issue we return to when discussing the interpretation of the information treatment.

2.4 Perceived Income Levels of Peers

In the survey, respondents are asked about how much they believe their peers earn. Given that we also ask survey participants about their own household income levels, we can therefore measure their perceived relative income standings compared to their peers. Figure 1 plots the resulting distribution of perceived relative income. We can see that there is significant variation in where households perceive themselves to be relative to their peers. The average log relative income is negative, indicating that, on average, Dutch households believe that their peers earn somewhat more than they do. The median

respondent in our survey reports earning an annual income of €35,500 but the median estimate of their peers' income is almost €40,000 per year (Table 1). In our sample though, we observe many households who believe they earn far more or far less than their peers. The distribution displays a fat tail on its lower end, indicating that there are relatively more households who perceive themselves as earning far less than their peers than there are who perceive themselves as earning far more.

Figure 2 plots a binscatter showing the correlation between the income of respondents and their estimates of their peers' income. As one might expect, peer income is increasing in household income, consistent with some sorting across income groups. However, the slope of the relationship is less than one. Respondents earning around €40,000 perceive their peers to earn about the same as they do, on average. Higher-income households perceive themselves as earning more than their peers on average, while lower-income households perceive themselves as earning less than their peers. Beliefs about peers are thus compressed toward the middle, as higher-order expectations would predict. Related misperceptions are documented in Hvidberg et al. (2023), Karadja et al. (2017), and Fehr et al. (2022).⁶

As shown in Figure 1, there is much more variation in perceived relative income than what can be accounted for by individuals' own income. To assess what other factors correlate with their perceived rankings, we regress perceived individual relative income on a wide range of observable characteristics and report the results of these regressions in Table 2. Column (1), for example, shows that women tend to perceive their income as lower than their peers by nearly 7 percent compared to men. Those who are employed, living with a partner and who own their home generally also perceive their income as being higher than their peers, with these effects ranging from 10% to 22%. Interestingly, once we condition on these variables, education is not related to perceived income standing, nor is age. In column (3), we replace the relative perceived income as the dependent variable with the absolute value of the perceived income gap, which allows us to ask what characteristics are associated with individuals perceiving themselves as further from their peers in either direction. In this case, the main driver of perceived differences is living with a partner: those who live with a partner generally believe their peers have similar incomes whereas those who do not perceive larger income differentials between themselves and their peers on average.

Thus, our survey results indicate that there is widespread variation in how individuals perceive themselves financially relative to their peers, even in a relatively low inequality country like the Netherlands. We observe individuals who perceive themselves as much better off than their peers as well as individuals who think they are much worse off.

⁶ Jager et al. (2024) similarly find that workers in low-paying firms tend to underestimate wages elsewhere.

3. A Framework for Characterizing Relative Income Effects

To organize the different margins through which relative income can affect household behavior, we consider a simple static model. The framework is intended to clarify which outcomes speak to different channels and to distinguish responses that occur while own income and work time are fixed from those that arise once households can adjust labor supply. Detailed derivations are provided in Appendix C.

A household chooses durable consumption c_d , nondurable consumption c_n , labor supply l , time spent socializing s , private leisure h , and borrowing b to solve

$$\max_{c_d, c_n, l, s, h, b} \left\{ \frac{C^{1-\sigma}}{1-\sigma} + \alpha \ln R + \beta s^\nu R^\delta + \phi h^\nu - \frac{\kappa}{1+\epsilon} l^{1+\epsilon} - \frac{\xi}{1+\theta} b^{1+\theta} \right\} \quad (3.1)$$

subject to

$$p_d c_d + c_n = \lambda (wl)^{1-\tau} + b, \quad (3.2)$$

and

$$l + s + h = 1. \quad (3.3)$$

Disposable income is $y = \lambda (wl)^{1-\tau}$, and relative income is

$$R \equiv \frac{y}{y^{peer}}, \quad (3.4)$$

where y^{peer} denotes perceived disposable income of the household's peers. Thus, a decline in R corresponds to a deterioration in perceived relative standing. The consumption composite is

$$C = R^\gamma [(c_d R^{-\mu})^\rho + c_n^\rho]^{1/\rho}, \quad (3.5)$$

where $\varepsilon = 1/(1-\rho)$ is the elasticity of substitution between durable and nondurable consumption. We assume $\gamma > 0$, so that higher relative income raises the utility generated by a given overall consumption bundle. This captures a keeping-up-with-the-Joneses channel analogous to the utility effects from external consumption habits. We also assume $\mu > 0$, which allows relative income to alter the utility value and therefore the desired share of visible durable consumption. The parameter α captures a direct utility effect of relative status, while δ governs how relative status affects the utility from socializing.

3.1 Spending Responses

We first consider spending responses while current work time and own-income are fixed. This corresponds most closely to the initial effect of the information treatment that we will estimate. Since the treatment changes perceived peer income while own income is predetermined,

$$d \ln R = -d \ln y^{peer}. \quad (3.6)$$

Let total consumption expenditure be $X \equiv p_d c_d + c_n$, and let $s_d \equiv p_d c_d / X$ denote the durable expenditure share. The price index for the consumption composite can be written as

$$P(R) = R^{-\gamma} [p_d^{1-\varepsilon} R^{\mu(1-\varepsilon)} + 1]^{1/(1-\varepsilon)}, \quad (3.7)$$

so that $X = P(R)C$. Its elasticity with respect to relative income is

$$\eta_P \equiv \frac{\partial \ln P(R)}{\partial \ln R} = \mu s_d - \gamma. \quad (3.8)$$

This expression captures two potentially offsetting forces. The term $-\gamma$ reflects the keeping-up channel. A decline in relative income reduces the utility obtained from a given consumption bundle and raises the effective cost of attaining a given level of consumption utility. The term μs_d reflects the conspicuous-consumption channel operating through durables. Because relative income changes the effective value of durable consumption, it also changes the effective price of the overall consumption composite. The net effect on the effective price therefore depends on the relative strength of these two forces.

Let $\omega_l \equiv y/X$ denote the share of current expenditure financed out of labor income. Holding work time and current income fixed, any adjustment in expenditure occurs through the intertemporal financing margin b . The short-run elasticity of total consumption expenditure with respect to relative income is

$$\left. \frac{d \ln X}{d \ln R} \right|_{SR} \approx (1 - \omega_l) \frac{\sigma - 1}{\theta} (\mu s_d - \gamma). \quad (3.9)$$

For $\sigma > 1$ (σ is the coefficient of relative risk aversion), the keeping-up channel implies that a decline in relative income raises the marginal value of additional expenditure and therefore tends to increase total spending. The conspicuous-consumption term works in the opposite direction through the effective price index. The magnitude of the total spending response is also smaller when the financing margin is more costly, as captured by a larger θ (the curvature of the borrowing-cost term in the utility function), or when a larger share of expenditure is tied to predetermined labor income. As a result, a small response of total spending to a change in relative income could reflect weak relative-income effects, but it could also arise when the keeping-up and conspicuous-consumption channels offset one another.

The composition of expenditure provides a clean prediction for the conspicuous-consumption channel. Household optimality implies

$$\frac{d \ln(c_d/c_n)}{d \ln R} = -\mu(\varepsilon - 1). \quad (3.10)$$

Unlike the total spending response, this comparative static does not depend on γ . With $\mu > 0$, a decline in relative income raises durable consumption relative to nondurable consumption when $\varepsilon > 1$. In this

case, learning that peers earn more shifts consumption toward visible durable goods even if total spending changes little. If $\varepsilon < 1$, the compositional response has the opposite sign. The combination of the total spending response and the durable-to-nondurable response therefore helps distinguish a broad keeping-up channel from a reallocation toward conspicuous consumption.

3.2 Socializing and Happiness

We next consider socializing and happiness while continuing to hold work time fixed. With l fixed, total non-work time $s + h$ is fixed, so a change in relative income initially reallocates time between socializing and private leisure rather than changing the total amount of non-work time. The optimal allocation satisfies

$$\frac{d\ln(s/h)}{d\ln R} = \frac{\delta}{1 - \nu}. \quad (3.11)$$

Thus, when $\delta > 0$, higher relative income raises socializing relative to private leisure. To map this result more directly into the time spent socializing, define $\chi_s \equiv s/(s + h)$ as the share of non-work time devoted to socializing. Holding work time fixed,

$$\left. \frac{d\ln s}{d\ln R} \right|_l = (1 - \chi_s) \frac{\delta}{1 - \nu}. \quad (3.12)$$

Hence, if $\delta > 0$, an improvement in relative standing increases time spent socializing, while a deterioration in relative standing reduces it.

Relative income can also affect utility (or “happiness”) directly. Let V denote optimized utility conditional on work time. Applying the envelope theorem gives the semi-elasticity of happiness with respect to relative income:

$$\left. \frac{dV}{d\ln R} \right|_l = C^{1-\sigma}(\gamma - \mu s_d) + \alpha + \beta \delta s^\nu R^\delta. \quad (3.13)$$

There are three channels. The first is the consumption channel, which is the mirror image of the effective-price effect described above and depends on the net strength of the keeping-up and conspicuous-consumption forces. The second is the direct status effect, governed by α . The third operates through the utility value of socializing and is governed by δ . The overall effect on happiness is therefore ambiguous without additional restrictions. In particular, sizable effects on happiness can arise even when total expenditure responds little because direct status and socializing enter utility separately from total consumption spending.

3.3 Work Time, Income Adjustment, and Persistence

The preceding comparative statics hold work time and own income fixed. Over longer horizons, however, households may adjust labor supply in response to a change in relative standing. Let

$$\Phi \equiv \frac{d \ln l}{d \ln R} \quad (3.14)$$

denote the elasticity of work time with respect to relative income. The sign of Φ is ambiguous. A financial channel arises because relative income changes the effective price of consumption and hence the marginal value of additional resources. A time-value channel arises because relative income changes the utility from socializing and therefore the opportunity cost of work. Depending on which force dominates, a deterioration in relative standing can either increase or reduce work time.

Labor adjustment also changes own income and therefore feeds back into relative income itself. Since $y = \lambda(wl)^{1-\tau}$,

$$d \ln R = (1 - \tau) d \ln l - d \ln y^{peer}. \quad (3.15)$$

Combining this expression with the definition of Φ yields

$$\frac{d \ln R}{d \ln y^{peer}} = - \frac{1}{1 - (1 - \tau)\Phi}. \quad (3.16)$$

If work time does not respond, the initial change in perceived peer income translates one-for-one into an opposite change in relative income. If a decline in relative income induces additional work, so that $\Phi < 0$, the resulting increase in own income attenuates the initial deterioration in relative standing. If instead lower relative income reduces work, the initial shock can be amplified. The persistence of the experimental change in relative standing therefore depends on the endogenous response of work and income.

Allowing work time to adjust also modifies the earlier behavioral responses. For total expenditure, the elasticity becomes

$$\frac{d \ln X}{d \ln R} \approx \omega_l(1 - \tau)\Phi + (1 - \omega_l) \frac{\sigma - 1}{\theta} (\mu s_d - \gamma). \quad (3.17)$$

The first term is now an income channel operating through work, while the second is the short-run financing channel described above. Similarly, work adjustment changes the total amount of non-work time available for socializing. The elasticity of time spent socializing becomes

$$\frac{d \ln s}{d \ln R} = (1 - \chi_s) \frac{\delta}{1 - \nu} - \frac{l}{1 - l} \Phi. \quad (3.18)$$

The first term captures the direct reallocation of non-work time toward or away from socializing, while the second captures the change in the amount of non-work time generated by the labor response. Thus, the spending, socializing, and happiness effects described above are most naturally interpreted as short-run

responses to a change in relative standing. Their persistence and longer-run magnitudes depend on how households subsequently adjust work and own income. Table 3 summarizes these comparative statics.

3.4 Equity and Redistribution

Relative standing may also affect beliefs about equity and the preferred degree of redistribution. To capture this possibility, Appendix C extends the framework by allowing households to have preferences over the progressivity of the tax system rather than taking it as given. Greater progressivity redistributes resources toward households with relatively low income, but it may also entail perceived efficiency costs by weakening incentives to work. Relative income can affect both sides of this tradeoff. A household that perceives itself as earning less than its peers has a larger private gain from redistribution. At the same time, relative standing may affect how costly the household perceives redistribution to be. In particular, we allow households with higher relative income to attach a greater incentive value to income differences, and therefore to perceive a larger efficiency cost from redistribution. This provides a channel through which peer comparisons can affect views about inequality even without changing beliefs about the extent to which individual outcomes reflect hard work.

These forces imply that a decline in relative income can increase the preferred degree of redistribution through several channels. First, the household stands to gain more directly from transfers when it falls further below the relevant comparison group. Second, if lower relative standing reduces the perceived incentive benefits of inequality, the perceived cost of redistribution also falls. Finally, when $\sigma > 1$ and the keeping-up channel dominates the conspicuous-consumption channel so that $\eta_P = \mu s_d - \gamma < 0$, a decline in R raises the effective price of consumption and increases the marginal value of additional resources, further strengthening the value of redistribution. Appendix C derives the corresponding comparative statics and provides conditions under which preferred progressivity declines with R . Equivalently, learning that peers earn more than previously believed can increase the demand for redistribution.

These results speak most directly to preferred redistribution. However, the same mechanism provides a natural interpretation for beliefs about whether observed income or wealth differences are excessive: relative standing can affect not only an individual's pecuniary stake in redistribution, but also the perceived social benefits and costs of inequality. This distinction is useful empirically because changes in relative income in our setting affect both views about whether inequality is too large and beliefs about whether income differences provide incentives.

4. First Stage and Identification

At the heart of our empirical strategy is the information treatment to generate exogenous variation in the perceived relative income standings of respondents. As described in Section 2, we implement this approach by randomly assigning survey participants to either a control group or the treatment group. The control group is not provided with any additional information. The treatment group is provided with information about the income level of their peers (“peer income treatment”). We can then assess whether this new information affects the beliefs and actions of treated individuals relative to those in the control group.

Unlike traditional RCTs in which all respondents within a treatment group receive the same piece of information, in this case the signals vary within a treatment group based on a set of characteristics. Specifically, because we aim to provide relevant information about an individual’s peers, we consider some key characteristics that typically define social circles. In the case of income, we provide the typical income of Dutch households in the same age group, with the same general education level, and with the same partner status. There are hence many different values or signals being provided about peers’ income levels given the heterogeneity in individual characteristics of respondents.

To see whether and how these treatments affected beliefs about peers, we first present visual representations of treatment effects in Figure 3. We plot fitted relationships between the posterior beliefs of individuals about their peers’ income levels and their priors about peers’ incomes for different groups of individuals. One group is the control group which received no information. For this group, one would expect posteriors to equal priors on average since they did not receive information in between providing their priors and posteriors. Indeed, we see visually that the average relationship between posteriors and priors for this group has a slope close to one. This is reassuring for the consistency of responses and the survey quality as the priors were collected in the first part of the survey, about fifteen days before collecting information on posteriors.

We then group households by the euro amount that they were told was their peer group’s average income. Note that if they were to respond as Bayesians, we would expect their posteriors to be related to their priors as follows:

$$Posterior_i = (1 - G) \times Prior_i + G \times Signal_i \quad (4.1)$$

such that posteriors are a weighted average of their prior beliefs and the signal they receive, where the weight is the Kalman gain which captures the perceived precision of the signal. If the provided signal is perceived as informative, G will be greater than zero and their posteriors will move away from their priors and toward the signal. As can be seen in Figure 3, this is precisely what seems to happen. For a given signal and demographic group, we see that respondents whose prior beliefs about their peers’ income were higher

than the signal tend to report posteriors that are much closer to the signal than to their prior beliefs. This holds across different levels of the provided signal and for different levels of prior beliefs.

To quantify these treatment effects more precisely, we estimate the following specification:

$$\begin{aligned} \text{Posterior}_i = & b_0 + b_1 \times \text{Prior}_i + b_2 \times \text{Prior}_i \times \mathbb{I}(i \in \text{Treatment}) \\ & + b_3 \times \mathbb{I}(i \in \text{Treatment}) \times \text{TreatmentSignal}_i + \text{error} \end{aligned} \quad (4.2)$$

where $\mathbb{I}(i \in \text{Treatment})$ is an indicator variable for being in the treatment group. Posteriors and prior beliefs are for peers' income (in logs). Note that we depart from typical estimates of treatment effects (e.g., Weber et al. 2025) by including an interaction of the treatment indicator with the value of the provided treatment (TreatmentSignal_i). This is to capture the fact that the provided signal varies within the treatment group. Under Bayesian updating, we have:

$$\begin{aligned} \text{Posterior}_i = & (1 - G) \times \text{Prior}_i + G \times \text{Signal}_i \\ = & \text{Prior}_i - G \times \text{Prior}_i + G \times \text{Signal}_i \end{aligned} \quad (4.3)$$

where $G > 0$ for the treatment group when the signal is at least somewhat informative. If the signal varies across individuals, then it is important to capture this variation in our empirical specification. Effectively, different values of the signal shift the relationship between the posterior and prior up and down, as shown in Figure 3, but leave the slope unchanged. For the control group, our empirical specification predicts that $b_1 = 1$ while $b_2 = b_3 = 0$ whereas within the treatment group we have $b_1 = 1, b_2 = -G, b_3 = G$. Thus, a testable prediction from Bayesian updating is that $b_2 + b_3 = 0$.

We report estimates of this specification in Table 4 using Huber regressions to control for outliers. Column (1) presents coefficients for beliefs about peers' income levels in the first survey wave. We find that b_1 is slightly smaller than one, as is commonly the case (e.g., Coibion, Gorodnichenko and Weber 2022). Measurement error in beliefs will naturally induce this type of downward bias in the relationship between posteriors and priors for the control group. We also find that $b_2 \approx -0.27$, which indicates that treated individuals reduce the weight on their prior belief by that amount in forming posteriors. Consistent with Bayesian updating, our estimates also imply $b_2 + b_3 \approx 0$. Since both coefficients should depend only on the Kalman gain, this result provides new evidence consistent with Bayesian updating when agents receive different signals about the same object. Importantly, the F-statistic is close to 100, which indicates that the treatments provide strong exogenous variation in beliefs about peers' incomes. We find similar results if we focus on the subset of individuals who learned (for the control group would have learned) that peers' income was lower than they thought (column 3, "good" news) or those who learned (for the control group would have learned) that peers' income was higher than they thought (column 2, "bad" news). We also find similar results when we use posterior beliefs from the subsequent follow-up waves

(columns 4 and 5), indicating that the treatments have persistent effects on beliefs about peers' income. This persistence suggests that survey demand and anchoring effects are unlikely to drive the belief revisions.⁷ Because the signal varies within the treatment group, the Bayesian restriction is also a specification test of the first-stage functional form rather than a separate result about belief formation. The reported F-statistic is computed with respect to the excluded instruments only.

We also consider whether treatment effects vary along different observable characteristics of respondents, including gender, age, education, income and debt level. We find no meaningful variation in treatment effects, conditional on prior beliefs about relative standings (see Appendix Tables A1-A5). This is consistent with other evidence that individuals consistently respond like Bayesians to treatments, adjusting their beliefs away from their priors and toward the provided signal (e.g., Coibion, Gorodnichenko and Weber 2022). While observable characteristics often help explain why individuals hold different prior beliefs, as we found in Table 2, conditional on their priors, individuals respond similarly to information treatments.

Jointly, these results indicate that our information treatments were successful in inducing powerful changes in respondents' perceptions about their peers' income. As a result, we are also creating exogenous variation in the *relative* income standings perceived by those respondents. Random assignment ensures this variation is orthogonal to unobserved determinants of the outcomes we study; the exclusion restriction requires that the information affects outcomes only through the beliefs it revises, rather than directly through channels such as anchoring on the figure provided or survey demand effects. The persistence of the belief effects across follow-up waves speaks to both, since neither would be expected to survive over one to three months. The exclusion restriction does not require the treatment to leave all other beliefs unchanged. Inferences about the broader income environment that arise from learning one's relative position are part of the total effect we seek to measure. The supplementary vignette (Appendix Table A17) shows that relative-standing information indeed moves beliefs about incomes outside respondents' immediate social circles. What the restriction rules out are direct effects of the numerical information unrelated to the belief revisions it induces. With this exogenous variation, we can then study whether these perceived positions relative to peers causally change how individuals perceive equity, redistribution, or the returns to work as well as whether they affect their employment, spending decisions, and happiness.

⁷ Survey demand effects are unlikely to be of concern here for two further reasons. First, the information we provide is such that it should not be immediately clear to respondents how they should answer a post-treatment question to satisfy the interviewer. Second, many of our outcomes are measured in follow-up waves, when respondents are unlikely to strategically link the information treatment to the choices they report.

5. The Causal Effects of Peer Comparisons

Does perceived relative income causally affect household choices and beliefs? We now use the exogenous variation generated by the information treatment to study the outcomes in the framework of Section 3. We begin with spending, distinguishing the response of total expenditure from changes in its composition, before turning to socializing and happiness. Next, we examine employment and expected income, which speak to whether households adjust their own income and thereby attenuate or amplify the initial change in relative standing. Finally, we study beliefs about equity and redistribution, corresponding to the extension in Section 3.4. Throughout, the estimates should be interpreted as total causal effects of a change in perceived relative standing, since they may combine several of the channels described in Section 3 rather than identifying any one structural parameter. For transparency, Appendix Tables A13-A16 also report the corresponding reduced-form treatment effects for equity, socializing, work and income, and spending, using the split by whether respondents initially over- or underestimated peer income.

5.1 Spending Responses

Following Section 3.1, we first consider spending. The framework of that section distinguishes two different mechanisms through which changes in relative income can affect spending. The keeping-up channel raises the value of additional overall consumption, while the conspicuous-consumption channel changes the desired composition of spending toward visible durable goods. Because these forces enter the effective price of consumption with opposite signs, we can observe small changes in total spending even when relative standing has meaningful effects on the composition of spending. Our setting provides exogenous and measurable variation in perceived peer income with which to study both margins.

Our measures of spending come from the follow-up waves. Respondents report combined monthly spending on non-durables and services, whether they purchased any large durable goods in the previous month and, conditional on a purchase, how much they spent. They are also asked whether they took out any new loans or mortgages. These measures allow us to study separately the level of non-durable expenditure, the intensive and extensive margins of durable purchases, total expenditure, and the financing margin over the first three months after the treatment. For these spending questions, respondents can select “I do not know”, in which case they are shown unfolding brackets. We use the midpoint of the selected interval, while responses remaining missing after this follow-up are treated as missing. Remaining item non-response is unrelated to treatment status.

To estimate the response to perceived peer income, we treat own income as predetermined over this short time horizon as in Section 3.1, so an increase in perceived peer income corresponds to an

initial deterioration in relative standing. For the spending outcomes, the first-stage specification is therefore given by:

$$\begin{aligned} \log(\mathbb{E}Peer\ Income_i^{posterior}) = & b_0 + b_1 \times \log(\mathbb{E}Peer\ Income_i^{prior}) \\ & + b_2 \times \log(\mathbb{E}Peer\ Income_i^{prior}) \times \mathbb{I}(i \in Treatment) \\ & + b_3 \times \mathbb{I}(i \in Treatment) \times \log(TreatmentSignal_i) \\ & + controls_i + error_i \end{aligned} \quad (5.1)$$

and the second stage is given by:

$$\begin{aligned} \log(Spending_i) = & c_0 + c_1 \times \log(\mathbb{E}Peer\ Income_i^{posterior}) \\ & + c_2 \times \log(\mathbb{E}Peer\ Income_i^{prior}) + controls_i + error_i \end{aligned} \quad (5.2)$$

where $controls_i$ includes (log) own income, age and age squared, educational attainment, gender, number of household members, number of children, employment status, homeownership status, and regional dummy variables. This specification thus gives a direct estimate of the elasticity of individual spending with respect to perceived peer income.

Panel A of Table 5 reports the results for non-durable goods and services. Across all respondents, higher perceived peer income is associated with lower non-durable spending, although the estimate is not statistically significant in the full sample. The effect is more clearly identified among respondents receiving bad news, for whom the information treatment implies that peers earn more than they previously thought. Thus, the response of non-durable spending points toward a reduction in less visible consumption when relative standing deteriorates.

Panels B and C turn to durable goods. Higher perceived peer income raises both durable spending and the probability of making a durable purchase, with the effects again most clearly identified among respondents receiving bad news. Hence, when households learn that their peers earn more than they thought, they shift expenditure toward durables and away from non-durables and services. This pattern maps directly into the conspicuous-consumption margin in Section 3.1. In the model, a decline in relative income raises durable consumption relative to non-durable consumption when the conspicuous-consumption parameter is positive and the elasticity of substitution between the two goods exceeds one. The data support such a reallocative response toward more visible goods even if total spending changes little. Panel D provides no evidence of an increase in new borrowing over this period, consistent with much of the additional durable spending being financed by reductions elsewhere in the consumption basket.

The implied net effect on total spending is small. Evaluated at the median, the estimated increase in durable spending is about €13 per month while the decline in non-durable spending is about €15, for a net change of roughly -€2. This corresponds to a 0.2 percent decline in total spending when perceived

peer income rises by 1 percent. As an alternative to combining the component estimates, Panel E estimates total spending directly. The elasticity with respect to perceived peer income is -1.98 (s.e. 1.24), with a 95 percent upper bound of 0.45. Viewed through Section 3.1, a small net response does not by itself imply that relative-income considerations are weak: the keeping-up and conspicuous-consumption channels can offset each other in total expenditure even while producing a clear change in composition.

The total-spending estimate does, however, place a useful bound on a large positive short-run pass-through from peer income to own expenditure. Standard external-habit estimates in macroeconomics often imply pass-through from others' consumption into own spending of roughly 60 to 90 percent (Smets and Wouters 2007; Havranek et al. 2017). Our upper bound is well below that range. At the same time, the comparison should not necessarily be read as a structural estimate of the keeping-up parameter in Section 3.1, because the observed net response also reflects the conspicuous-consumption channel and because our treatment shifts beliefs about peers' income rather than peers' consumption. This distinction also matters for the macroeconomic interpretation of the results. The composition effects provide evidence for a conspicuous-consumption response to relative standing, while the total-spending estimates provide little support for a large net external-habit response over the horizon we observe. Mechanisms such as internal habit formation or rule-of-thumb behavior can generate consumption inertia without requiring a large contemporaneous peer effect (Christiano, Eichenbaum and Evans 2005; Campbell and Mankiw 1989). In finance, rare disasters and Epstein-Zin preferences provide alternative mechanisms for generating time-varying risk premia (Barro 2009; Bansal and Yaron 2004). Recent microeconomic evidence also indicates that subjective disaster beliefs are held and acted upon by households, investors, and firms (Beutel and Stockerl 2025; Giglio et al. 2021; Menkhoff 2025). Our estimates do not distinguish among these alternatives, but they provide direct microeconomic evidence on the particular peer-spending mechanism emphasized by external-habit models.

One limitation of our estimates is that they cover spending only over the first three months after the information treatment. A slowly moving reference stock could generate a small short-run response and a larger long-run one, as in Ravn, Schmitt-Grohe and Uribe (2006). In addition, if respondents eventually adjust their working hours and income, this can change relative standing itself over time. We return to this margin in Section 5.3. Thus, the spending results are most naturally interpreted as short-run responses to the initial change in perceived relative income.

The supplementary vignette provides complementary evidence on these spending responses, although the pattern is not identical to that in the field experiment. Respondents assigned to imagine that their income was 20 percent below that of their peers were less likely to report that they would increase nondurable spending than respondents assigned to be 20 percent above their peers. This is consistent with

the decline in nondurable spending among the bad-news respondents that we observe in the field. However, they were also less likely to report that they would purchase a durable good, whereas in the field experiment a deterioration in relative standing increases durable spending. Respondents in the lower-relative-income scenario were also less likely to save and more likely to borrow. Thus, the vignette confirms that relative standing affects financial choices but does not reproduce the shift toward durable consumption that we observe in the Dutch data. We report the full set of vignette responses in Appendix Table A17 and Figures A4 and A5.

The open-ended explanations provide some insight into this difference. Respondents in the lower-relative-income scenario more frequently described themselves as being in a financially vulnerable position or expressed concerns about debt, while status or display motives were much more common among respondents assigned to be above their peers. Many respondents in both groups also indicated that the comparison itself would have little effect on their choices. These responses suggest that relative-income comparisons can activate several competing considerations, including status concerns and financial caution, whose relative importance may depend on the direction and context of the comparison. This reinforces the distinction between the net response of total expenditure and the forces affecting the composition of spending, while cautioning against interpreting the durable-goods response as a mechanical consequence of changes in relative standing.

5.2 Socializing and Happiness

We next turn to socializing, the second margin highlighted in the model. Holding work time fixed, the model predicts that higher relative income increases socializing relative to private leisure when relative status raises the utility from social interaction. The follow-up surveys allow us to test this implication directly. Panel A of Table 6 reports the effect of perceived relative income on the number of meetings with friends, neighbors, acquaintances, or coworkers outside of work. We find tentative evidence of a positive effect: households that perceive themselves as earning more relative to their peers spend more time socializing with them, although the estimate is imprecise and statistically significant only at the 10 percent level. Panels B and C consider the setting in which this additional social interaction occurs. There is suggestive evidence of more visits to sports, social or other clubs, but no evidence of greater participation in political or community-related organizations. The response therefore appears to concern social interaction itself rather than civic or political engagement. Taken together, these estimates are consistent with the social-status channel in the model, although their precision warrants caution. This margin may matter beyond leisure itself because social interaction is an input into networks through which information and job opportunities can be transmitted. Bayer, Ross and Topa

(2008), for example, show that residential proximity can generate job referrals and affect labor market outcomes. Our design does not identify such downstream effects, but it suggests that the intensity of social interaction, and hence potentially the social capital underlying these networks, can itself respond to relative economic position.

The supplementary vignettes provide corroborating evidence for this margin. Respondents assigned to imagine that their income was 20 percent below that of their peers reported that they would spend 0.53 fewer hours per week socializing, whereas those assigned to be 20 percent above their peers reported that they would spend 0.55 additional hours socializing, a difference of 1.08 hours per week (Appendix Table A17). The open-ended explanations (Appendix Figure A5) suggest substantial heterogeneity, with some respondents viewing the comparison as largely irrelevant to their social behavior and others invoking differences in circumstances or broader social considerations.

With relative income seemingly affecting both consumption and social interactions, we next turn to whether it also affects happiness. In the model, relative income can affect utility through three distinct channels: the consumption channel, the utility derived from socializing, and a direct status effect. Happiness can respond strongly to relative standing even when total spending changes little. Greater socializing could account for part of such a response, but it need not be the primary mechanism.

We test this implication using self-reported happiness as the outcome in the following specification:

$$Happiness_i = b_1 \times \log\left(\frac{Own\ Income_i^{post}}{Peer\ Income_i^{post}}\right) + b_2 \times \log\left(\frac{Own\ Income_i^{prior}}{Peer\ Income_i^{prior}}\right) + Controls_i + error \quad (5.3)$$

where posterior relative income is instrumented with the interactions of the treatment indicator with the prior belief and with the (log) signal, as in the first stage (5.1) with relative income in place of peer income. Because the endogenous regressor is $\log(\text{own income} / \text{perceived peer income})$, this specification imposes equal and opposite coefficients on own and peer income. This is the homogeneity restriction implied by the framework, in which relative income is the relevant state variable: the experiment therefore identifies the relative margin rather than a separate level effect of own income.⁸

Table 7 shows a large causal effect of perceived relative income on happiness: a one-standard-deviation increase in perceived relative income raises happiness by about 0.4 standard deviations. The IV estimate is roughly three times the corresponding OLS estimate, suggesting that the cross-sectional

⁸ Correlations between happiness (and other beliefs) and perceived relative income are reported in Appendix Figures A2 and A3.

associations emphasized in earlier work substantially understate the causal relationship (Ferrer-i-Carbonell 2005; Luttmer 2005). The effect is concentrated among respondents receiving good news about their relative standing; for the bad-news group the estimate is close to zero and statistically insignificant (see Appendix Tables A7 and A8). Happiness is measured one month after the information treatment, so the effect persists beyond the immediate updating exercise.

To the best of our knowledge, this is the first clear causal evidence that perceived relative income has a quantitatively large effect on happiness. The result is particularly informative when viewed together with the spending estimates above. The framework explicitly allows a large utility response to relative standing without a correspondingly large movement in total expenditure because status and social interaction enter utility separately from consumption. The combination of weak total-spending effects and strong happiness effects is therefore not internally inconsistent. Instead, it points away from an interpretation in which relative income matters only because it changes desired consumption.

The framework also helps organize the possible mechanisms behind the happiness response. A first channel operates through consumption, a second through changes in socializing, and a third through the direct utility value of relative status. In addition, changes in views about inequality could empirically covary with happiness even though this mechanism is not needed for the core result. We therefore examine whether the estimated effect on happiness is attenuated when we control for ex-post changes in these outcomes.

Columns (3)-(6) of Table 7 add these controls one at a time and jointly. Controlling for time spent with peers reduces the coefficient by about 10 percent, while controlling for spending also lowers it somewhat. Adding controls for beliefs about inequality has essentially no effect on the estimated coefficient on relative income. Even when all three are included together, most of the happiness effect remains. The evidence is therefore consistent with a modest contribution from socializing and consumption but mostly points to an important direct status component of relative income, as captured by the direct utility term in Section 3.

The vignette results (Appendix Table A17) also reinforce the causal relationship between relative standing and happiness. Respondents assigned to the lower-relative-income scenario reported average happiness of 3.03 on a seven-point scale, compared with 4.28 among respondents assigned to the higher-relative-income scenario, a difference of 1.25 points. Because the vignette changes relative position directly without changing actual income or consumption, this result is consistent with relative income affecting utility through channels other than total expenditure, including direct status utility and the value of social interaction. The open-ended responses again point to multiple underlying

considerations, including merit, structural differences across households, altruistic concerns, and in some cases the view that the comparison itself should not affect well-being.

5.3 Work Time, Expected Income, and Persistence

The preceding results are most naturally interpreted as responses to a change in relative standing while own income is initially predetermined. Section 3.3 emphasizes that this need not remain true over longer horizons. Households may adjust work in response to relative standing, changing their own income and therefore the persistence of the initial shock to relative income. The model does not impose a sign on this response because the financial incentive to acquire additional resources and the value of non-work time can move in opposite directions. Our employment and expected-income measures provide evidence on this adjustment margin. They also connect to earlier work showing that workers who learn that peers earn more report lower job satisfaction and greater search effort or wage demands (Card et al. 2012; Jager et al. 2024), while other studies document morale and separation responses to pay differences (Dube et al. 2019).

Respondents report employment status in both the initial and follow-up waves. We estimate specification (5.3) using an indicator for employment in the follow-up wave as the outcome and control for prior employment status. Panel A of Table 8 shows no statistically significant effect for the full sample, but this masks a pronounced response among respondents receiving bad news. When households learn that their peers earn more than they previously thought, they become more likely to be employed in subsequent months. This pattern corresponds to a negative work-time response to relative income: a deterioration in relative standing induces additional work. The estimated magnitude is large over the short horizon we observe, with a one-standard-deviation increase in relative income lowering the probability of employment by roughly 15 percentage points in the bad-news sample. There is no comparable response among those receiving good news.

Employment is only one margin through which own income can adjust. In the follow-up wave, households are also asked to predict their net household income for 2020. We use these expectations to ask more directly whether higher perceived peer income is associated with an offsetting increase in own income. The first-stage specification is equation (5.1) and the second stage is given by:

$$\log(\mathbb{E}Income_i) = c_0 + c_1 \times \log(\mathbb{E}Peer\ Income_i^{posterior}) + c_2 \times \log(\mathbb{E}Peer\ Income_i^{prior}) + controls_i + error_i \quad (5.4)$$

where $controls_i$ includes (log) own past income, age and age squared, educational attainment, gender, number of household members, number of children, employment status, homeownership status, and regional dummy variables. $\log(\mathbb{E}Income_i)$ is the expected income for 2020 of household i . Because

our dependent variable is now the log of expected household income, this specification gives us a direct estimate of the elasticity of peers' income into individual's own expected income.

Panel B of Table 8 reports the results. For the full sample, the estimated effect of perceived peer income on expected household income is 0.48 with a standard error of 0.30. The point estimate therefore suggests partial adjustment of own income in the same direction as peer income, and the estimate is somewhat larger in the bad-news sample. However, the confidence interval is wide enough to include both no income adjustment and an approximately one-for-one response. Moreover, the dependent variable is expected rather than realized long-run income. The data therefore do not allow us to determine how much of the initial change in relative standing ultimately persists.

The employment results nonetheless provide evidence that households can respond to a deterioration in perceived relative standing by increasing work, which would tend to attenuate the initial decline in relative income. This is qualitatively consistent with the search responses in Card et al. (2012) and Jager et al. (2024), and the asymmetry is also similar: the labor-market response is concentrated among those learning that peers are doing better than they thought. Quantitatively, however, the expected-income estimates are too imprecise to pin down the longer-run feedback emphasized in Section 3.3.

The supplementary vignette provides more direct evidence on the labor-supply margin underlying these employment responses. Respondents assigned to imagine that their income was 20 percent below that of their peers reported that they would work 2.06 additional hours per week, whereas those assigned to be 20 percent above their peers reported that they would work 0.90 fewer hours, a difference of almost three hours per week (Appendix Table A17). This pattern is consistent with the mechanism in Section 3.3 whereby a deterioration in relative standing can induce additional work and thereby partly offset the initial decline in relative income. The open-ended explanations again reveal substantial heterogeneity: some respondents emphasized satisfaction with their current work or work-life balance and saw little reason to adjust, while others explicitly described a desire to preserve or improve their relative position.

5.4 Equity and Redistribution

Finally, we consider beliefs about equity and redistribution. Section 3.4 extends the framework to this margin by allowing relative standing to affect both the household's private stake in redistribution and the perceived efficiency cost of greater progressivity. A household that sees itself as poorer relative to its peers stands to gain more from redistribution. At the same time, relative standing may change how strongly the household values income differences as incentives for effort. The model therefore predicts

that relative income can affect preferred redistribution even without changing beliefs about whether higher incomes are deserved.

To assess these effects, we estimate specification (5.3) with various beliefs from wave 2 as outcome variables. Panels A and G of Table 9 consider whether respondents believe that income and wealth differences in the Netherlands are too large. In both cases, higher perceived relative income causes respondents to become more accepting of existing differences. These results are consistent with the extension in Section 3.4: an improvement in relative standing reduces the household's private gain from redistribution and can also raise the perceived benefit of maintaining income differences as incentives.

Panel B turns directly to whether incomes should be made more equal. Again, we find a large causal effect. A one-standard-deviation increase in perceived relative income lowers support for greater income equality by about 0.4 standard deviations. Thus, perceived position relative to peers has an economically important effect on the demand for redistribution. Prior experimental evidence has been mixed, with Cruces et al. (2013) finding a similar response, Hoy and Mager (2021) finding the opposite, Fehr et al. (2022) finding no effect in the context of global inequality, and Ciani et al. (2021) concluding from a meta-analysis that the average effects of information treatments on redistribution preferences are relatively small.

The remaining outcomes help distinguish the mechanisms emphasized in Section 3.4. Panel C shows that higher relative income increases agreement that income differences provide incentives for individual effort. In contrast, Panels D and E show no detectable effect on beliefs that hard work brings a better life or that success reflects luck rather than hard work. The shift therefore does not appear to be that respondents who learn they are relatively richer conclude that their position is more deserved. Rather, they place greater weight on the incentive role of income differences. This pattern is consistent with the perceived-efficiency-cost channel in the model. Our design does not separately identify that channel from straightforward self-interest, since the same information that changes the household's private stake in redistribution can also change its assessment of the incentive value of inequality.

Panel F considers a broader fairness-related outcome: agreement that cheating on taxes can never be justified. We find no robust effect of perceived relative income on this measure. The response to relative standing therefore appears to be specific to views about inequality and redistribution rather than reflecting a general change in attitudes toward rule-following.

Following Hvidberg et al. (2023), we also estimate the effects separately for respondents receiving good and bad news about their relative standing. These samples are smaller and the estimates correspondingly less precise, but we find little evidence of systematic differences across the two groups. This symmetry is also consistent with the baseline framework in Section 3, which is

deliberately smooth and does not build in reference dependence or other mechanisms that would generate different responses to positive and negative information shocks.

The supplementary vignette provides additional evidence that relative standing shapes perceptions of equity. Respondents assigned to imagine that their income was 20 percent below that of their peers rated the situation at 3.65 on a seven-point fairness scale, compared with 4.59 among those assigned to be 20 percent above their peers, a difference of 0.94 points (Appendix Table A17). The response of preferred income taxes is smaller and does not provide a direct analogue of the redistribution result in the field experiment, so we view the fairness response as the cleaner comparison. The open-ended explanations point to several distinct considerations, including merit, altruistic concerns, and structural differences across jobs and households.

Overall, the equity results fit broadly with the framework in Section 3.4. Higher perceived relative income makes respondents less concerned about existing income and wealth differences and less supportive of making incomes more equal. At the same time, it increases the perceived incentive value of income differences without changing beliefs about whether income reflects hard work or luck. The results therefore point to both self-interest and perceived efficiency costs as plausible reasons why relative standing affects redistribution preferences, but the experiment does not separately isolate the two mechanisms.

6. Conclusion

How important are peer effects? Our results suggest that the answer depends importantly on the margin being considered. Perceived relative income has only small effects on total spending, at least over the relatively short horizon we observe, but this masks meaningful changes in the composition of expenditure. When individuals learn that their peers earn more than they thought, spending shifts toward durable goods, even as total expenditure changes little. At the same time, relative income has sizable effects along a number of other dimensions. Higher perceived relative income increases happiness and, more tentatively, time spent socializing with peers, while lower relative income can induce individuals to increase work. Relative standing also changes views about inequality and redistribution. Thus, the limited response of total expenditure should not be interpreted as evidence that peer comparisons are generally unimportant.

The framework in this paper provides a way to reconcile these seemingly different responses. Relative income can affect the utility from overall consumption, the desired composition of spending, the value of social interaction, and utility directly. These channels need not move together, and some may offset one another when looking only at total expenditure. In particular, the combination of a small total-spending response with a large happiness response is consistent with relative standing affecting utility through status and social interactions in addition to consumption. Our findings therefore provide relatively little support

for a large net short-run peer-spending effect of the type emphasized by external-habit models, while at the same time showing that relative-income comparisons can have important effects on household behavior and well-being. The supplementary vignette evidence broadly reinforces this interpretation for socializing, work, happiness, and fairness, while also illustrating that spending responses can depend on the particular setting and the competing motives activated by relative comparisons.

An additional implication of our results is that the effects of a change in perceived relative standing need not be permanent. The information treatment initially changes perceived peer income while own income is largely predetermined, but households can subsequently respond by adjusting work and income. We find that individuals who learn that their peers earn more than they thought become more likely to be employed, and the vignette evidence similarly indicates a desire to work more when placed below one's peers. Such responses would tend to offset the initial deterioration in relative standing. At the same time, our estimates of expected income are too imprecise to determine how much of this adjustment ultimately occurs. Understanding the dynamics through which peer comparisons affect work, income, and hence relative standing itself seems an important direction for future research.

More generally, our results point to relative income as affecting a much broader set of choices and beliefs than is captured by the traditional notion of “keeping up with the Joneses”. Peer comparisons influence not only what households buy, but also how they allocate their time, how they respond in the labor market, how they view inequality, and how happy they are. The large causal effect on happiness is particularly striking given the limited response of total spending. In our experiment, this effect is concentrated among respondents receiving good news about their relative standing, while those receiving bad news show little average happiness response. Given that respondents on average were somewhat pessimistic about their position relative to peers, this asymmetry also illustrates that correcting misperceptions about relative standing need not have symmetric welfare consequences. More broadly, identifying when and why relative comparisons matter across these different margins remains an important question for understanding the economic and welfare consequences of social interactions.

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Table 1: Descriptive Statistics

	Mean	Percentiles			St.dev.
		P25	P50	P75	
	(1)	(2)	(3)	(4)	(5)
Age	56.08	44.00	58.00	69.00	15.89
Female	0.49				
Education: Post-secondary	0.36				
Education: Secondary	0.33				
Education: Less than secondary	0.28				
Number of family members in the household	2.24	1.00	2.00	3.00	1.17
Spending on nondurable goods and services in the last month, €	860.91	400.00	650.00	1,000.00	1,029.59
Spending on durable goods in the last month, €	1,662.78	100.00	350.00	1,000.00	5,199.76
Share of respondents reporting positive spending on durable goods over the last month	0.47				
Household net annual income, '000€	38.76	25.70	35.50	47.50	24.77
Household total debt, '000€	90.46	0.00	30.00	155.00	139.26
Peer estimated net annual income, '000€	40.99	30.00	39.76	45.00	27.04
Share of respondents not being able to report peer estimated income	0.22				

Notes: The table reports descriptive statistics for various household characteristics of survey participants. None of the variables is winsorized, censored, etc. “Household net annual income” is based on the following question: “What was the total net annual income of your household in 2019?”. “Household total debt” is based on the following question: “What is the total value of your household debt? Please take into account mortgage loans using your house or other real estate as collateral, car loans, extended lines of credit, personal loans, student loans, checking account overdrafts, loans from family, friends or acquaintances, outstanding credit card debts, outstanding debts from mail-order firms or from other hire purchases, etc.? Please provide the total amount in euro without dots or commas.”. “Peer estimated net annual income” is based on the following question: “How much do you think was the average net annual income of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work in 2019?”. Moments for “Spending on durable goods in the last month, €” are conditional on positive spending.

Table 2: Predictors of Perceived EIncome Standing Relative to Peers

	$100 \times \log(\text{own}/\text{peer})$		$100 \times \log(\text{own}/\text{peer}) $	
	(1)	(2)	(3)	(4)
female	-6.77*** (0.99)	-4.30*** (0.76)	1.14* (0.68)	1.03 (0.66)
age	-0.19 (0.23)	-0.19 (0.17)	0.18 (0.15)	0.16 (0.15)
age ² /100	0.20 (0.21)	0.30* (0.15)	-0.17 (0.14)	-0.17 (0.14)
Education (omitted: Primary education)				
Prevocational secondary	0.46 (3.32)	1.40 (2.26)	-1.03 (2.21)	-0.89 (2.13)
Senior general secondary/pre-university	-3.19 (3.68)	-8.49*** (2.53)	-1.56 (2.47)	-0.64 (2.40)
Senior secondary vocational	-2.72 (3.45)	-2.81 (2.35)	-2.34 (2.33)	-1.94 (2.25)
Higher vocational	0.53 (3.44)	-7.46*** (2.35)	-1.61 (2.30)	-0.54 (2.24)
University	-1.67 (3.93)	-15.64*** (2.70)	0.20 (2.61)	1.99 (2.56)
Employed	12.82*** (1.34)	0.22 (0.99)	-4.88*** (0.91)	-3.89*** (0.90)
HH head lives with a partner	22.04*** (2.37)	-4.14** (1.81)	-10.59*** (1.60)	-8.41*** (1.60)
Own home	10.17*** (1.25)	-2.57*** (0.97)	-1.72** (0.86)	-0.50 (0.87)
# of HH members	-1.65 (2.02)	-1.03 (1.51)	-0.12 (1.34)	-0.22 (1.32)
# of children	0.79 (2.03)	-0.98 (1.52)	0.42 (1.34)	0.59 (1.32)
Social class (omitted: high=1)				
2	-7.86*** (1.70)	-3.55*** (1.29)	-2.72** (1.20)	-2.99** (1.17)
3	-9.78*** (1.81)	-5.32*** (1.37)	-2.80** (1.26)	-3.09** (1.24)
4	-14.31*** (2.17)	-3.55** (1.59)	-2.20 (1.48)	-3.02** (1.46)
5 (low)	-13.59*** (4.40)	2.74 (3.40)	-7.04** (3.35)	-8.24** (3.23)
Region (omitted: Three major cities)				
Rest west	-3.66** (1.63)	-1.21 (1.25)	-2.36** (1.12)	-2.46** (1.09)
North	-3.41* (2.00)	4.94*** (1.58)	-0.41 (1.35)	-1.15 (1.32)
East	-3.92** (1.70)	1.79 (1.30)	-2.45** (1.16)	-2.93*** (1.13)
South	-1.96 (1.70)	2.14* (1.30)	-1.27 (1.15)	-1.50 (1.13)
Log (own income)		0.67*** (0.01)		-0.06*** (0.01)
Observations	2,436	2,421	2,377	2,379
R-squared	0.27	0.69	0.17	0.19

Notes: The table reports results of regressing perceived relative income (columns (1) and (2)) and the absolute value of the perceived income gap (columns (3) and (4)) on observable household and individual characteristics. Social class is similar to Duncan Socioeconomic Index used in IPUMS and elsewhere. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 3. Predicted Comparative Statics with Respect to Relative Income

Outcome	Comparative static	Implication of lower R
Panel A. Spending responses		
Total spending, work time fixed	$\left. \frac{d \ln X}{d \ln R} \right _{SR} \approx (1 - \omega_l) \frac{\sigma - 1}{\theta} (\mu s_d - \gamma).$	Ambiguous. If $\gamma > \mu s_d$ and $\sigma > 1$, a decline in R raises total spending.
Durables relative to nondurables	$\frac{d \ln(c_d/c_n)}{d \ln R} = -\mu(\varepsilon - 1).$	If $\mu > 0$ and $\varepsilon > 1$, a decline in R raises durables relative to nondurables.
Panel B. Socializing and happiness, work time fixed		
Time spent socializing	$\left. \frac{d \ln s}{d \ln R} \right _l = (1 - \chi_s) \frac{\delta}{1 - \nu}.$	If $\delta > 0$, a decline in R reduces time spent socializing.
Happiness	$\left. \frac{dV}{d \ln R} \right _l = C^{1-\sigma}(\gamma - \mu s_d) + \alpha + \beta \delta s^\nu R^\delta.$	Ambiguous overall. Consumption, direct status, and socializing all contribute.
Panel C. Work adjustment and persistence		
Work time	$\Phi \equiv \frac{d \ln l}{d \ln R}$	Ambiguous. Financial and time-value channels can work in opposite directions.
Relative income after work adjustment	$\frac{d \ln R}{d \ln y^{peer}} = -\frac{1}{1 - (1 - \tau)\Phi}.$	If $\Phi < 0$, work adjustment attenuates the initial fall in R ; if $\Phi > 0$, it amplifies it.

Notes: $R = y/y_{peer}$. X denotes total consumption expenditure, s_d the durable expenditure share, ω_l the share of expenditure financed from current labor income, χ_s the share of non-work time devoted to socializing, and Φ is the elasticity of work time with respect to relative income. SR denotes the short run, with work time and own income fixed. A decline in R corresponds to an upward revision in perceived peer income, holding own income fixed.

Table 4: Effects of Treatments on Perceived Peer Income

Dep. var.: Peer income	Wave 1B			Wave 2	Wave 3
	All	Bad news	Good news	All	All
	(1)	(2)	(3)	(4)	(5)
$Prior_i$	0.812*** (0.015)	0.813*** (0.021)	0.858*** (0.028)	0.408*** (0.018)	0.784*** (0.030)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.268*** (0.020)	-0.229*** (0.037)	-0.322*** (0.045)	-0.298*** (0.021)	-0.620*** (0.036)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.271*** (0.020)	0.233*** (0.038)	0.321*** (0.044)	0.301*** (0.021)	0.623*** (0.037)
Observations	1,257	708	510	1,268	1,272
R-squared	0.718	0.708	0.721	0.348	0.389
F-stat	95.84	19.34	32.25	102.1	146.7

Notes: The table reports estimation results for specification (4.2). Column (2) “bad” news reports estimates for the subsample of individuals who learn that peers’ income was higher than they thought. Column (3) “Good” news reports estimates for the subsample of individuals who learn that peers’ income was lower than they thought. Columns (1), (4) and (5) are estimated on full samples. All regressions are estimated with Huber robust method. F-statistic is reported for the test that the coefficients on $Prior_i \times \mathbb{I}(i \in Treatment)$ and $\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$ being jointly zero. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 5: Effects of Perceived Peer Income Standing on Spending Decisions and Borrowing

Dep. Var. title of the panel	Sample		
	Full	Bad news	Good news
	(1)	(2)	(3)
Panel A. Spending on non-durable goods, log(spending).			
Posterior about peer $\mathbb{E}$ income	-1.86 (1.31) [-5.3, 0.8]	-2.32** (1.01) [-7.5, -0.6]	-3.76* (2.00) [-19.7, -0.6]
Observations	2,965	1,225	1,632
1st stage F-stat	13.14	12.33	5.621
Panel B. Spending on durable goods, log(spending).			
Posterior about peer $\mathbb{E}$ income	2.77 (1.92) [-1.2, 7.5]	6.88*** (2.52) [3.6, 15.4]	-4.26 (3.33) [-27.4, 6.2]
Observations	3,150	1,305	1,723
1st stage F-stat	14.07	8.500	3.640
Panel C. Spending on durable goods, extensive margin, linear probability model.			
Posterior about peer $\mathbb{E}$ income	0.36 (0.36) [-0.4, 1.2]	1.29*** (0.48) [0.7, 3.1]	-1.40* (0.77) [-18.3, 0.2]
Observations	3,263	1,354	1,782
1st stage F-stat	13.47	7.990	2.960
Panel D. Take out a new loan or mortgage, linear probability model.			
Posterior about peer $\mathbb{E}$ income	-0.10 (0.25) [-0.6, 0.3]	0.02 (0.24) [-0.5, 0.5]	0.25 (0.24) [-0.14, 1.1]
Observations	1,142	472	629
1st stage F-stat	12.64	7.936	6.946
Panel E. Total spending, log(spending)			
Posterior about peer $\mathbb{E}$ income	-1.98 (1.24) [-5.4, 0.7]	-1.99** (0.94) [-9.3, -0.3]	-4.01* (2.37) [∞ , -0.6]
Observations	2,898	1,194	1,600
1st stage F-stat	13.18	12.42	3.792

Notes: The table reports estimated coefficient c_1 in specification (5.2) pooled across all follow-up waves for various measures of consumer spending and loan taking. Columns (1)-(3) use peer income as the regressor of interest. The posteriors are instrumented as in equation (5.1). Panel titles indicate which measure is used as an outcome variable. Column (1) reports estimates for the full sample. Column (2) reports estimates for the subsample where the income reported in the information treatment is greater than the prior peer estimated income. Column (3) reports estimates for the subsample where the income reported in the information treatment is lower than the prior peer estimated income. 1st stage F-stat report the F-test for the coefficients on instrumental variables being equal to zero. Outliers and influential observations are identified and removed according to the procedure described in Coibion et al. (2023). Robust standard errors clustered by household are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels. Weak-IV-robust 95 percent confidence sets are reported in brackets.

Table 6: Effects of Perceived Standing Relative to Peers on Socialization

Dep. Var. title of the panel “How often in the last month have you...”	Sample		
	Full	Bad news	Good news
	(1)	(2)	(3)
Panel A. Meetings with friends, neighbors, acquaintances or colleagues outside working hours.			
Posterior about relative $\mathbb{E}$ income	6.52* (3.78) [-0.5, 16.7]	0.29 (2.80) [-5.8, 7.5]	8.59 (7.82) [- ∞ , ∞]
Observations	1,116	459	601
1st stage F-stat	11.16	9.408	2.654
Panel B. Visits of a sports club, community center or other club.			
Posterior about relative $\mathbb{E}$ income	4.39 (3.64) [-2.5, 13.7]	-3.05 (3.09) [-13.0, 4.4]	8.20* (4.92) [-1.4, 24.2]
Observations	1,124	451	603
1st stage F-stat	12.89	6.437	6.988
Panel C. Participation in a political or social organization/activities.			
Posterior about relative $\mathbb{E}$ income	1.00 (0.99) [-1.0, 3.3]	0.33 (0.97) [-1.6, 2.8]	0.64 (1.95) [-13, 11]
Observations	1,129	465	609
1st stage F-stat	13.95	13.26	4.244

Notes: The table reports estimated b_1 in specification (5.3) for various measures of socialization where posterior beliefs about relative income positions are instrumented with treatment dummies interacted with priors and signals. Panel titles indicate which measure is used as an outcome variable. Columns (1)-(3) use relative income as the regressor of interest. Socialization is measured from the following question: “We are interested in activities that you may do. How often in the last month have you: (1) Met with friends, neighbors, acquaintances, or people at work (after working hours); (2) Gone to a sport, social or other kind of club; (3) Taken part in a political or community-related organization/activities”; responses are coded as 0 (“0 times”), 1.5 (“1-2 times”), 3 (“3-4 times”), 7 (“5-9 times”), 12.5 (“10 times or more”). Column (1) reports estimates for the full sample. Column (2) reports estimates for the subsample where the income reported in the information treatment is greater than the prior peer estimated income. Column (3) reports estimates for the subsample where the income reported in the information treatment is smaller than the prior peer estimated income. Outliers and influential observations are identified and removed according to the procedure described in Coibion et al. (2023). Robust standard errors clustered by household are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels. Weak-IV-robust 95 percent confidence sets are reported in brackets.

Table 7: Effects of Perceived Peer Income Standing on Happiness

	Dependent variable: Happiness					
	OLS	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Posterior log ($\mathbb{E}HH$ income/ $\mathbb{E}Peer$ HH income)	0.21*** (0.05)	0.60*** (0.20)	0.57*** (0.20)	0.50*** (0.19)	0.65*** (0.20)	0.48** (0.21)
Income inequality in the Netherlands is too large			0.01 (0.02)			0.02 (0.02)
Time spent with friends/colleagues/neighbors				0.01** (0.01)		0.01* (0.01)
Spending on non-durables, log(spending)					-0.01 (0.01)	-0.02 (0.02)
Spending on durables, log(spending)					0.01 (0.01)	0.01 (0.01)
Observations	1,101	1,111	1,106	1,106	926	920
R ²	0.09	0.07	0.08	0.09	0.08	0.12

Notes: The table reports estimated coefficient b_1 in specification (5.3). All IV specifications are estimated using instrumental variables approach where posteriors are instrumented with treatment dummies interacted with priors and signals. Outliers and influential observations are identified and removed according to the procedure described in Coibion et al. (2023). Robust standard errors clustered by household are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 8: Effects of Perceived Standing Relative to Peers on Employment and Expected Income

Panel A: Employment status	$\mathbb{I}(\text{employed})$		
	Full	Bad news	Good news
	(1)	(2)	(3)
Posterior Log ($\mathbb{E}\text{HH income}/\mathbb{E}\text{Peer HH income}$)	0.04 (0.08) [-0.05, 0.2]	-0.35*** (0.11) [-0.6, -0.2]	0.18 (0.27) [-0.6, 0.8]
Observations	1,236	500	677
1st stage F-stat	119	54.99	11.94

Panel B: Expected HH income	Log ($\mathbb{E}\text{HH income}$)		
	Full	Bad news	Good news
	(1)	(2)	(3)
Posterior Log ($\mathbb{E}\text{Peer HH income}$)	0.48 (0.30) [-0.02, 1.2]	0.57* (0.31) [-0.1, 1.5]	0.68 (0.50) [-0.3, 17.3]
Observations	976	397	539
1st stage F-stat	15.43	9.073	4.308

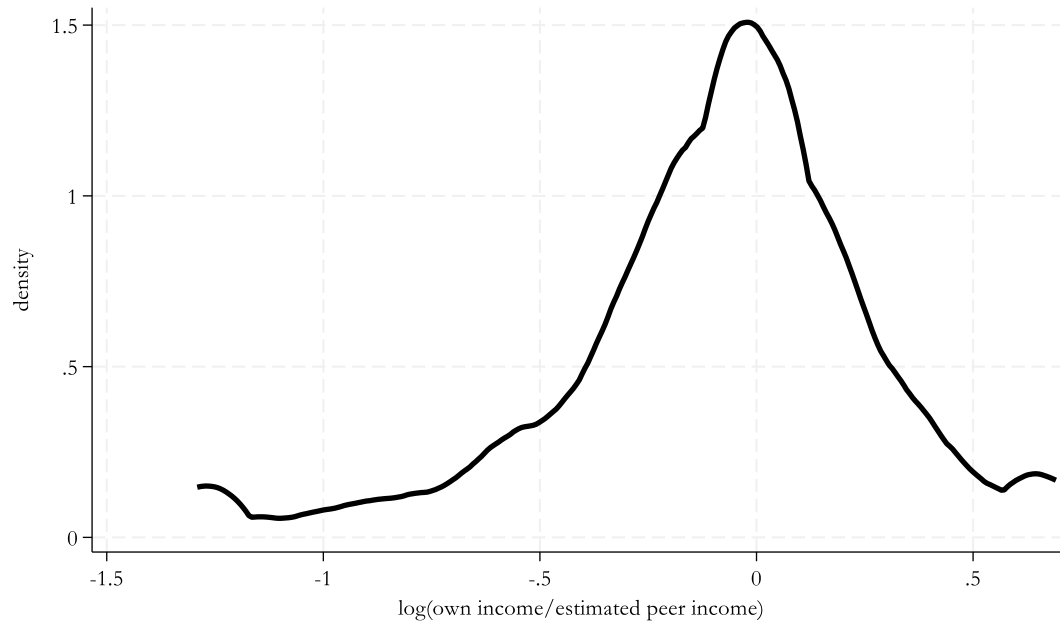
Notes: Panel A of the table reports estimated coefficient b_1 in specification (5.3) when regressing employment in follow-up waves on an individual's posterior relative perceived income. Panel B of the table reports estimated coefficient c_1 in specification (5.4) when regressing log of expected income for 2020 in follow-up waves on the log of an individual's posterior belief about peer incomes. Column (1) reports estimates for the full sample. Column (2) reports estimates for the subsample where the income reported in the information treatment is greater than the prior peer estimated income (bad news) while Column (3) reports estimates for the subsample where the income reported in the information treatment is smaller than the prior peer estimated income (good news). 1st stage F-stat report the F-test for the coefficients on instrumental variables being equal to zero. Outliers and influential observations are identified and removed according to the procedure described in Coibion et al. (2023). Robust standard errors clustered by household are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels. Weak-IV-robust 95 percent confidence sets are reported in brackets.

Table 9: Effects of Perceived Standing Relative to Peers on Beliefs about Equity and Redistribution

Dep. Var. title of the panel To what extent do you agree with the statement	Sample		
	Full	Bad news	Good news
	(1)	(2)	(3)
Panel A. Differences in incomes in the Netherlands are too large.			
Posterior about relative $\mathbb{E}$ income	-2.15*** (0.66) [-3.5, -0.9]	-1.33 (1.07) [-3.6, 1.0]	-2.36 (1.96) [-7.1, 2.1]
Observations	1,119	448	607
1st stage F-stat	91.37	27.56	9.75
Panel B. Incomes should be made more equal.			
Posterior about relative $\mathbb{E}$ income	-1.80** (0.71) [-3.1, -0.4]	-3.16** (1.29) [-6.1, -0.6]	1.04 (2.08) [-3.3, 6.0]
Observations	1,115	448	611
1st stage F-stat	95.29	31.34	11.50
Panel C. Differences in income are an incentive for individual effort.			
Posterior about relative $\mathbb{E}$ income	1.60*** (0.60) [0.5, 2.8]	1.37 (0.99) [-0.5, 3.6]	2.80* (1.65) [-0.6, 6.8]
Observations	1,117	444	616
1st stage F-stat	112.4	38.09	15.96
Panel D. In the long run, hard work usually brings a better life.			
Posterior about relative $\mathbb{E}$ income	0.73 (0.68) [-0.7, 2.0]	0.59 (0.93) [-1.4, 2.5]	-4.75* (2.65) [-12.2, 0.5]
Observations	1,115	446	610
1st stage F-stat	91.08	35.61	7.6
Panel E. Hard work does not generally bring success – it is more a matter of luck and connections.			
Posterior about relative $\mathbb{E}$ income	-0.19 (0.69) [-1.6, 1.2]	1.97* (1.10) [-0.2, 4.6]	3.34* (1.94) [-0.8, 7.8]
Observations	1,115	447	608
1st stage F-stat	97.66	27.98	13.62
Panel F. Cheating on taxes if you have a chance can never be justified.			
Posterior about relative $\mathbb{E}$ income	0.95 (0.76) [-0.6, 2.5]	-2.30* (1.32) [-5.4, 0.3]	2.35 (2.20) [-1.9, 8.2]
Observations	1,115	441	611
1st stage F-stat	114.4	32.0	15.18
Panel G. Differences in wealth in the Netherlands are too large.			
Posterior about relative $\mathbb{E}$ income	-1.61** (0.64) [-2.9, -0.4]	-1.90* (1.03) [-4.3, 0.2]	0.95 (1.67) [-2.6, 4.7]
Observations	1,111	446	611
1st stage F-stat	100.8	30.18	17.22

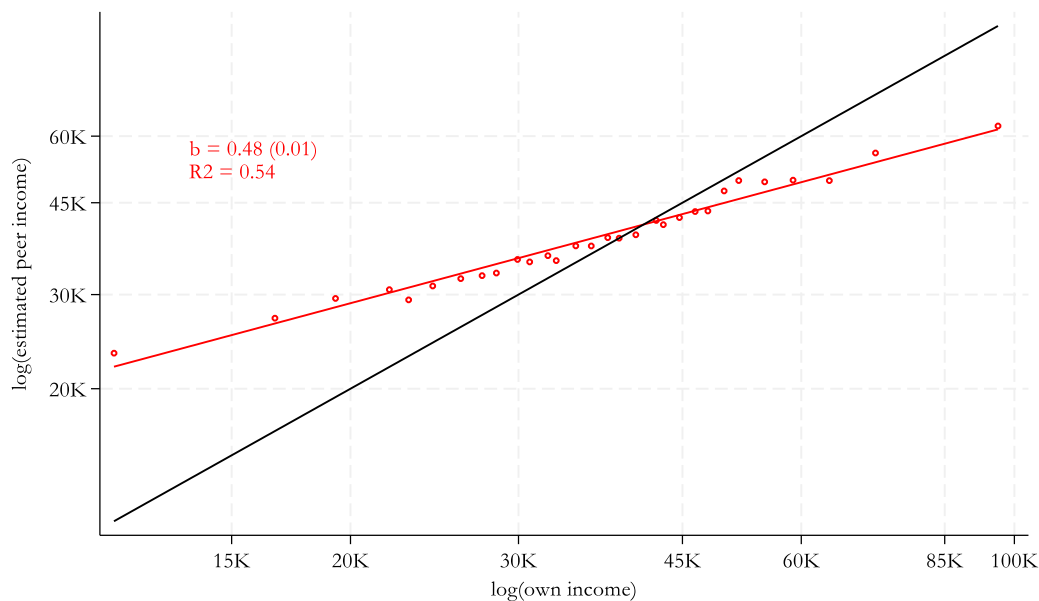
Notes: The table reports estimated b_1 in specification (5.3) where posterior beliefs about relative income positions are instrumented with treatment dummies interacted with priors and signals. Panel titles indicate which measure is used as an outcome variable. Columns (1)-(3) use relative income as the regressor of interest. Column (1) reports estimates for the full sample. Column (2) reports estimates for the subsample where the income reported in the information treatment is greater than the prior peer estimated income. Column (3) reports estimates for the subsample where the income reported in the information treatment is smaller than the prior peer estimated income. Views are measured on a scale of 1 to 10, with 10 meaning “completely agree”. Outliers and influential observations are identified and removed according to the procedure described in Coibion et al. (2023). Robust standard errors clustered by household are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels. Weak-IV-robust 95 percent confidence sets are reported in brackets.

Figure 1: Distributions of Perceived Standings Relative to Peers.



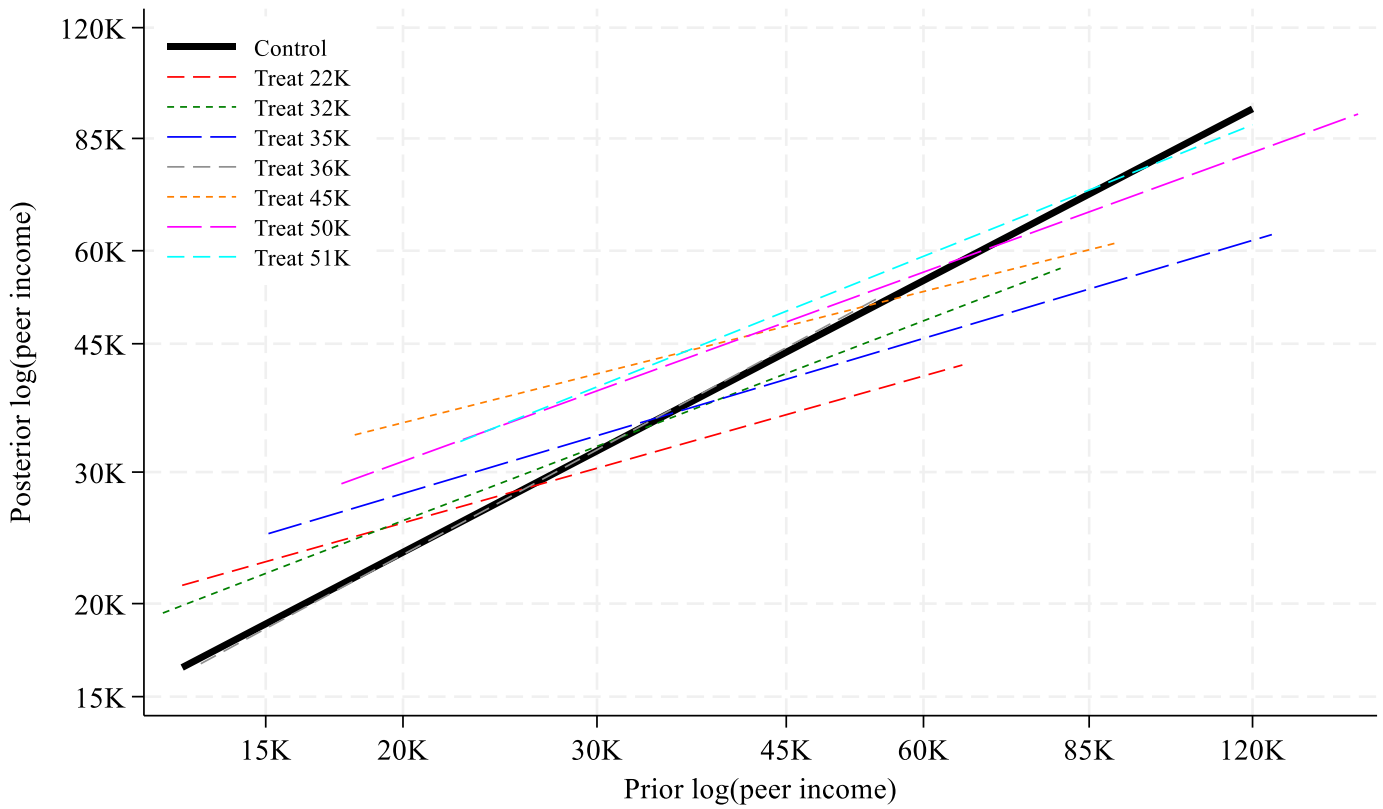
Notes: The figure plots the kernel density of perceived relative income, defined as the log ratio of own to estimated peer income as measured in wave 1A. Values above zero indicate respondents who believe they earn more than their peers. Peers are defined by respondents themselves as friends, neighbours, acquaintances, or people at work.

Figure 2: Perceived Peer Income vs. Own Income.



Notes: The figure shows a binscatter of estimated peer income against own income (measured in wave 1A). The solid black line is the 45° line, along which respondents believe their peers earn exactly what they do. A fitted slope below one indicates that beliefs about peers are compressed relative to own circumstances.

Figure 3: Effects of Information Treatments on Perceived Income of Peers.



Notes: The figure shows fitted linear relationships between posterior beliefs about peer income, measured in wave 1B, and prior beliefs, measured in wave 1A. The solid thick black line is the control group, which received no information; the remaining lines are treatment groups (selected) defined by the signal received, as indicated in the legend.

ONLINE APPENDIX

Appendix A: Additional Tables and Figures

Appendix Table A1. Treatment Effects on Perceived Relative Peer Standings, by Gender.

Female VARIABLES	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior_i$	0.807*** (0.022)	0.802*** (0.030)	0.860*** (0.042)	0.372*** (0.023)	0.819*** (0.041)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.250*** (0.027)	-0.199*** (0.047)	-0.299*** (0.060)	-0.311*** (0.027)	-0.663*** (0.050)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.254*** (0.027)	0.203*** (0.048)	0.300*** (0.059)	0.314*** (0.027)	0.666*** (0.050)
Observations	590	355	218	592	594
R-squared	0.731	0.686	0.778	0.321	0.415
F-stat	44.96	9.095	16.08	66.99	89.50
Male VARIABLES	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior$	0.818*** (0.021)	0.827*** (0.028)	0.855*** (0.038)	0.448*** (0.025)	0.747*** (0.040)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.291*** (0.029)	-0.265*** (0.055)	-0.379*** (0.067)	-0.294*** (0.032)	-0.579*** (0.051)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.294*** (0.029)	0.269*** (0.056)	0.376*** (0.066)	0.296*** (0.033)	0.581*** (0.052)
Observations	667	353	292	676	678
R-squared	0.704	0.728	0.652	0.380	0.359
F-stat	52.33	11.71	19.17	41.58	63.55

Notes: See notes to Table 4. Estimates are for women in top panels and men in bottom panels.

Appendix Table A2. Treatment Effects on Perceived Relative Peer Standings, by Age.

Age≤50 VARIABLES	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior_i$	0.812*** (0.027)	0.792*** (0.033)	0.880*** (0.050)	0.417*** (0.031)	0.798*** (0.046)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.271*** (0.033)	-0.263*** (0.058)	-0.353*** (0.094)	-0.270*** (0.036)	-0.621*** (0.052)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.276*** (0.033)	0.269*** (0.060)	0.353*** (0.092)	0.276*** (0.036)	0.629*** (0.052)
Observations	366	206	149	371	372
R-squared	0.752	0.769	0.730	0.400	0.448
F-stat	35.93	10.63	10.48	30.15	71.82
Age>50 VARIABLES	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior_i$	0.787*** (0.020)	0.800*** (0.028)	0.822*** (0.036)	0.367*** (0.022)	0.735*** (0.039)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.256*** (0.025)	-0.230*** (0.050)	-0.284*** (0.054)	-0.286*** (0.024)	-0.589*** (0.046)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.259*** (0.026)	0.235*** (0.050)	0.283*** (0.053)	0.287*** (0.024)	0.589*** (0.047)
Observations	891	502	361	897	900
R-squared	0.680	0.658	0.697	0.291	0.330
F-stat	52.23	10.80	16.72	74.49	81.78

Notes: See notes to Table 4. Estimates are for those aged less than 50 in top panels and those aged 50 or older in bottom panels.

Appendix Table A3. Treatment Effects on Perceived Relative Peer Standings, by Education.

College or more	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer income					
$Prior_i$	0.813*** (0.028)	0.812*** (0.038)	0.888*** (0.048)	0.429*** (0.030)	0.683*** (0.049)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.284*** (0.037)	-0.268*** (0.067)	-0.475*** (0.083)	-0.282*** (0.042)	-0.561*** (0.056)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.286*** (0.037)	0.273*** (0.068)	0.470*** (0.081)	0.283*** (0.042)	0.560*** (0.056)
Observations	449	197	238	457	462
R-squared	0.694	0.707	0.630	0.356	0.309
F-stat	33.84	8.254	19.54	22.75	49.90
High school or less	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer income					
$Prior_i$	0.784*** (0.021)	0.781*** (0.029)	0.827*** (0.040)	0.356*** (0.023)	0.779*** (0.040)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.236*** (0.025)	-0.206*** (0.045)	-0.249*** (0.054)	-0.275*** (0.025)	-0.590*** (0.049)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.240*** (0.025)	0.209*** (0.046)	0.250*** (0.054)	0.278*** (0.025)	0.595*** (0.050)
Observations	807	510	272	810	809
R-squared	0.688	0.629	0.743	0.264	0.355
F-stat	44.28	11.08	13.56	61.17	71.82

Notes: See notes to Table 4. Estimates are for those with at least some college education in top panels and those with no college education in bottom panels.

Appendix Table A4. Treatment Effects on Perceived Relative Peer Standings, by Income.

High income (above median)	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior_i$	0.765*** (0.026)	0.794*** (0.033)	0.802*** (0.053)	0.445*** (0.033)	0.512*** (0.043)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.223*** (0.036)	-0.168*** (0.063)	-0.314*** (0.086)	-0.313*** (0.037)	-0.482*** (0.048)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.225*** (0.036)	0.171*** (0.065)	0.313*** (0.084)	0.315*** (0.037)	0.484*** (0.049)
Observations	635	343	270	632	640
R-squared	0.630	0.656	0.537	0.302	0.205
F-stat	21.96	3.542	9.674	35.51	49.91
Low income (below median)	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior_i$	0.750*** (0.025)	0.694*** (0.038)	0.828*** (0.044)	0.298*** (0.026)	0.452*** (0.045)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.243*** (0.027)	-0.229*** (0.043)	-0.295*** (0.061)	-0.237*** (0.025)	-0.380*** (0.046)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.246*** (0.027)	0.233*** (0.044)	0.295*** (0.060)	0.239*** (0.025)	0.379*** (0.047)
Observations	622	365	240	636	632
R-squared	0.613	0.500	0.670	0.189	0.161
F-stat	41.95	14.90	19.41	44.68	35.07

Notes: See notes to Table 4. Estimates for those with high income in top panels and those with low income in bottom panels.

Appendix Table A5. Treatment Effects on Perceived Relative Peer Standings, by Debt.

High debt (above median)	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior_i$	0.828*** (0.020)	0.824*** (0.029)	0.905*** (0.038)	0.467*** (0.027)	0.736*** (0.042)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.277*** (0.029)	-0.243*** (0.057)	-0.386*** (0.068)	-0.296*** (0.038)	-0.546*** (0.049)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.281*** (0.029)	0.249*** (0.058)	0.385*** (0.067)	0.300*** (0.038)	0.546*** (0.049)
Observations	574	321	228	571	580
R-squared	0.748	0.748	0.730	0.400	0.388
F-stat	50.83	9.288	22.25	31.71	62.81
Low debt (below median)	Wave 1B			Wave 2	Wave 3
	All	“good” news	“bad” news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer $\mathbb{E}$ income					
$Prior_i$	0.754*** (0.025)	0.753*** (0.036)	0.797*** (0.043)	0.332*** (0.026)	0.674*** (0.048)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.240*** (0.029)	-0.193*** (0.052)	-0.251*** (0.067)	-0.252*** (0.028)	-0.558*** (0.055)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.242*** (0.029)	0.195*** (0.053)	0.250*** (0.066)	0.253*** (0.028)	0.561*** (0.055)
Observations	571	322	235	581	579
R-squared	0.638	0.605	0.657	0.241	0.286
F-stat	34.53	8.530	9.489	41.98	51.64

Notes: See notes to Table 4. Estimates for those with high income in top panels and those with low income in bottom panels.

Appendix Table A6. Effects of Treatments on Perceived Relative Peer Standing, Joint Treatment Effects.

	Posterior	
	Peer $\mathbb{E}$ Income	
	Wave 1B	Wave 2
	(1)	(2)
$Prior \mathbb{E} Income_i$	0.711*** (0.016)	0.396*** (0.018)
$Prior \mathbb{E} Income_i \times \mathbb{I}(i \in Treatment Income)$	-0.188*** (0.023)	-0.275*** (0.024)
$Prior \mathbb{E} Income_i \times \mathbb{I}(i \in Treatment debt)$	-0.012 (0.008)	-0.016* (0.010)
$\mathbb{I}(i \in Treatment Income) \times TreatmentIncomeSignal_i$	0.209*** (0.023)	0.297*** (0.024)
$Prior Debt_i$	0.030*** (0.005)	0.025*** (0.006)
$Prior Debt_i \times \mathbb{I}(i \in Treatment Income)$	-0.017** (0.007)	-0.017** (0.008)
$Prior Debt_i \times \mathbb{I}(i \in Treatment debt)$	-0.032*** (0.007)	-0.013 (0.009)
$\mathbb{I}(i \in Treatment Debt) \times TreatmentDebtSignal_i$	0.046*** (0.005)	0.029*** (0.007)
Observations	1,493	1,512
R-squared	0.711	0.400
F-stat: Income treatment	33.64	54.49
F-stat: Debt treatment	25.01	8.646

Notes: The table reports results when the specification (4.2) includes both income and debt variables at the same time. See notes to Table 4.

Appendix Table A7. Effects of Relative Perceived Income on Happiness: Those Receiving “Bad” News

	Dependent variable: Happiness					
	OLS	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Posterior log ($\mathbb{E}$ HH income/ $\mathbb{E}$ Peer HH income)	0.28*** (0.09)	0.10 (0.31)	0.21 (0.31)	0.13 (0.31)	0.11 (0.34)	0.35 (0.31)
Income in the Netherlands are too large			0.02 (0.02)			0.01 (0.02)
Time spent with friends/colleagues/neighbors				0.01* (0.01)		0.02*** (0.01)
Spending on non-durables, log(spending)					-0.02 (0.02)	-0.02 (0.02)
Spending on durables, log(spending)					0.00 (0.01)	0.00 (0.01)
Observations	442	445	444	443	367	365
R ²	0.09	0.10	0.11	0.11	0.14	0.16

Notes: See notes to Table 7.

Appendix Table A8. Effects of Relative Perceived Income on Happiness: Receiving “Good” News

	Dependent variable: Happiness					
	OLS	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Posterior log ($\mathbb{E}$ HH income/ $\mathbb{E}$ Peer HH income)	0.25*** (0.07)	1.44*** (0.50)	1.19*** (0.44)	1.11*** (0.43)	-0.22 (0.53)	-0.32 (0.47)
Income in the Netherlands are too large			0.03 (0.03)			-0.01 (0.02)
Time spent with friends/colleagues/neighbors				0.01** (0.01)		0.00 (0.01)
Spending on non-durables, log(spending)					0.00 (0.02)	0.00 (0.02)
Spending on durables, log(spending)					0.02** (0.01)	0.02* (0.01)
Observations	599	606	603	605	491	490
R ²	0.11	0.03	0.08	0.07	0.11	0.11

Notes: See notes to Table 7.

Appendix Table A9: Effects of Perceived Peer Income Standing on Equity and Redistributive Concerns by Subsamples

Dep. var. is indicated in the title of the panel To what extent do you agree with	Gender		Age		Education		Income		Debt	
	Fem.	Male	<=50	>50	Coll.+	HS-	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Differences in incomes in the Netherlands are too large.										
Posterior log (EHH income/EPeer HH income)	-2.17** (0.90)	-2.31*** (0.87)	-3.08** (1.20)	-1.80** (0.75)	-2.41** (1.00)	-2.27*** (0.86)	-1.11 (1.37)	-2.21** (0.89)	-1.94** (0.98)	-1.91** (0.95)
Observations	521	598	309	810	407	712	541	578	497	527
1st stage F-stat	41.01	50.71	23.56	69.27	35.88	57.98	22.02	51.59	39.94	42.36
Panel B: Incomes should be made more equal.										
Posterior log (EHH income/EPeer HH income)	-1.96* (1.00)	-1.78* (0.92)	-2.15* (1.20)	-1.80** (0.84)	-2.07** (1.00)	-1.88* (0.96)	-0.27 (1.62)	-2.31*** (0.88)	-0.97 (1.05)	-2.51** (1.10)
Observations	519	596	308	807	408	707	537	578	496	525
1st stage F-stat	42.44	52.75	27.11	68.30	42.37	54.66	18.93	62.66	45.72	42.10
Panel C: Differences in income are an incentive for individual effort.										
Posterior log (EHH income/EPeer HH income)	2.16** (0.96)	1.43* (0.81)	1.94** (0.89)	1.52* (0.80)	1.09 (0.84)	1.97** (0.83)	3.61*** (1.37)	0.82 (0.77)	0.93 (0.89)	2.05** (0.89)
Observations	521	596	308	809	407	710	541	576	497	525
1st stage F-stat	47.26	64.14	36.07	71.03	51.12	62.53	24.08	73.88	52.42	48.60
Panel D: In the long run, hard work usually brings a better life.										
Posterior log (EHH income/EPeer HH income)	-0.27 (0.82)	1.92* (1.03)	0.08 (1.30)	1.35* (0.80)	1.48 (0.97)	0.40 (0.88)	0.29 (1.50)	0.74 (0.90)	0.14 (0.98)	0.80 (0.98)
Observations	521	594	306	809	406	709	538	577	495	525
1st stage F-stat	47.67	42.81	22.38	71.03	35.78	57.17	18.82	55.70	36.72	46.67
Panel E: Hard work does not generally bring success – it is more a matter of luck and connections.										
Posterior log (EHH income/EPeer HH income)	-0.70 (0.93)	0.19 (0.97)	1.01 (1.19)	-0.76 (0.83)	-0.05 (1.12)	-0.15 (0.85)	0.42 (1.46)	0.07 (0.85)	0.19 (1.02)	-0.61 (0.97)
Observations	520	595	307	808	404	711	539	576	497	523
1st stage F-stat	45.80	49.84	28.24	69.17	39.31	60.98	23.57	60.41	44.14	44.16
Panel F: Cheating on taxes if you have a chance can never be justified.										
Posterior log (EHH income/EPeer HH income)	0.44 (0.96)	1.54 (1.16)	0.27 (1.35)	1.28 (0.90)	1.03 (1.03)	0.76 (1.03)	2.54 (1.68)	0.18 (1.00)	1.56 (1.03)	-0.73 (1.18)
Observations	523	592	307	808	402	713	538	577	496	525
1st stage F-stat	57.81	56.48	27.24	90.34	46.47	69.06	30.06	64.31	58.29	48.93
Panel G: Differences in wealth in the Netherlands are too large.										
Posterior log (EHH income/EPeer HH income)	-2.49*** (0.89)	-1.12 (0.89)	-2.90** (1.19)	-1.26* (0.75)	1.60 (0.98)	-1.88** (0.83)	-0.60 (1.44)	-2.17*** (0.25)	-1.70* (0.90)	-1.63* (0.93)
Observations	519	592	304	807	400	711	532	579	490	527
1st stage F-stat	40.62	60.37	27.92	72.38	43.74	61.03	22.61	59.46	49.74	43.65
Panel H: Happiness										
Posterior log (EHH income/EPeer HH income)	0.56** (0.28)	0.67** (0.28)	0.54 (0.40)	0.67*** (0.23)	0.38 (0.26)	0.80*** (0.27)	0.40 (0.43)	0.60** (0.25)	0.34 (0.29)	0.55* (0.29)
Observations	513	598	303	808	404	707	538	573	500	525
1st stage F-stat	47.17	54.68	25.52	81.03	41.83	68.04	26.85	63.49	52.43	45.47

Notes: the table reproduces Tables 7 and 9 by subsamples.

Appendix Table A10: Effects of Perceived Peer Income Standing on Socialization by Subsamples

Dep. var. is indicated in the title of the panel	Gender		Age		Education		Income		Debt	
	Fem.	Male	<=50	>50	Coll.+	HS-	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Meetings with friends, neighbors, acquaintances or colleagues outside working hours.										
Posterior log ($\mathbb{E}$ HH income/ $\mathbb{E}$ Peer HH income)	4.25 (4.68)	8.18 (5.88)	10.38** (5.11)	2.87 (5.07)	9.20* (5.55)	5.22 (5.14)	-6.92 (9.19)	7.81** (3.84)	3.58 (3.82)	3.67 (6.00)
Observations	521	595	310	806	405	711	536	580	496	526
1st stage F-stat	6.640	4.470	5.466	6.093	5.141	5.574	1.996	13.44	8.923	4.900
Panel B. Visits of a sports club, community center or other club.										
Posterior log ($\mathbb{E}$ HH income/ $\mathbb{E}$ Peer HH income)	2.46 (5.68)	6.93 (4.65)	1.49 (5.63)	4.31 (5.09)	8.16 (6.43)	4.97 (4.36)	10.12 (9.95)	3.14 (3.26)	1.09 (4.34)	6.55 (6.42)
Observations	520	604	306	818	408	716	542	582	503	530
1st stage F-stat	6.266	6.868	5.322	6.915	4.705	7.717	2.658	14.07	9.901	4.240
Panel C. Participation in a political or social organization/activities.										
Posterior log ($\mathbb{E}$ HH income/ $\mathbb{E}$ Peer HH income)	-1.24 (1.33)	2.66* (1.58)	0.79 (1.13)	2.11 (1.73)	1.62 (1.54)	0.62 (1.26)	1.31 (2.38)	0.47 (0.92)	2.68** (1.18)	-1.78 (1.92)
Observations	524	605	314	815	412	717	544	585	504	532
1st stage F-stat	6.588	7.008	7.132	6.290	7.347	6.472	2.562	15.32	11.17	4.307

Notes: the table reproduces Table 6 by subsamples.

Appendix Table A11: P-values adjusted for multiple hypothesis testing.

	EIncome	
	Raw	Adj.
	(1)	(2)
Table 9		
Differences in incomes in the Netherlands are too large.	0.0019	0.0010
Incomes should be made more equal.	0.0132	0.0040
Differences in income are an incentive for individual effort.	0.0953	0.1289
In the long run, hard work usually brings a better life.	0.3245	0.4126
Hard work does not generally bring success – it is more a matter of luck and connections	0.9634	0.9610
Cheating on taxes if you have a chance can never be justified	0.2216	0.4126
Differences in wealth in the Netherlands are too large	0.0127	0.0040
Table 6		
Meetings with friends, neighbors, acquaintances or colleagues outside working hours	0.0171	0.0040
Visits of a sports club, community center or other club	0.0552	0.0559
Participation in a political or social organization/activities	0.4815	0.7093
Table 8		
Employment status	0.0188	0.0040
Log(HH income)	0.0000	0.0010
Table 5		
Spending on non-durable goods, log(spending).	0.0041	0.0020
Spending on durable goods, log(spending).	0.0020	0.0010
Spending on durable goods, extensive margin, linear probability model.	0.0022	0.0010
Take out a new loan or mortgage, linear probability model.	0.2309	0.4126
Table 7		
Happiness	0.8494	0.9610

Notes: The table reports (“raw”) p-values for coefficients reported in Tables 5-9 as well as p-values adjusted (“Adj.”) for multiple hypothesis testing (Romano and Wolf 2016).

Appendix Table A12: Balance across Treatment Arms

	Control group (1)	Income treatment (2)
Age	55.60 (16.07)	56.72 (15.87)
Female	0.49 (0.50)	0.48 (0.50)
Education: Post-secondary	0.36 (0.48)	0.38 (0.48)
Education: Secondary	0.36 (0.48)	0.32 (0.47)
Education: Less than secondary	0.28 (0.45)	0.30 (0.46)
Number of family members in the household	2.24 (1.21)	2.24 (1.14)
Spending on nondurable goods and services in the last month, €	809.28 (602.44)	790.91 (554.44)
Spending on durable goods in the last month, €	1135.69 (2190.28)	1093.37 (2156.68)
Share of respondents reporting positive spending on durables	0.45 (0.50)	0.47 (0.50)
Household net annual income, ‘000€	37.58 (19.36)	39.33 (29.59)
Household total debt, ‘000€	81.49 (111.14)	94.04 (166.85)
Peer estimated net annual income, ‘000€	41.15 (31.23)	40.15 (22.08)
Peer estimated total debt, ‘000€	121.03 (177.58)	117.47 (175.36)
Share of respondents not being able to report peer estimated income	0.23 (0.42)	0.22 (0.41)
Share of respondents not being able to report peer estimated debt	0.32 (0.47)	0.32 (0.47)

Notes: The table reports means and standard deviations (in parentheses) for different observable characteristics of respondents in each treatment arm. The first row of the table indicates treatment arms for which moments are reported.

Appendix Table A13. Reduced-form estimates corresponding to Table 9.

Dep. Var. title of the panel To what extent do you agree with the statement	Posterior for relative $\mathbb{E}$ income log ($\mathbb{E}$ HH income/ $\mathbb{E}$ Peer HH income)		
	Full	Bad news	Good news
	(1)	(2)	(3)
Panel A. Differences in incomes in the Netherlands are too large.			
$Prior_i$	-0.75*** (0.25)	-0.85** (0.42)	-1.11*** (0.25)
$Prior_i \times \mathbb{I}(i \in Treatment)$	0.40 (0.33)	-0.20 (0.65)	0.30 (0.39)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	-0.82*** (0.26)	-0.52 (0.46)	-0.46 (0.39)
Panel B. Incomes should be made more equal.			
$Prior_i$	-0.56** (0.26)	-0.82* (0.44)	-0.85*** (0.28)
$Prior_i \times \mathbb{I}(i \in Treatment)$	0.09 (0.36)	0.89 (0.75)	-0.47 (0.43)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	-0.71** (0.29)	-1.32** (0.53)	0.13 (0.42)
Panel C. Differences in income are an incentive for individual effort.			
$Prior_i$	0.18 (0.24)	0.27 (0.37)	0.15 (0.27)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.34 (0.32)	-0.80 (0.61)	-0.22 (0.41)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.64*** (0.24)	0.64 (0.44)	0.68* (0.39)
Panel D. In the long run, hard work usually brings a better life.			
$Prior_i$	0.54** (0.22)	0.79** (0.34)	0.78*** (0.27)
$Prior_i \times \mathbb{I}(i \in Treatment)$	0.08 (0.33)	-0.24 (0.58)	0.61 (0.42)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.26 (0.26)	0.27 (0.43)	-0.72* (0.40)
Panel E. Hard work does not generally bring success – it is more a matter of luck and connections.			
$Prior_i$	-0.58** (0.23)	-0.75* (0.39)	-0.94*** (0.32)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.00 (0.32)	-0.36 (0.61)	-0.62 (0.44)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	-0.07 (0.26)	0.85* (0.49)	0.71 (0.44)
Panel F. Cheating on taxes if you have a chance can never be justified.			
$Prior_i$	0.39 (0.25)	0.81** (0.36)	0.31 (0.32)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.79** (0.40)	1.07 (0.74)	-1.03** (0.49)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.41 (0.32)	-1.10* (0.59)	0.32 (0.50)
Panel G. Differences in wealth in the Netherlands are too large.			
$Prior_i$	-0.66** (0.26)	-1.04** (0.42)	-0.93*** (0.27)
$Prior_i \times \mathbb{I}(i \in Treatment)$	0.29 (0.34)	0.48 (0.68)	-0.19 (0.41)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	-0.64** (0.26)	-0.78* (0.45)	0.22 (0.40)

Notes: The table reports reduced-form estimates corresponding to Table 9. See notes to Table 9 for more details.

Appendix Table A14. Reduced-form estimates corresponding to Table 6.

Dep. Var. title of the panel “How often in the last month have you...”	Posterior for relative income		
	$\log(\mathbb{E}HH \text{ income}/\mathbb{E}Peer \text{ HH income})$		
	Full	Bad news	Good news
	(1)	(2)	(3)
Panel A. Meetings with friends, neighbors, acquaintances or colleagues outside working hours.			
$Prior_i$	0.41 (0.52)	-0.45 (1.04)	0.27 (0.70)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.54 (0.59)	0.21 (1.24)	-0.73 (0.72)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	1.01* (0.55)	-0.02 (0.90)	0.88 (0.78)
Observations	1,116	459	601
R-squared	0.06	0.08	0.08
Panel B. Visits of a sports club, community center or other club.			
$Prior_i$	0.45 (0.59)	-0.82 (0.97)	2.41** (0.96)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-1.00 (0.67)	-0.29 (1.29)	-1.20* (0.72)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.71 (0.60)	-0.53 (0.80)	0.97 (0.91)
Observations	1,124	451	603
R-squared	0.04	0.15	0.07
Panel C. Participation in a political or social organization/activities.			
$Prior_i$	0.26 (0.16)	-0.21 (0.39)	0.37* (0.21)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.18 (0.18)	-0.19 (0.41)	-0.16 (0.17)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.15 (0.16)	0.14 (0.31)	0.35* (0.19)
Observations	1,129	465	609
R-squared	0.04	0.07	0.05

Notes: The table reports reduced-form estimates corresponding to Table 6. See notes to Table 6 for more details.

Appendix Table A15. Reduced-form estimates corresponding to Table 8.

Panel A: Employment status	$\mathbb{I}(\text{employed})$		
	Full	Bad news	Good news
	(1)	(2)	(3)
$Prior_i$	0.04 (0.03)	0.05 (0.05)	0.10** (0.04)
$Prior_i \times \mathbb{I}(i \in \text{Treatment})$	-0.11** (0.04)	0.00 (0.10)	-0.12** (0.05)
$\mathbb{I}(i \in \text{Treatment}) \times \text{TreatmentSignal}_i$	0.02 (0.03)	-0.15*** (0.05)	0.02 (0.05)
Observations	1,236	500	677
R-squared	0.64	0.69	0.68
Panel B: Expected HH income	Log (EHH income)		
	Full	Bad news	Good news
	(1)	(2)	(3)
$Prior_i$	0.14*** (0.05)	0.34** (0.14)	0.05 (0.07)
$Prior_i \times \mathbb{I}(i \in \text{Treatment})$	-0.10** (0.05)	-0.36*** (0.14)	-0.04 (0.08)
$\mathbb{I}(i \in \text{Treatment}) \times \text{TreatmentSignal}_i$	0.10** (0.05)	0.35*** (0.13)	0.04 (0.08)
Observations	976	397	539
R-squared	0.60	0.65	0.60

Notes: The table reports reduced-form estimates corresponding to Table 8. See notes to Table 8 for more details.

Appendix Table A16. Reduced-form estimates corresponding to Table 5.

Dep. Var. title of the panel	Peer estimated $\mathbb{E}$ income		
	Full	Bad news	Good news
	(1)	(2)	(3)
Panel A. Spending on non-durable goods, log(spending).			
$Prior_i$	0.10 (0.19)	-0.31 (0.32)	0.23 (0.25)
$Prior_i \times \mathbb{I}(i \in Treatment)$	0.19 (0.19)	0.48 (0.39)	0.73** (0.29)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	-0.20 (0.19)	-0.49 (0.38)	-0.75** (0.30)
Observations	2,965	1,225	1,632
R-squared	0.11	0.14	0.12
Panel B. Spending on durable goods, log(spending).			
$Prior_i$	0.13 (0.22)	1.41*** (0.42)	-0.35 (0.32)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.31 (0.26)	-2.16*** (0.62)	0.61 (0.46)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.33 (0.27)	2.14*** (0.61)	-0.63 (0.47)
Observations	3,150	1,305	1,723
R-squared	0.09	0.10	0.11
Panel C. Spending on durable goods, extensive margin, linear probability model.			
$Prior_i$	0.02 (0.04)	0.25*** (0.08)	-0.05 (0.06)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.04 (0.05)	-0.42*** (0.12)	0.17** (0.09)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.04 (0.05)	0.41*** (0.12)	-0.17* (0.09)
Observations	3,263	1,354	1,782
R-squared	0.09	0.09	0.11
Panel D. Take out a new loan or mortgage, linear probability model.			
$Prior_i$	-0.04 (0.03)	-0.02 (0.07)	-0.15*** (0.05)
$Prior_i \times \mathbb{I}(i \in Treatment)$	0.01 (0.04)	0.06 (0.10)	-0.04 (0.06)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	-0.01 (0.04)	-0.05 (0.09)	0.04 (0.06)
Observations	1,142	472	629
R-squared	0.04	0.07	0.05
Panel E. Total spending, log(spending).			
$Prior_i$	0.12 (0.17)	-0.13 (0.34)	0.23 (0.23)
$Prior_i \times \mathbb{I}(i \in Treatment)$	0.20 (0.18)	0.20 (0.40)	0.76*** (0.28)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	-0.21 (0.18)	-0.21 (0.39)	-0.78*** (0.30)
Observations	2,898	1,194	1,600
R-squared	0.13	0.15	0.16

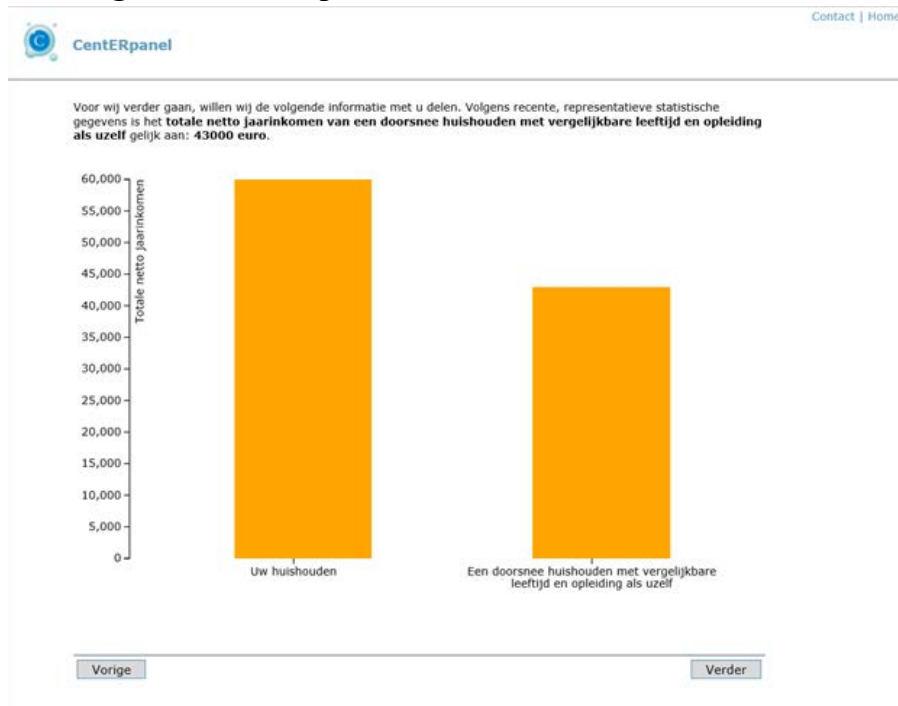
Notes: The table reports reduced-form estimates corresponding to Table 5. See notes to Table 5 for more details.

Appendix Table A17. Responses to hypothetical scenarios

	Unit	Hypothetical scenario: your income is X than your peers' income		
		20% lower	20% higher	Difference
	(1)	(2)	(3)	(4)
<i>Panel A. Unconditional responses</i>				
Satisfied with your life	1 (Extremely dissatisfied) to 5 (Extremely satisfied)	3.43 [0.95]	3.41 [0.96]	-0.02 (0.07)
Happy	1 (Extremely unhappy) to 5 (Extremely happy)	3.61 [0.86]	3.58 [0.90]	-0.03 (0.09)
<i>Panel B. Responses conditional on scenario X</i>				
Fairness	1 (completely unfair) to 7 (completely fair)	3.65 [1.29]	4.59 [1.36]	0.94*** (0.10)
Happiness	1 (very unhappy) to 7 (very happy)	3.03 [1.10]	4.28 [1.04]	1.25*** (0.08)
Financial situation	1 (much worse) to 7 (much better)	3.05 [0.97]	4.83 [0.88]	1.78*** (0.07)
Income taxes	-1 (decrease), 0 (no change), 1 (increase)	-0.42 [0.66]	-0.27 [0.71]	0.15*** (0.05)
Inference about other people's income	-1 (lower), 0 (no change), 1 (higher)	0.55 [0.60]	-0.38 [0.66]	-0.93*** (0.05)
Hours of work	-1 (decrease), 0 (no change), 1 (increase)	2.24 [0.54]	1.90 [0.39]	-0.34*** (0.03)
Hours of work	Hours per week	2.06 [6.13]	-0.90 [3.82]	-2.96*** (0.38)
Hours of socializing	-1 (decrease), 0 (no change), 1 (increase)	1.89 [0.47]	2.13 [0.40]	0.24*** (0.03)
Hours of socializing	Hours per week	-0.53 [3.24]	0.55 [2.12]	1.08*** (0.20)
Buy a durable good (house, car, big item)	1 (more likely), 0 (no change), -1 (less likely)	-0.33 [0.65]	0.34 [0.58]	0.66*** (0.05)
Increase nondurable spending	1 (more likely), 0 (no change), -1 (less likely)	-0.45 [0.65]	0.02 [0.58]	0.47*** (0.05)
Save	1 (more likely), 0 (no change), -1 (less likely)	0.44 [0.69]	0.69 [0.51]	0.25*** (0.04)
Borrow	1 (more likely), 0 (no change), -1 (less likely)	-0.35 [0.66]	-0.65 [0.50]	-0.30*** (0.04)

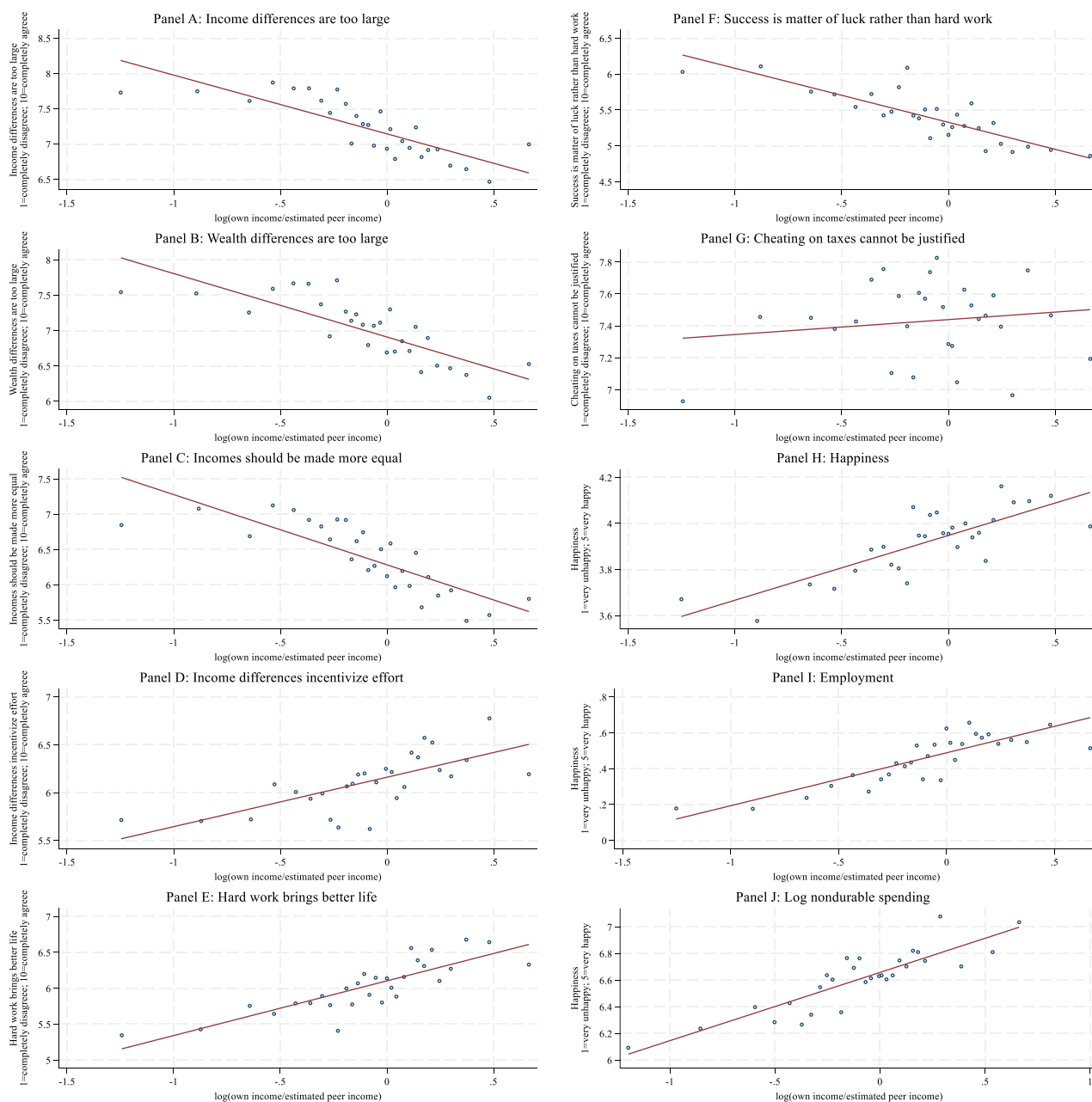
Notes: the table reports statistics for responses to hypothetical questions. The unit of measurement is indicated in column (1). Panel B shows responses conditional on a scenario in which a respondent's income is 20% lower or 20% higher than their peers' income. Assignment into lower/higher is random. For columns (2) and (3) standard deviations are reported in square brackets. For column (4), standard errors are reported in parentheses. ***, **, * indicates statistical significance of differences between columns (2) and (3) at 1, 5 and 10 percent levels.

Appendix Figure A1: Examples of screenshots shown at the treatment stage



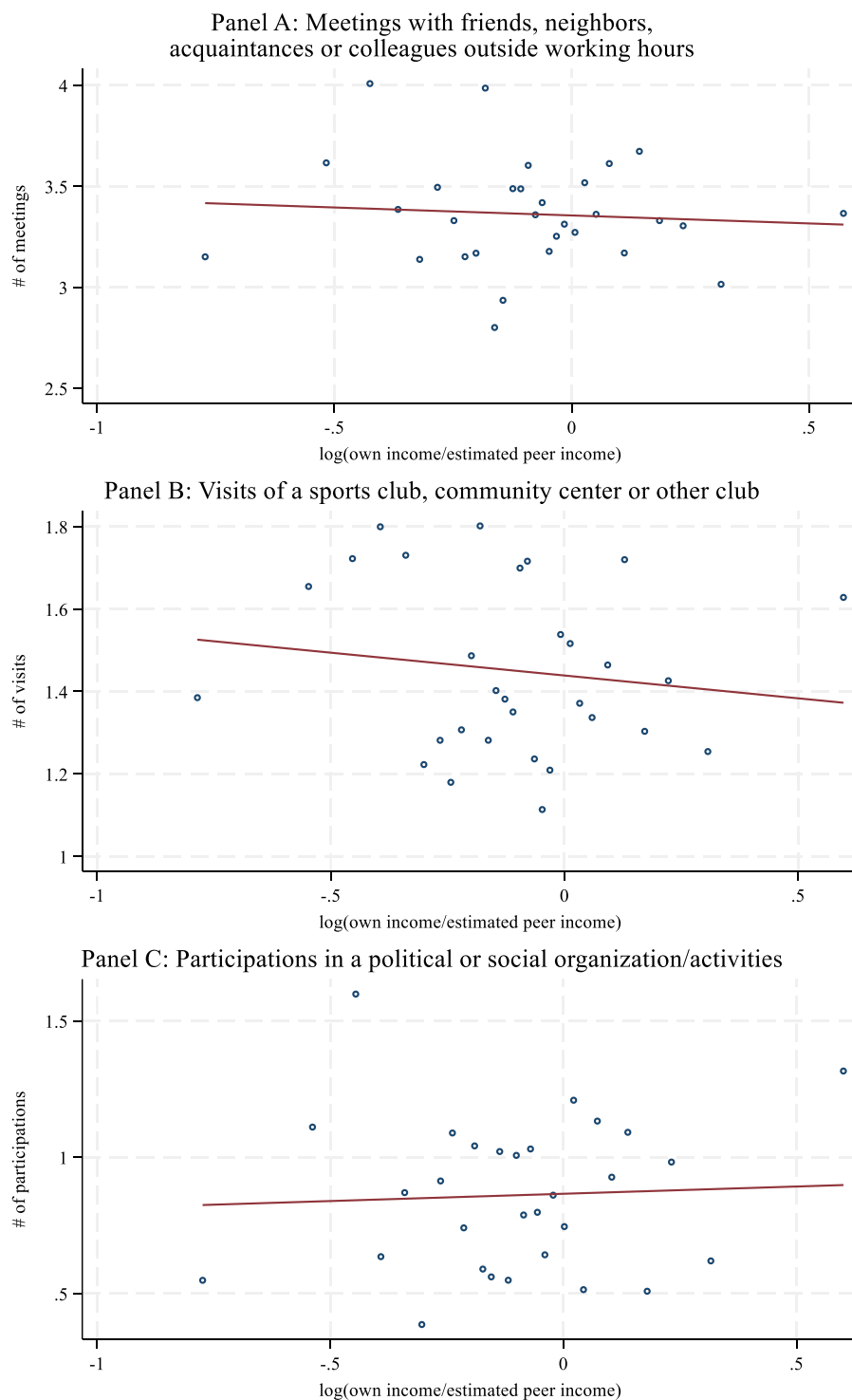
Notes: The chart shows each treated respondent's self-reported level of total household income versus the income of a typical household like them.

Appendix Figure A2: Correlations of Perceived Standings Relative to Peers with Beliefs, Well-being and Behavior after controlling for sociodemographic characteristics.



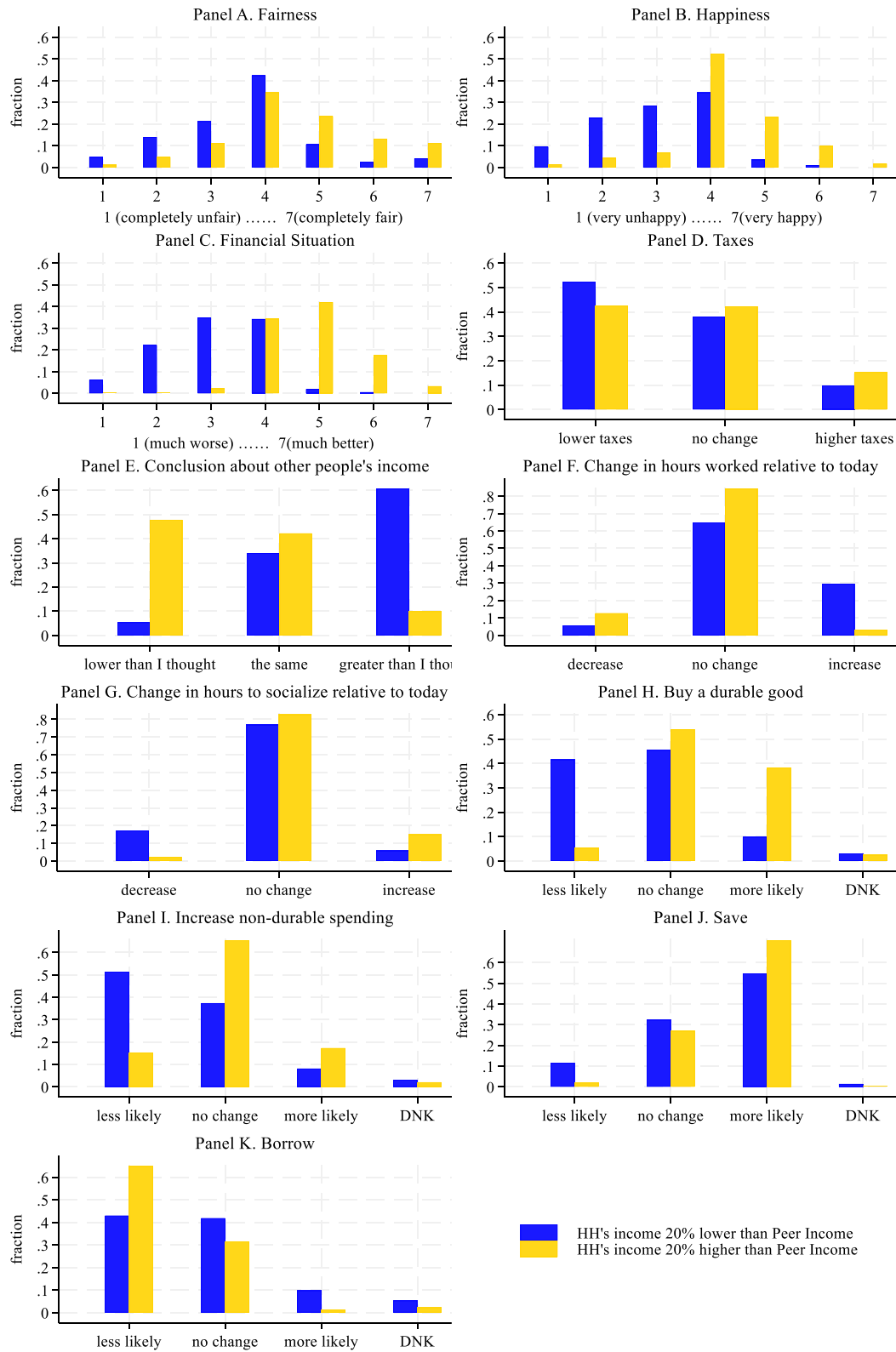
Notes: The binscatters report joint distributions after partialling out the variables used in column (1) of Table 2.

Appendix Figure A3: Correlation of Perceived Standings Relative to Peers and Socialization after controlling for sociodemographic characteristics.



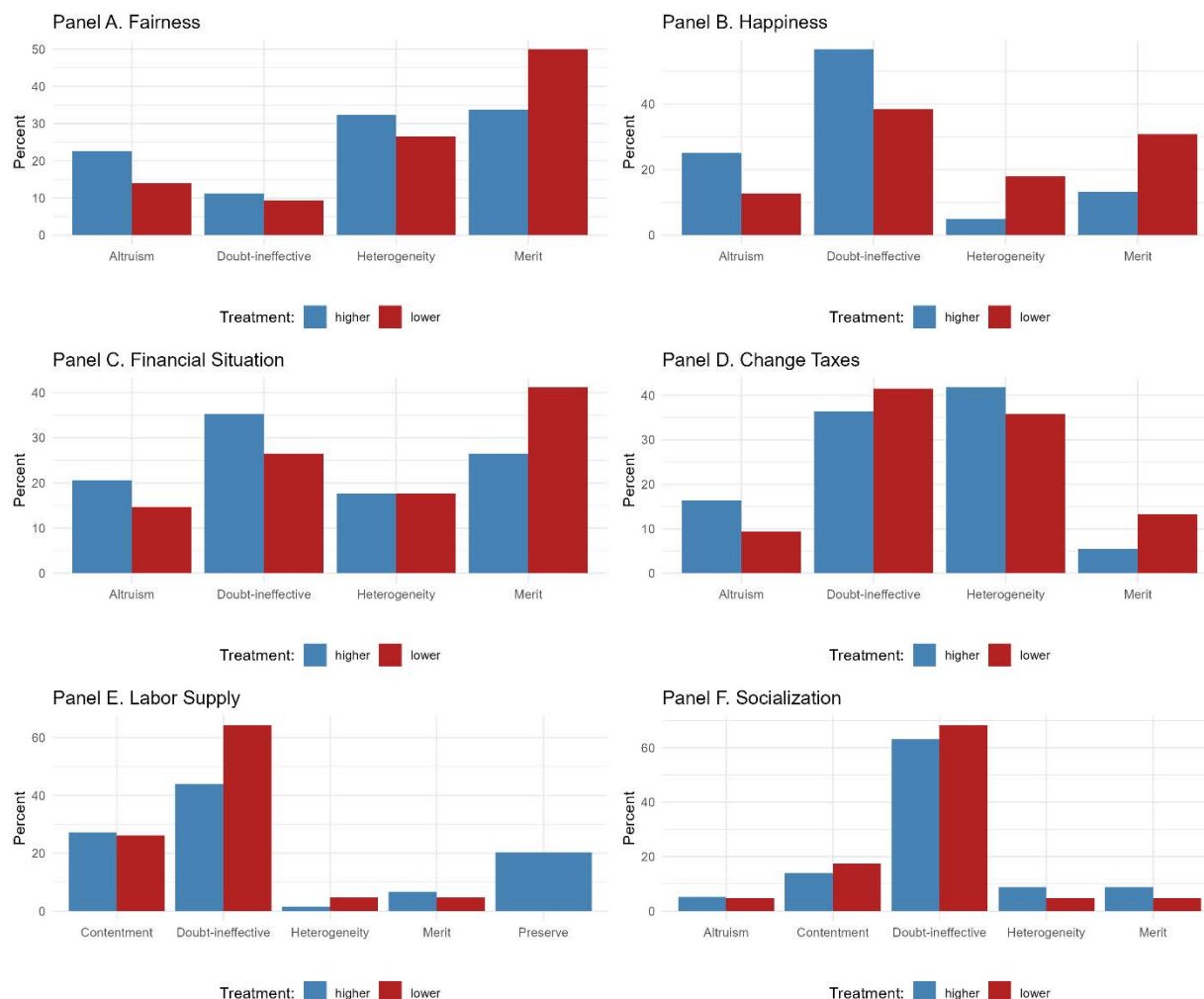
Notes: The binscatters report joint distributions after partialling out correlations with variables used in column (1) of Table 2.

Appendix Figure A4. Distribution of responses by scenario



Notes: the figure reports the distribution for responses to hypothetical questions.

Appendix Figure A5. Classification of open-ended questions.



Notes: Responses to the open-ended questions are classified using the following categories.

Doubt-ineffective: This category is for people who don't engage with the question, by saying comparisons are irrelevant, or that their assignment doesn't make sense (they doubt the accuracy). This suggests that these participants aren't likely to be affected highly by any info treatments.

Altruism: This category is for people who express priority for others, or economy as a whole as opposed to themselves. In this sense they might feel unfair being paid more money because they believe they are not entitled to it and would be more in favor of more equal distributions or helping the lower class. Because this motive runs counter to narrow self-interest, these respondents may respond in the opposite direction to what a self-interest model predicts.

Merit: This category is for people who express the income differences are earned by people. People with higher income often 'deserve' it because they have worked hard to get to that level. This category should hypothetically be the one that induces the most behavior changes, as they believe that choices they make are directly what determines income.

Heterogeneity: This category is for those who attribute the income differences, maybe not to individual hard work, but structural differences between jobs and households. They state things like different jobs, do different things and therefore provide different income according to risk, responsibilities, etc. They can also attribute this to difference in household composition, so maybe they make less, but they say this is normal, I am not a 2-person household, or maybe there are households with multiple incomes and that is just a structural fact. We would hypothesize that these people believe that income is not directly within their control, so we expect to have less behavioral changes compared to the merit-based respondents. **Note, this category is slightly different in Panel D. For taxes heterogeneity is defined by people thinking that taxes need to be heterogenous based on income and wealth. So most notably people suggest that the rich pay more taxes, and lower income and labor should be taxed less.

Contentment: This category is for people who express they are content/ satisfied with their current work or work life balance. These people are unlikely to change, as they don't see any need to. In this sense they are always nontakers regardless of information.

Preserve: This category is for people that always want to improve their standing, so even if they are already ahead, they want to work more to retain their standing or further improve. In this sense they are always takers regardless of information.

Fear of Debt: This category is for people who note fear of borrowing and getting stuck with high monthly payments and compounding interest that become out of control. People in this category avoid borrowing due to this fear and are unlikely to increase under any scenario.

Perceived risky position: This category is for people who see themselves at risk/more vulnerable compared to others, and suggest taking action to protect themselves financially.

Show-off: This category is for people who take the information/opportunity to spend/buy bigger and better things often to display to their peers.

Appendix B: Survey Questionnaires

Below we report the questionnaires for wave one (first and second part), two and three and the programming instructions.

Sample: All adult members in the household

Programming instructions: Randomly divide **households** across 3 groups: A, B and C. Assign both adult household members the same group number.

WAVE I A: Survey questions (Field work from 13 January – 28 January 2020)

This questionnaire is announced with the text: “This survey is about income of consumers”

ASK GROUPS A, B and C:

1) What was the total net annual income of your household in 2019?

☐ .. euro

☐ I do not know

(If Answer 1 = “I do not know”)

1b) Please provide an estimate of the total net annual income of your household in 2019, using the categories listed below?

☐ Net annual income was negative

☐ 0 euro

☐ 1 – 4,999 euro

☐ 5,000 – 9,999 euro

☐ 10,000 – 14,999 euro

☐ 15,000 – 19,999 euro

☐ 20,000 – 24,999 euro

☐ 25,000 – 29,999 euro

☐ 30,000 – 34,999 euro

☐ 35,000 – 39,999 euro

☐ 40,000 – 44,999 euro

☐ 45,000 – 49,999 euro

☐ 50,000 – 59,999 euro

☐ 60,000 – 69,999 euro

☐ 70,000 – 79,999 euro

☐ 80,000 – 89,999 euro

☐ 90,000 – 99,999 euro

☐ 100,000 – 149,999 euro

☐ 150,000 – 199,999 euro

☐ 200,000 – 499,999 euro

☐ 500,000 euro or more

☐ I do not know

2) How much do you think was the average net annual income of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work in 2019?

☐ .. euro

☐ I do not know

(IF Answer 2 = "I do not know")

2b) Please note that there is no right or wrong answer to this question. We are interested in your view of the average net annual income in 2019 of households that you associate frequently with. Please provide your estimate using the categories listed below?

- ☐ Average net annual income was negative
- ☐ 0 euro
- ☐ 1 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 14,999 euro
- ☐ 15,000 – 19,999 euro
- ☐ 20,000 – 24,999 euro
- ☐ 25,000 – 29,999 euro
- ☐ 30,000 – 34,999 euro
- ☐ 35,000 – 39,999 euro
- ☐ 40,000 – 44,999 euro
- ☐ 45,000 – 49,999 euro
- ☐ 50,000 – 59,999 euro
- ☐ 60,000 – 69,999 euro
- ☐ 70,000 – 79,999 euro
- ☐ 80,000 – 89,999 euro
- ☐ 90,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 499,999 euro
- ☐ 500,000 euro or more
- ☐ I do not know

3) How much higher or lower do you think **your household's total net income** will be over the next twelve months compared to the last twelve months?

Please allocate 100 points in the table below indicating how likely the listed changes are. Note that the points in the column should sum to 100.

	Points
Net income increases 8% or more	
Net income increases 4% or more, but less than 8%	
Net income increases 2% or more, but less than 4%	
Net income increases 1% or more, but less than 2%	
Net income increases or decreases less than 1%	
Net income decreases 1% or more, but less than 2%	
Net income decreases 2% or more, but less than 4%	
Net income decreases 4% or more, but less than 8%	
Net income decreases 8% or more	
Total (the points should sum to 100)	100

☐ I do not know

4) What is the total value of your household debt? Please take into account mortgage loans using your house or other real estate as collateral, car loans, extended lines of credit, personal loans, student loans, checking account overdrafts, loans from family, friends or acquaintances, outstanding credit card debts, outstanding debts from mail-order firms or from other hire purchases, etc.? Please provide the total amount in euro without dots or commas.

- ☐ .. euro
- ☐ We do not have debt
- ☐ I do not know

(IF Answer 4 = "I do not know")

4b) Please provide an estimate of your total household debt, using the categories listed below?

- ☐ 0 euro
- ☐ 1 – 999 euro
- ☐ 1,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 74,999 euro
- ☐ 75,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 249,999 euro
- ☐ 250,000 – 299,999 euro
- ☐ 300,000 – 349,999 euro
- ☐ 350,000 – 399,999 euro
- ☐ 400,000 – 499,999 euro
- ☐ 500,000 – 749,999 euro
- ☐ 750,000 euro or more
- ☐ I do not know

5) How much do you think is the average debt of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work? Please provide the total amount in euro without dots or commas.

- ☐ .. euro
- ☐ They do not have debt
- ☐ I do not know

(IF Answer 5 = "I do not know")

5b) Please note that there is no right or wrong answer to this question. We are interested in your view of the average debt of households that you associate frequently with. Please provide your estimate using the categories listed below?

- ☐ 0 euro
- ☐ 1 – 999 euro
- ☐ 1,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 74,999 euro
- ☐ 75,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 249,999 euro
- ☐ 250,000 – 299,999 euro
- ☐ 300,000 – 349,999 euro
- ☐ 350,000 – 399,999 euro
- ☐ 400,000 – 499,999 euro
- ☐ 500,000 – 749,999 euro
- ☐ 750,000 euro or more
- ☐ I do not know

6) How much higher or lower do you think **your total household debt** will be in twelve months compared to now?

Please allocate 100 points in the table below indicating how likely the listed changes are. Note that the points in the column should sum to 100.

	Points
Total household debt increases 20% or more	
Total household debt increases 10% or more, but less than 20%	
Total household debt increases 5% or more, but less than 10%	
Total household debt increases 1% or more, but less than 5%	
Total household debt increases or decreases less than 1%	
Total household debt decreases 1% or more, but less than 5%	
Total household debt decreases 5% or more, but less than 10%	
Total household debt decreases 10% or more, but less than 20%	
Total household debt decreases 20% or more	
Total (the points should sum to 100)	100

☐ I do not know

ASK GROUPS A and B:

7) Generally speaking, do you think that now is a good time or a bad time **to buy**...

	Very bad	Bad	Neither good nor bad	Good	Very good
- A house or apartment	☐	☐	☐	☐	☐
- A car or other vehicle	☐	☐	☐	☐	☐
- Large appliances, furniture, electronics (incl. gadgets)	☐	☐	☐	☐	☐
- Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐	☐	☐	☐
- Holiday packages and/ or trips abroad	☐	☐	☐	☐	☐
- Vacation homes, boats or other big-ticket recreational equipment	☐	☐	☐	☐	☐

ASK GROUPS A and C:

8) Generally speaking, do you think now is a good time or a bad time **to borrow money in order to buy**...

	Very bad	Bad	Neither good nor bad	Good	Very good
- A house or apartment	☐	☐	☐	☐	☐
- A car or other vehicle	☐	☐	☐	☐	☐
- Large appliances, furniture, electronics (incl. gadgets)	☐	☐	☐	☐	☐
- Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐	☐	☐	☐
- Holiday packages and/ or trips abroad	☐	☐	☐	☐	☐
- Vacation homes, boats or other big-ticket recreational equipment	☐	☐	☐	☐	☐

ASK GROUPS A, B and C:

9) We are interested in activities that you may do. How often in the last month have you ..

	0 times	1-2 times	3-4 times	5-9 times	10 times or more
- Met with friends, neighbors, acquaintances, or people at work (after working hours)	☐	☐	☐	☐	☐
- Gone to a sport, social or other kind of club	☐	☐	☐	☐	☐
- Taken part in a political or community-related organization/activities	☐	☐	☐	☐	☐

10) Do you think **your household will be financially** better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

11) To what extent do you agree with the statement “**Differences in incomes in the Netherlands are too large**”? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree)	2	3	4	5	6	7	8	9	10 (completely agree)
☐	☐	☐	☐	☐	☐	☐	☐	☐	☐

12) All in all, to what extent do you consider yourself a happy person?

- ☐ Very happy
- ☐ Happy
- ☐ Happy nor unhappy
- ☐ Unhappy
- ☐ Very unhappy
- ☐ I do not know

13) To what extent do you agree with the statements below? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree)	2	3	4	5	6	7	8	9	10 (completely agree)
☐	☐	☐	☐	☐	☐	☐	☐	☐	☐

- a) Incomes should be made more equal
- b) Differences in income are an incentive for individual effort
- c) In the long run, hard work usually brings a better life
- d) Hard work does not generally bring success – it is more a matter of luck and connections
- e) Cheating on taxes if you have a chance can never be justified

14) To what extent do you agree with the statement “**Differences in wealth in the Netherlands are too large**”? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree)	2	3	4	5	6	7	8	9	10 (completely agree)
☐	☐	☐	☐	☐	☐	☐	☐	☐	☐

15) Do you think that **economic conditions in the Netherlands** will be better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

WAVE I B: Survey questions (Field work from 31 January – 18 February 2020)

This questionnaire is announced with the text: “This survey is about income of consumers and it is a follow-up of a survey two weeks ago”

ASK GROUPS A, B and C (group numbers are taken the same as in wave 1 A):

C1) How much did your household spend on purchases of **durable goods** in the **last month** (January 2020)?

Durable goods are goods that last in time, including for instance cars, electronics, kitchen appliances, furniture, house maintenance, jewelries, etc. (please exclude purchases of houses, apartments, etc.). Please provide an answer in euros.

Answer:..... euros

... I did not buy any durables

... I do not know

(IF Answer C1 = “I do not know”)

C1b) Please provide an estimate of how much your household did spend on purchases of durable goods in the last month (January 2020), using the categories listed below?

☐ 0 euro

☐ 1 – 49 euro

☐ 50-99 euro

☐ 100 – 199 euro

☐ 200 – 299 euro

☐ 300 – 499 euro

☐ 500 – 999 euro

☐ 1,000 -1,999 euro

☐ 2,000 – 2,999 euro

☐ 3,000 – 4,999 euro

☐ 5,000 – 9,999 euro

☐ 10,000 – 19,999 euro

☐ 20,000 – 29,999 euro

☐ 30,000 – 49,999 euro

☐ 50,000 – 99,999 euro

☐ 100,000 – 199,999 euro

☐ 200,000 euro or more

☐ I do not know

C2) What do you think your household's spending on purchases of **durable goods** will be per month in the next three months (February, March and April 2020)? Please provide an answer in euros.

February: euros
... I do not have plans to buy durables in this month
... I do not know

March: euros
... I do not have plans to buy durables in this month
... I do not know

April: euros
... I do not have plans to buy durables in this month
... I do not know

(If Answer C2 = "I do not know" for [February/March/April] 2020)

C2b) Please provide an estimate of how much you think your household will spend on purchases of durable goods in the month [February/March/April] 2020, using the categories listed below?

- ☐ 0 euro
- ☐ 1 – 49 euro
- ☐ 50-99 euro
- ☐ 100 – 199 euro
- ☐ 200 – 299 euro
- ☐ 300 – 499 euro
- ☐ 500 – 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

C3) What was your households' spending on **nondurable goods and services** in the **last month** (January 2020)?

Nondurable goods and services include for instance food, tobacco, alcohol, clothing, haircuts, gasoline, utilities, transportation, and other small services and nondurable goods that do not last in time. Please provide an answer in euros.

Answer: euros
... I do not know

(IF Answer C3 = "I do not know")

C3b) Please provide an estimate of how much your household did spend on nondurable goods and services in the last month (January 2020), using the categories listed below?

- ☐ 0 euro
- ☐ 1 – 49 euro
- ☐ 50-99 euro
- ☐ 100 – 199 euro
- ☐ 200 – 299 euro
- ☐ 300 – 499 euro
- ☐ 500 – 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

C4) Which of the expense items below did your household spend money on in the last month (January 2020)? Multiple answers possible.

- ☐ A house or apartment
- ☐ A car or other vehicle
- ☐ Large appliances, furniture, electronics (incl. gadgets)
- ☐ Luxury items such as jewelries, expensive watches, pieces of artwork
- ☐ Holiday packages and/or trips abroad
- ☐ Vacation homes, boats or other big-ticket recreational equipment
- ☐ None of the above

C5) In the last month (January 2020), did your household take out any new loan and/or increase the outstanding debt on an existing loan?

Please answer the question for mortgage loans using your house or other real estate as collateral and for other loans (car loans, extended lines of credit, personal loans, student loans, checking account overdrafts, loans from family, friends or acquaintances, outstanding credit card debts, outstanding debts from mail-order firms or from other hire purchases, etc.)

	Yes	No
- Mortgage debt	☐	☐
- Other debt	☐	☐

Programming instructions: Use for XX the responses to questions 1 and 1b from wave 1 A. If an amount was provided in response to question 1, then XX is equal to this amount. Else: If a category was reported in question 1b, then XX is equal to the midpoint of the selected interval. Take XX equal to 650,000 (euro) if “500,000 euro or more” was reported in question 1b. Skip question 16 and go directly to question 17 if “Net annual income was negative” or “I do not know” was reported in question 1b.

16) You have reported previously that the total net annual income of your household in 2019 was about XX euro. Is this correct?

☐ Yes, this is correct

☐ No, this is not correct

(IF Answer 16 = “No” THEN questions 17 and 17b, ELSE question 18)

(Programming instructions: questions 17 and 17b are the same as questions 1 and 1b from wave 1 A)

17) What was the total net annual income of your household in 2019?

☐ .. euro

☐ I do not know

(IF Answer 17 = “I do not know”)

17b) Please provide an estimate of the total net annual income of your household in 2019, using the categories listed below?

☐ Net annual income was negative

☐ 0 euro

☐ 1 – 4,999 euro

☐ 5,000 – 9,999 euro

☐ 10,000 – 14,999 euro

☐ 15,000 – 19,999 euro

☐ 20,000 – 24,999 euro

☐ 25,000 – 29,999 euro

☐ 30,000 – 34,999 euro

☐ 35,000 – 39,999 euro

☐ 40,000 – 44,999 euro

☐ 45,000 – 49,999 euro

☐ 50,000 – 59,999 euro

☐ 60,000 – 69,999 euro

☐ 70,000 – 79,999 euro

☐ 80,000 – 89,999 euro

☐ 90,000 – 99,999 euro

☐ 100,000 – 149,999 euro

☐ 150,000 – 199,999 euro

☐ 200,000 – 499,999 euro

☐ 500,000 euro or more

☐ I do not know

Programming instructions: Use for YY the responses to questions 4 and 4b from wave 1 A. If an amount was provided in response to question 4, then YY is equal to this amount. Take YY equal to "0 (zero)" if "We do not have debt" was reported in question 4. Else: If a category was reported in question 4b, then YY is equal to the midpoint of the selected interval. Take YY equal to 875,000 (euro) if "750,000 euro or more" was reported in question 4b. Skip question 18 and go directly to question 19 if "I do not know" was reported in question 4b.

18) You have reported previously that the total value of your household debt is about YY euro. Is this correct?

☐ Yes, this is correct

☐ No, this is not correct

(IF Answer 18 = "No" THEN questions 19 and 19b ELSE question 20)

(Programming instructions: questions 19 and 19b are the same as questions 4 and 4b from wave 1 A)

19) What is the total value of your household debt? Please take into account mortgage loans using your house or other real estate as collateral, car loans, extended lines of credit, personal loans, student loans, checking account overdrafts, loans from family, friends or acquaintances, outstanding credit card debts, outstanding debts from mail-order firms or from other hire purchases, etc.? Please provide the total amount in euro without dots or commas.

☐ .. euro

☐ We do not have debt

☐ I do not know

(IF Answer 19 = "I do not know")

19b) Please provide an estimate of your total household debt, using the categories listed below?

☐ 0 euro

☐ 1 – 999 euro

☐ 1,000 – 4,999 euro

☐ 5,000 – 9,999 euro

☐ 10,000 – 19,999 euro

☐ 20,000 – 29,999 euro

☐ 30,000 – 49,999 euro

☐ 50,000 – 74,999 euro

☐ 75,000 – 99,999 euro

☐ 100,000 – 149,999 euro

☐ 150,000 – 199,999 euro

☐ 200,000 – 249,999 euro

☐ 250,000 – 299,999 euro

☐ 300,000 – 349,999 euro

☐ 350,000 – 399,999 euro

☐ 400,000 – 499,999 euro

☐ 500,000 – 749,999 euro

☐ 750,000 euro or more

☐ I do not know

Programming instructions for INFORMATION TREATMENT: Respondents in group A (the control group) receive no information. Respondents in group B and group C receive the following information as listed below where the values of X and Y provided to the respondents depend on the age of the respondent (age categories: <36, 36-50, 51-65, 66), the education of the respondent (education categories: less than high school, high school, college, university), whether the respondent has a partner or not and in addition for Y whether the respondent is a homeowner or a renter. Tables 1 and 2 in the Annex show the values for X and Y.

INFORMATION 1 (for respondents in Group B):

Before we proceed, we would like to share the following information with you. According to recent representative statistical data, the **total net annual household income of a typical household of similar age and education like yours is: X euro.**

Programming note: The screen shows the text plus a bar chart with two bars: own vs comparison group income (no bar chart is shown when own net household income is unknown or a negative number is provided in the open question; when category negative income is chosen a bar chart is shown where own net household income is put to zero)

INFORMATION 2 (for respondents in Group C):

Before we proceed, we would like to share the following information with you. According to recent representative statistical data, the **total value of household debt of a typical household of similar age and education like yours is: Y euro.**

Programming note: The screen shows the text plus a bar chart with two bars: own vs comparison group debt (no bar chart is shown when own household debt is unknown)

ASK GROUPS A, B and C:

(Programming instructions: questions 20 and 20b are the same as questions 2 and 2b from wave 1 A)

20) How much do you think was the average net annual income of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work in 2019?

- ☐ .. euro
- ☐ I do not know

(IF Answer 20 = "I do not know")

20b) Please note that there is no right or wrong answer to this question. We are interested in your view of the average net annual income in 2019 of households that you associate frequently with. Please provide your estimate using the categories listed below?

- ☐ Average net annual income was negative
- ☐ 0 euro
- ☐ 1 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 14,999 euro
- ☐ 15,000 – 19,999 euro
- ☐ 20,000 – 24,999 euro
- ☐ 25,000 – 29,999 euro
- ☐ 30,000 – 34,999 euro
- ☐ 35,000 – 39,999 euro

- ☐ 40,000 – 44,999 euro
- ☐ 45,000 – 49,999 euro
- ☐ 50,000 – 59,999 euro
- ☐ 60,000 – 69,999 euro
- ☐ 70,000 – 79,999 euro
- ☐ 80,000 – 89,999 euro
- ☐ 90,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 499,999 euro
- ☐ 500,000 euro or more
- ☐ I do not know

(Programming instructions: questions 21 and 21b are the same as questions 5 and 5b from wave 1 A)

21) How much do you think is the average debt of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work? Please provide the total amount in euro without dots or commas.

- ☐ .. euro
- ☐ They do not have debt
- ☐ I do not know

(IF Answer 21 = “I do not know”)

21b) Please note that there is no right or wrong answer to this question. We are interested in your view of the average debt of households that you associate frequently with. Please provide your estimate using the categories listed below?

- ☐ 0 euro
- ☐ 1 – 999 euro
- ☐ 1,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 74,999 euro
- ☐ 75,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 249,999 euro
- ☐ 250,000 – 299,999 euro
- ☐ 300,000 – 349,999 euro
- ☐ 350,000 – 399,999 euro
- ☐ 400,000 – 499,999 euro
- ☐ 500,000 – 749,999 euro
- ☐ 750,000 euro or more
- ☐ I do not know

22) To what extent do you agree with the statement “**If I compare myself with my friends, I think in general I am financially better off**”? Please provide an answer on a scale from 1 to 7, where 1 means “completely disagree” and 7 means “completely agree”.

1 (completely disagree) 2 3 4 5 6 7 (completely agree)
☐ ☐ ☐ ☐ ☐ ☐ ☐

ASK GROUPS A and B:

(Programming instructions: question 23 is the same as question 7 from wave 1 A)

23) Generally speaking, do you think that now is a good time or a bad time **to buy**...

	Very bad	Bad	Neither good nor bad	Good	Very good
- A house or apartment	☐	☐	☐	☐	☐
- A car or other vehicle	☐	☐	☐	☐	☐
- Large appliances, furniture, electronics (incl. gadgets)	☐	☐	☐	☐	☐
- Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐	☐	☐	☐
- Holiday packages and/ or trips abroad	☐	☐	☐	☐	☐
- Vacation homes, boats or other big-ticket recreational equipment	☐	☐	☐	☐	☐

ASK GROUPS A and C:

(Programming instructions: question 24 is the same as question 8 from wave 1 A)

24) Generally speaking, do you think now is a good time or a bad time **to borrow money in order to buy**...

	Very bad	Bad	Neither good nor bad	Good	Very good
- A house or apartment	☐	☐	☐	☐	☐
- A car or other vehicle	☐	☐	☐	☐	☐
- Large appliances, furniture, electronics (incl. gadgets)	☐	☐	☐	☐	☐
- Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐	☐	☐	☐
- Holiday packages and/ or trips abroad	☐	☐	☐	☐	☐
- Vacation homes, boats or other big-ticket recreational equipment	☐	☐	☐	☐	☐

ASK GROUPS A, B and C:

25) Suppose that your bank offers you the possibility to borrow as much money as you like to borrow at a fixed interest rate of 1% per year. Would you consider borrowing money from the bank?

☐ Yes
☐ No

(IF Answer 25 = "Yes")

26) What would be the maximum fraction of your household income that you would be willing to pay per month in order to service this loan?

- ☐ 1-10 per cent of household income
- ☐ 11-20 per cent of household income
- ☐ 21-30 per cent of household income
- ☐ 31-40 per cent of household income
- ☐ 41-50 per cent of household income
- ☐ more than 50 per cent of household income

(Programming instructions: question 27 is the same as question 10 from wave 1 A)

27) Do you think **your household will be financially** better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

(Programming instructions: question 28 is the same as question 11 from wave 1 A)

28) To what extent do you agree with the statement "**Differences in incomes in the Netherlands are too large**"? Please provide an answer on a scale from 1 to 10, where 1 means "completely disagree" and 10 means "completely agree".

- | | | | | | | | | | |
|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| 1 (completely disagree) | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 (completely agree) |
| ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ |

(Programming instructions: question 29 is the same as question 15 from wave 1 A)

29) Do you think that **economic conditions in the Netherlands** will be better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

30) We now would like to ask you a question about the minimum pay you would accept in another job.

Say someone offers you a job which covers the same sort of work as you do now, and which is in the same city/town where you work now. Imagine all financial circumstances, apart from the pay, to be equal to those in your current position. Would you consider accepting this job?

- ☐ Yes
- ☐ No
- ☐ Not applicable, I do not have a job

(IF Answer 30 = “Yes”)

31) How much would then the minimum net wages have to be such that you would accept that new job? Would you please round off the amount on whole euros.

[] .. euro per month

[] I do not know

Annex wave 1B: Values used in information treatment for net household income and total household debt of a typical household of similar age and education like yours.

Table 1. Net annual household income based on age and education DHS panel members				
	Education level			
	Less than high school	High school	College	University
<i>Panel A. Net household income per year for singles (in euro)</i>				
Age < 36	14000	18000	26000	18000
Age: 36-50	15000	23000	29000	39000
Age: 51-65	19000	22000	27000	29000
Age > 65	21000	22000	27000	36000
<i>Panel B. Net household income per year for couples (in euro)</i>				
Age < 36	35000	39000	50000	56000
Age: 36-50	36000	45000	50000	65000
Age: 51-65	35000	42000	45000	53000
Age > 65	32000	35000	43000	51000
Notes: Numbers represent median net household income of panel members in different cells based on the available background information.				

Table 2. Total household debt based on age and education DHS panel members				
	Education level			
	Less than high school	High school	College	University
<i>Panel A.1 Total household debt for singles, renters (in euro)</i>				
Age < 36	6000	8000	10000	11000
Age: 36-50	1000	2000	5000	5000
Age: 51-65	3000	5000	7000	8000
Age > 65	0	1000	3000	4000
<i>Panel A.2 Total household debt for singles, homeowners (in euro)</i>				
Age < 36	133000	137000	154000	199000
Age: 36-50	136000	140000	157000	202000
Age: 51-65	71000	76000	93000	138000
Age > 65	48000	53000	70000	115000
<i>Panel B.1 Total household debt for couples, renters (in euro)</i>				
Age < 36	15000	12000	10000	24000

Age: 36-50	10000	6000	4000	19000
Age: 51-65	4000	0	0	13000
Age > 65	8000	5000	3000	17000
<i>Panel B.2 Total household debt for couples, homeowners (in euro)</i>				
Age < 36	145000	156000	185000	233000
Age: 36-50	162000	174000	202000	251000
Age: 51-65	89000	100000	129000	178000
Age > 65	55000	66000	95000	143000
Notes: Since some age-educations cells have limited number of observations for household debt, we report predicted values based on regressions using the debt information provided by panel members in the annual surveys of 2017 and 2018. The value zero is assigned when predicted values are negative (2 occasions for renters).				

WAVE II: Survey questions (Field work from 28 February – 17 March 2020)

This questionnaire is announced with the text: “This survey is about income of consumers and it is a follow-up of a survey one month ago”

ASK GROUPS A, B and C (group numbers are taken the same as in waves 1 A and 1 B):

(Programming instructions: questions 20 and 20b are taken from wave 1 B and are the same as questions 2 and 2b from wave 1 A as well)

20) How much do you think was the average net annual income of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work in 2019?

☐ .. euro

☐ I do not know

(IF Answer 20 = “I do not know”)

20b) Please note that there is no right or wrong answer to this question. We are interested in your view of the average net annual income in 2019 of households that you associate frequently with. Please provide your estimate using the categories listed below?

☐ Average net annual income was negative

☐ 0 euro

☐ 1 - 4,999 euro

☐ 5,000 – 9,999 euro

☐ 10,000 – 14,999 euro

☐ 15,000 – 19,999 euro

☐ 20,000 – 24,999 euro

☐ 25,000 – 29,999 euro

☐ 30,000 – 34,999 euro

☐ 35,000 – 39,999 euro

☐ 40,000 – 44,999 euro

☐ 45,000 – 49,999 euro

☐ 50,000 – 59,999 euro

☐ 60,000 – 69,999 euro

☐ 70,000 – 79,999 euro

☐ 80,000 – 89,999 euro

☐ 90,000 – 99,999 euro

- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 499,999 euro
- ☐ 500,000 euro or more
- ☐ I do not know

(Programming instructions: questions 21 and 21b are taken from wave 1 B and are the same as questions 5 and 5b from wave 1 A as well)

21) How much do you think is the average debt of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work? Please provide the total amount in euro without dots or commas.

- ☐ .. euro
- ☐ They do not have debt
- ☐ I do not know

(IF Answer 21 = “I do not know”)

21b) Please note that there is no right or wrong answer to this question. We are interested in your view of the average debt of households that you associate frequently with. Please provide your estimate using the categories listed below?

- ☐ 0 euro
- ☐ 1 - 999 euro
- ☐ 1,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 74,999 euro
- ☐ 75,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 249,999 euro
- ☐ 250,000 – 299,999 euro
- ☐ 300,000 – 349,999 euro
- ☐ 350,000 – 399,999 euro
- ☐ 400,000 – 499,999 euro
- ☐ 500,000 – 749,999 euro
- ☐ 750,000 euro or more
- ☐ I do not know

(Programming instructions: questions C1-C5 are taken from wave 1 B and adjusted where a reference period is mentioned)

C1) How much did your household spend on purchases of **durable goods** in the **last month** (February 2020)?

Durable goods are goods that last in time, including for instance cars, electronics, kitchen appliances, furniture, house maintenance, jewelries, etc. (please exclude purchases of houses, apartments, etc.). Please provide an answer in euros.

Answer:..... euros
 ... I did not buy any durables
 ... I do not know

(IF Answer C1 = "I do not know")

C1b) Please provide an estimate of how much your household did spend on purchases of durable goods in the last month (February 2020), using the categories listed below?

- ☐ 0 euro
- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

C2) What do you think your household's spending on purchases of **durable goods** will be per month in the next two months (March and April 2020)? Please provide an answer in euros.

March: euros
... I do not have plans to buy durables in this month
... I do not know

April: euros
... I do not have plans to buy durables in this month
... I do not know

(IF Answer C2 = "I do not know" for [March/April] 2020)

C2b) Please provide an estimate of how much you think your household will spend on purchases of durable goods in the month **[March/April]** 2020, using the categories listed below?

- ☐ 0 euro
- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro

- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

C3) What was your households' spending on **nondurable goods and services** in the **last month** (February 2020)?

Nondurable goods and services include for instance food, tobacco, alcohol, clothing, haircuts, gasoline, utilities, transportation, and other small services and nondurable goods that do not last in time. Please provide an answer in euros.

Answer: euros
 ... I do not know

(IF Answer C3 = "I do not know")

C3b) Please provide an estimate of how much your household did spend on nondurable goods and services in the last month (February 2020), using the categories listed below?

- ☐ 0 euro
- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

C4) Which of the expense items below did your household spend money on in the last month (February 2020)? Multiple answers possible.

- ☐ A house or apartment
- ☐ A car or other vehicle
- ☐ Large appliances, furniture, electronics (incl. gadgets)
- ☐ Luxury items such as jewelries, expensive watches, pieces of artwork
- ☐ Holiday packages and/or trips abroad
- ☐ Vacation homes, boats or other big-ticket recreational equipment

[] None of the above

C5) In the last month (February 2020), did your household take out any new loan and/or increase the outstanding debt on an existing loan?

Please answer the question for mortgage loans using your house or other real estate as collateral and for other loans (car loans, extended lines of credit, personal loans, student loans, checking account overdrafts, loans from family, friends or acquaintances, outstanding credit card debts, outstanding debts from mail-order firms or from other hire purchases, etc.)

	Yes	No
- Mortgage debt	☐	☐
- Other debt	☐	☐

ASK GROUPS A and B:

(Programming instructions: question 23 is taken from wave 1 B and is the same as question 7 from wave 1 A as well)

23) Generally speaking, do you think that now is a good time or a bad time **to buy**...

	Very bad	Bad	Neither good nor bad	Good	Very good
- A house or apartment	☐	☐	☐	☐	☐
- A car or other vehicle	☐	☐	☐	☐	☐
- Large appliances, furniture, electronics (incl. gadgets)	☐	☐	☐	☐	☐
- Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐	☐	☐	☐
- Holiday packages and/ or trips abroad	☐	☐	☐	☐	☐
- Vacation homes, boats or other big-ticket recreational equipment	☐	☐	☐	☐	☐

ASK GROUPS A and C:

(Programming instructions: question 24 is taken from wave 1 B and is the same as question 8 from wave 1 A as well)

24) Generally speaking, do you think now is a good time or a bad time **to borrow money in order to buy**...

	Very bad	Bad	Neither good nor bad	Good	Very good
- A house or apartment	☐	☐	☐	☐	☐
- A car or other vehicle	☐	☐	☐	☐	☐
- Large appliances, furniture, electronics (incl. gadgets)	☐	☐	☐	☐	☐
- Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐	☐	☐	☐
- Holiday packages and/ or trips abroad	☐	☐	☐	☐	☐
- Vacation homes, boats or other big-ticket recreational equipment	☐	☐	☐	☐	☐

ASK GROUPS A, B and C:

(Programming instructions: question 9 is taken from wave 1 A)

9) We are interested in activities that you may do. How often in the last month have you ..

	0 times	1-2 times	3-4 times	5-9 times	10 times or more
- Met with friends, neighbors, acquaintances, or people at work (after working hours)	☐	☐	☐	☐	☐
- Gone to a sport, social or other kind of club	☐	☐	☐	☐	☐
- Taken part in a political or community-related organization/activities	☐	☐	☐	☐	☐

(Programming instructions: question 27 is taken from wave 1 B and is the same as question 10 from wave 1 A as well)

27) Do you think **your household will be financially** better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

(Programming instructions: question 28 is taken from wave 1 B and is the same as question 11 from wave 1 A as well)

28) To what extent do you agree with the statement “**Differences in incomes in the Netherlands are too large**”? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree) 2 3 4 5 6 7 8 9 10 (completely agree)

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

(Programming instructions: question 12 is taken from wave 1 A)

12) All in all, to what extent do you consider yourself a happy person?

- ☐ Very happy
- ☐ Happy
- ☐ Happy nor unhappy
- ☐ Unhappy
- ☐ Very unhappy
- ☐ I do not know

(Programming instructions: question 13 is taken from wave 1 A)

13) To what extent do you agree with the statements below? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree) 2 3 4 5 6 7 8 9 10 (completely agree)

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

- a) Incomes should be made more equal
- b) Differences in income are an incentive for individual effort
- c) In the long run, hard work usually brings a better life
- d) Hard work does not generally bring success – it is more a matter of luck and connections
- e) Cheating on taxes if you have a chance can never be justified

(Programming instructions: question 14 is taken from wave 1 A)

14) To what extent do you agree with the statement “**Differences in wealth in the Netherlands are too large**”? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree) 2 3 4 5 6 7 8 9 10 (completely agree)
☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

(Programming instructions: question 29 is taken from wave 1 B and is the same as question 15 from wave 1 A as well)

29) Do you think that **economic conditions in the Netherlands** will be better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

WAVE III: Survey questions (Field work from 4 May – 19 May 2020)

Introductory text: “This survey is about income of consumers, social relations and the impact of the coronavirus on our daily lives.”

A. Social networks

(Programming instructions: question AQ1 is the same as question 9 in wave 1A and question 9 in wave 2, with two additional answer categories)

AQ1) We are interested in activities that you may do. How often in the last month have you ..

	0 times	1-2 times	3-4 times	5-9 times	10 times or more
- Met with friends, neighbors, acquaintances, or people at work (after working hours)	☐	☐	☐	☐	☐
- Gone to a sport, social or other kind of club	☐	☐	☐	☐	☐
- Taken part in a political or community-related organization/activities	☐	☐	☐	☐	☐
- Talked with friends, neighbors, acquaintances, or people at work (after working hours) via phone	☐	☐	☐	☐	☐
- Been in touch with friends, neighbors, acquaintances, or people at work (after working hours) via social media	☐	☐	☐	☐	☐

QA2) Which level of education do **most** of the people you associate frequently with, such as friends, neighbors, acquaintances, or people at work have?

- 1 primary education
- 2 junior vocational training
- 3 lower secondary education
- 4 secondary education/pre-university education
- 5 senior vocational training
- 6 vocational colleges/first year university education
- 7 university education

QA3) What kind of employment do **most** of the people you associate frequently with, such as friends, neighbors, acquaintances, or people at work have?

- 1 self-employed
- 2 practicing a free profession (freelance)
- 3 working in the family business
- 4 employed on a contractual basis
- 5 no paid job (or a small temporary job)
- 6 (early) retired

(Programming instructions: question AQ4 is the same as question 12 in wave 1A and question 12 in wave 2)

AQ4) All in all, to what extent do you consider yourself a happy person?

- ☐ Very happy
- ☐ Happy
- ☐ Happy nor unhappy
- ☐ Unhappy
- ☐ Very unhappy
- ☐ I do not know

B. Savings and access to credit

BQ1) Households save in various ways (depositing money in a bank account, buying financial assets, property or other assets) and for different reasons. One reason is to protect against contingencies, such as uncertainty about future earnings or unexpected outlays (e.g., owing to health problems or other emergencies).

Roughly how much do you think your household needs to put aside in total savings to deal with such unexpected events?

- ☐ .. euro

BQ2) Are you currently in a position to borrow a substantial sum of money from people you associate frequently with, such as friends, neighbors, acquaintances, or people at work?

- 1 yes
- 2 no

-9 don't know

BQ3) If you would need credit from a bank (or other financial firm) now, would you expect your application to be accepted?

- 1 yes
- 2 no

C. Employment block

CQ1) What best describes your current employment situation?

- ☐ Employed
- ☐ Self-employed (incl. running own business, freelancer)
- ☐ (Early) retired
- ☐ Unemployed and actively looking for a job
- ☐ Not working, interested in having a job but not actively looking for a job
- ☐ Laid off due to outbreak coronavirus, but expect to be rehired by previous employer
- ☐ Other

(IF Answer CQ1 = " Not working, interested in having a job but not actively looking for a job ")

CQ2) Here are a number of possible reasons why people who are not working choose not to look for work. Please select all that apply to you.

- ☐ Homemaker
- ☐ Raising children
- ☐ Student
- ☐ Disabled / health issues
- ☐ Couldn't find a job
- ☐ On break
- ☐ No financial need
- ☐ Waiting out coronavirus
- ☐ Laid off due to outbreak coronavirus, but expect to be rehired by previous employer
- ☐ Other

(IF Answer CQ1 = "employed" or "self-employed")

CQ3) Please check the statements that characterize your job? Multiple answers possible.

- ☐ I have to come to office, factory, etc. to perform my work duties
- ☐ I can work remotely from home
- ☐ I travel to meet my clients
- ☐ I travel to deliver products to customers
- ☐ My job has fixed hours.
- ☐ My hours vary depending on business intensity
- ☐ I work less than 20 hours per week on average.
- ☐ I work more than 40 hours per week on average.
- ☐ I can work as few or as many hours as I want.
- ☐ The number of hours that I work is determined by my supervisor.
- ☐ I am currently not going to my workplace because of the coronavirus
- ☐ None of the above

(IF Answer CQ1 = "employed" OR Answer CQ1 = "Unemployed and actively looking for a job" OR Answer CQ1 = "Not working, interested in having a job but not actively looking for a job" OR Answer CQ1 = "Laid off due to outbreak coronavirus, but expect to be rehired by previous employer")
(Programming instructions: wording dependent on routing)

CQ4) [Imagine that you decide to look for a new job in the next month.] Which of the following ways will be the most effective in helping you to find a new job?

- ☐ reading advertisements (on the internet or otherwise)
- ☐ answer advertisements (via internet or otherwise)
- ☐ place advertisements myself (on the internet or otherwise)
- ☐ ask around with employers
- ☐ ask people you associate frequently with, such as friends, neighbors, acquaintances[, or people at work]
- ☐ ask contacts in LinkedIn or other social networks
- ☐ through a job center (UWV)
- ☐ through a temporary employment agency
- ☐ other

(IF Answer CQ1 = "employed")
(Programming instructions: question CQ4 is the same as question 30 in wave 1B but with one additional sentence on the coronavirus outbreak).

CQ5) We now would like to ask you a question about the minimum pay you would accept in another job. Say someone offers you a job which covers the same sort of work as you do now, and which is in the same city/town where you work now. In addition, this job is not affected by the coronavirus outbreak. Imagine all financial circumstances, apart from the pay, to be equal to those in your current position. Would you consider accepting this job?

- ☐ Yes
- ☐ No

(IF Answer CQ5 = "Yes")
(Programming instructions: question CQ5 is the same as question 31 in wave 1B).

CQ6) How much would then the minimum net wages have to be such that you would accept that new job? Would you please round off the amount on whole euro.

- ☐ .. euro per month
- ☐ I do not know

D. Income block (own + peers)

(Programming instructions: questions DQ1 and DQ1b are similar to questions 17 and 17b from wave 1B and questions 1 and 1b from wave 1A but now looking forward to 2020 instead of backward to 2019)

DQ1) How much do you expect to be the total net annual income of your household in 2020?

- ☐ .. euro
- ☐ I do not know

(IF Answer DQ1 = "I do not know")

DQ1b) Please note that there is no right or wrong answer to this question. We are interested in your expectations about the net annual income in 2020 of your household. Please provide an estimate, using the categories listed below?

- ☐ Net annual income will be negative
- ☐ 0 euro
- ☐ 1 - 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 14,999 euro
- ☐ 15,000 – 19,999 euro
- ☐ 20,000 – 24,999 euro
- ☐ 25,000 – 29,999 euro
- ☐ 30,000 – 34,999 euro
- ☐ 35,000 – 39,999 euro
- ☐ 40,000 – 44,999 euro
- ☐ 45,000 – 49,999 euro
- ☐ 50,000 – 59,999 euro
- ☐ 60,000 – 69,999 euro
- ☐ 70,000 – 79,999 euro
- ☐ 80,000 – 89,999 euro
- ☐ 90,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 499,999 euro
- ☐ 500,000 euro or more
- ☐ I do not know

(Programming instructions: question DQ2 is the same as question 3 in wave 1A).

DQ2) How much higher or lower do you think **your household's total net income** will be over the next twelve months compared to the last twelve months?

Please allocate 100 points in the table below indicating how likely the listed changes are. Note that the points in the column should sum to 100.

	Points
Net income increases 8% or more	
Net income increases 4% or more, but less than 8%	
Net income increases 2% or more, but less than 4%	
Net income increases 1% or more, but less than 2%	
Net income increases or decreases less than 1%	
Net income decreases 1% or more, but less than 2%	
Net income decreases 2% or more, but less than 4%	
Net income decreases 4% or more, but less than 8%	
Net income decreases 8% or more	
Total (the points should sum to 100)	100

☐ I do not know

(Programming instructions: questions DQ3 and DQ3b are similar to questions 20 and 20b in waves 1B and 2 and questions 2 and 2b from wave 1A but now looking forward to 2020 instead of backward to 2019)

DQ3) How much do you expect to be the average net annual income of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work in 2020?

- ☐ .. euro
- ☐ I do not know

(IF Answer DQ3 = “I do not know”)

DQ3b) Please note that there is no right or wrong answer to this question. We are interested in your expectations about the average net annual income in 2020 of households that you associate frequently with. Please provide your estimate using the categories listed below?

- ☐ Average net annual income will be negative
- ☐ 0 euro
- ☐ 1 - 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 14,999 euro
- ☐ 15,000 – 19,999 euro
- ☐ 20,000 – 24,999 euro
- ☐ 25,000 – 29,999 euro
- ☐ 30,000 – 34,999 euro
- ☐ 35,000 – 39,999 euro
- ☐ 40,000 – 44,999 euro
- ☐ 45,000 – 49,999 euro
- ☐ 50,000 – 59,999 euro
- ☐ 60,000 – 69,999 euro
- ☐ 70,000 – 79,999 euro
- ☐ 80,000 – 89,999 euro
- ☐ 90,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 499,999 euro
- ☐ 500,000 euro or more
- ☐ I do not know

(IF Answer CQ1 = “Employed” OR Answer CQ1 = “Self-employed” OR Answer CQ1 = “Retired” OR Answer CQ1 = “Unemployed and actively looking for a job” OR Answer CQ1 = “Laid off due to outbreak coronavirus”)

(Programming instructions: Wording dependent on routing; Show response category “Not applicable, I do not receive a retirement benefit” only if CQ1 = “retired”)

(Programming instructions: For respondents who have to answer DQ4 and DQ4b, the order of the question is randomized such that half of these respondents answer DQ4 and DQ4b first and DQ5 and DQ5b thereafter and the other half answers DQ5 and DQ5b first and DQ4 and DQ4b thereafter)

DQ4) What do you think is the percent chance that [you will lose your current job during the next 12 months / you will stop working as self-employed/ close your business during the next 12 months / your retirement benefit will be cut during the next 12 months / you will find a job during the next 12 months / you will get back to your previous employer with the same salary and working hours as before]?

Please provide a number between 0 and 100, where 0 means “no chance” and 100 means “you are certain” that [you will lose your job / you will close your business / your retirement income will be reduced / you will find a job / you will get back to your previous employer].

- ☐ .. percent
- ☐ Not applicable, I do not receive a retirement benefit
- ☐ I do not know

(IF Answer DQ4 > 0) AND (Answer CQ1 = "Employed" OR Answer CQ1 = "Self-employed"))

(Programming instructions: use for X the value of the percentage chance provided in question DQ4)

DQ4b) You said you think there is a X percent chance that [you will lose your current job / you will stop working as self-employed or close your business] during the next 12 months. Out of this X percent chance, how much is due to the coronavirus outbreak?

Please provide a number between 0 and X, where 0 means ["job loss is unrelated to coronavirus outbreak" / "stop working as self-employed or close own business is unrelated to coronavirus outbreak"] and X means ["job loss is entirely due to the coronavirus outbreak" / "stop working as self-employed / close own business is entirely due to the coronavirus outbreak"].

☐ ..

☐ I do not know

(all respondents)

DQ5) What do you think is the average percent chance that people you associate frequently with, such as friends, neighbors, acquaintances, or people at work will lose their current job during the next 12 months?

Please provide a number between 0 and 100, where 0 means "no chance" and 100 means "you are certain" that they will lose your job.

☐ .. percent

☐ Not applicable, since people I associate frequently with typically do not have a job

☐ I do not know

(IF Answer DQ5 > 0)

(Programming instructions: use for Y the value of the percentage chance provided in question DQ5)

DQ5b) You said you think there is a Y percent chance on average that people you associate frequently with, such as friends, neighbors, acquaintances, or people at work will lose their current job during the next 12 months. Out of this Y percent chance, how much is due to the coronavirus outbreak?

Please provide a number between 0 and Y, where 0 means "job losses are unrelated to coronavirus outbreak" and X means "job losses are entirely due to the coronavirus outbreak".

☐ ..

☐ I do not know

DQ6a) What do you think is the current unemployment rate in the Netherlands?

☐ .. percent

☐ I do not know

DQ6b) What do you think will be the unemployment rate in the Netherlands in 12 months?

☐ .. percent

☐ I do not know

(IF Answer DQ6b > Answer DQ6a)

(Programming instructions: calculate the difference $Z = DQ6b - DQ6a$ and show the value of Z in the question below)

DQ6c) You said you think the unemployment rate in the Netherlands will increase by Z percent in the next 12 months. Out of this Z percent increase, how much is due to coronavirus outbreak?

Please provide a number between 0 and Z , where 0 means “increased unemployment is unrelated to coronavirus outbreak” and Z means “increased unemployment is entirely due to the coronavirus outbreak”.

- ☐ ..
- ☐ I do not know

(Programming instructions: question DQ7 is the same as question 27 in waves 1B and 2 and question 10 from wave 1A)

DQ7) Do you think **your household will be financially** better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

(Programming instructions: question DQ8 is the same as question 28 in waves 1B and 2 and question 11 from wave 1A)

DQ8) To what extent do you agree with the statement “**Differences in incomes in the Netherlands are too large**”? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree)	2	3	4	5	6	7	8	9	10 (completely agree)
☐	☐	☐	☐	☐	☐	☐	☐	☐	☐

(Programming instructions: question DQ9 is the same as question 29 in waves 1B and 2 and question 15 from wave 1A)

DQ9) Do you think that **economic conditions in the Netherlands** will be better or worse off in 12 months from now?

- ☐ Much worse off
- ☐ Somewhat worse off
- ☐ About the same
- ☐ Somewhat better off
- ☐ Much better off
- ☐ I do not know

(Programming instructions: question DQ10 is the same as question 22 in wave 1B)

DQ10) To what extent do you agree with the statement “**If I compare myself with my friends, I think in general I am financially better off**”? Please provide an answer on a scale from 1 to 7, where 1 means “completely disagree” and 7 means “completely agree”.

1 (completely disagree)	2	3	4	5	6	7 (completely agree)
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☐ ☐ ☐ ☐ ☐ ☐ ☐

(Programming instructions: question DQ11 is the same as question 14 waves 1A and 2)

DQ11) To what extent do you agree with the statement “**Differences in wealth in the Netherlands are too large**”? Please provide an answer on a scale from 1 to 10, where 1 means “completely disagree” and 10 means “completely agree”.

1 (completely disagree) 2 3 4 5 6 7 8 9 10 (completely agree)
☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

E. Consumption block

(Programming instructions: questions EQ1-EQ4 and EQ7 are taken from the consumption block in wave 2 (questions C1-C4 and C5) and adjusted where a reference period is mentioned. Questions EQ5-EQ6 are new)

(Note: similar questions with different reference periods have been asked in wave 1B)

EQ1) How much did your household spend on purchases of **durable goods in each of the last two months** (March and April 2020)?

Durable goods are goods that last in time, including for instance cars, electronics, kitchen appliances, furniture, house maintenance, jewelries, etc. (please exclude purchases of houses, apartments, etc.). Please provide an answer in euro.

March:..... euro
 ... I did not buy any durables
 ... I do not know

April: euro
 ... I did not buy any durables
 ... I do not know

(If Answer EQ1 = “I do not know” for [March/April] 2020)

EQ1b) Please provide an estimate of how much your household did spend on purchases of durable goods in the month [March/April] 2020, using the categories listed below?

- ☐ 0 euro
- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

EQ2) What do you think your household's spending on purchases of **durable goods** will be **per month** in the next two months (May and June 2020)? Please provide an answer in euro.

May: euro
... I do not have plans to buy durables in this month
... I do not know

June: euro
... I do not have plans to buy durables in this month
... I do not know

(IF Answer EQ2 = "I do not know" for [May/June] 2020)

EQ2b) Please provide an estimate of how much you think your household will spend on purchases of durable goods in the month **[May/June]** 2020, using the categories listed below?

- ☐ 0 euro
- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

EQ3) What was your households' spending on **nondurable goods and services in each of the last two months** (March and April 2020)?

Nondurable goods and services include for instance food, tobacco, alcohol, clothing, haircuts, gasoline, utilities, transportation, and other small services and nondurable goods that do not last in time. Please provide an answer in euro.

March: euro
... I do not know

April: euro
... I do not know

(IF Answer EQ3 = "I do not know" for [March/April] 2020)

EQ3b) Please provide an estimate of how much your household did spend on nondurable goods and services in the month **[March/April]** 2020, using the categories listed below?

- ☐ 0 euro
- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 – 29,999 euro
- ☐ 30,000 – 49,999 euro
- ☐ 50,000 – 99,999 euro
- ☐ 100,000 – 199,999 euro
- ☐ 200,000 euro or more
- ☐ I do not know

EQ4) Which of the expense items below did your household spend money on in each of the last two months (**March and April 2020**)? Multiple answers possible.

	March	April
A house or apartment	☐	☐
A car or other vehicle	☐	☐
Large appliances, furniture, electronics (incl. gadgets)	☐	☐
Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐
Holiday packages and/or trips abroad	☐	☐
Vacation homes, boats or other big-ticket recreational equipment	☐	☐
None of the above	☐	☐

EQ5) What was your households' additional spending on **nondurable goods and services and durable goods** in the last two months (**March and April 2020 together**) that was related to the **coronavirus outbreak**, i.e. expenses on for instance hand sanitizer, masks, other medical supplies, toilet paper, storable food, etc. that you otherwise would not have made?

Answer: euro
 ... I do not know

(IF Answer EQ5 > 0 OR Answer EQ5 = "I do not know")

EQ5b) Which of the expense items below did your household spend more money on in the last two months (**March and April 2020 together**) related to the coronavirus outbreak than you otherwise would have made? Multiple answers possible.

- ☐ Hand sanitizer
- ☐ Masks
- ☐ Other medical supplies
- ☐ Toilet paper
- ☐ Storable food
- ☐ Other
- ☐ No additional spending related to the coronavirus outbreak

EQ6) What do you think was the average additional spending on **nondurable and durable goods** of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work in the last two months (**March and April 2020 together**) that was related to the **coronavirus outbreak**?

Answer: euro
... I do not know

(Programming instructions: question EQ7 is taken from waves 1B and 2 and is the same as question 7 from wave 1A as well)

EQ7) Generally speaking, do you think that now is a good time or a bad time **to buy**...

	Very bad	Bad	Neither good nor bad	Good	Very good
- A house or apartment	☐	☐	☐	☐	☐
- A car or other vehicle	☐	☐	☐	☐	☐
- Large appliances, furniture, electronics (incl. gadgets)	☐	☐	☐	☐	☐
- Luxury items such as jewelries, expensive watches, pieces of artwork	☐	☐	☐	☐	☐
- Holiday packages and/ or trips abroad	☐	☐	☐	☐	☐
- Vacation homes, boats or other big-ticket recreational equipment	☐	☐	☐	☐	☐

F. COVID-19 block

(Programming instructions: please show the four answer items in randomized order on the screen)

FQ1) How concerned are you about the impact of the coronavirus on each of the following:

- The Netherlands's economic situation
- Your own health or the health of the members of your household
- The financial situation of your household
- The financial situation of households that you associate frequently with, such as friends, neighbors, acquaintances, or people at work

Please provide an answer on a scale from 1 to 7, where 1 means "very much unconcerned" and 7 means "very much concerned".

1 (very much unconcerned) 2 3 4 5 6 7 (very much concerned)
☐ ☐ ☐ ☐ ☐ ☐ ☐

FQ2) Since January 2020, has your net household income been affected by the coronavirus outbreak?

- ☐ Yes, it has decreased
- ☐ Yes, it has increased
- ☐ No, it has not been affected

(IF Answer FQ2 = "Yes, it has decreased")

FQ2a) Could you provide an estimate of total loss in your net household income due to the coronavirus outbreak since January 2020? (Please provide an answer in euro)

- ☐ .. euro
- ☐ I do not know

(IF Answer FQ2a = "I do not know")

FQ2b) Please provide an estimate of total loss in your net household income due to the coronavirus outbreak since January 2020, using the categories listed below?

- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro
- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 euro or more
- ☐ I do not know

FQ3) Since January 2020, has your household financial wealth (that is not including housing wealth) been affected by the coronavirus outbreak?

- ☐ Yes, it has decreased
- ☐ Yes, it has increased
- ☐ No, it has not been affected

(IF Answer FQ3 = "Yes, it has decreased")

FQ3a) Could you provide an estimate of total loss in your household financial wealth due to the coronavirus outbreak since January 2020? (Please provide an answer in euro)

- ☐ .. euro
- ☐ I do not know

(IF Answer FQ3a = "I do not know")

FQ3b) Please provide an estimate of total loss in your household financial wealth due to the coronavirus outbreak since January 2020, using the categories listed below?

- ☐ 1 - 49 euro
- ☐ 50-99 euro
- ☐ 100 - 199 euro
- ☐ 200 - 299 euro
- ☐ 300 - 499 euro
- ☐ 500 - 999 euro

- ☐ 1,000 -1,999 euro
- ☐ 2,000 – 2,999 euro
- ☐ 3,000 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 19,999 euro
- ☐ 20,000 euro or more
- ☐ I do not know

(IF Answer FQ2 = "Yes, it has decreased" OR Answer FQ2="Yes, it has increased")

FQ4a) How much longer do you think it will be until your household's net income returns to January 2020 levels? Please use the relevant unit of time (either years, months, or weeks).

- ☐ .. years.
- ☐ .. months
- ☐ .. weeks

(IF Answer FQ3 = "Yes, it has decreased" OR Answer FQ3="Yes, it has increased")

FQ4b) How much longer do you think it will be until your household's financial wealth (that is not including housing wealth) returns to January 2020 levels? Please use the relevant unit of time (either years, months, or weeks).

- ☐ .. years.
- ☐ .. months
- ☐ .. weeks

FQ5) How much longer do you think it will be before economic activity returns to normal in Netherlands? Please use the relevant unit of time (either years, months, weeks, or days).

- ☐ .. years
- ☐ .. months
- ☐ .. weeks

(Programming instructions: please show the three responses in randomized order on the screen)

FQ6) Please rate the following responses to the coronavirus outbreak on a scale from 1 to 7, where 1 means "very poor" and 7 means "very good"?

- | | | | | | | | |
|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| 1 (very poor) | 2 | 3 | 4 | 5 | 6 | 7 (very good) | I do not know |
| ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ |

- Response of the European Central Bank (ECB) to the coronavirus outbreak
- Response of the European Commission (EC) to the coronavirus outbreak
- The financial support measures of the Dutch government in response to the coronavirus outbreak

FQ7) Have you or another member of your household been diagnosed by any of the following medical conditions? Multiple answers possible.

- ☐ Asthma
- ☐ COPD
- ☐ Diabetes

- ☐ High blood pressure
- ☐ None of these medical conditions have been diagnosed (not for me nor for another household member)

FQ8) To the best of your knowledge, how many out of the ten people that you associate most frequently with, such as friends, neighbors, acquaintances, or people at work have been diagnosed with coronavirus?

- ☐ 0 (none)
- ☐ 1
- ☐ 2
- ☐ 3
- ☐ 4
- ☐ 5
- ☐ 6
- ☐ 7
- ☐ 8
- ☐ 9
- ☐ 10 (all ten)

XQ1) This is the last question. Imagine you and another player are playing a game in which each player requests an amount of money. This amount must be (an integer) between 11 and 20 euro. Each player will receive the amount he requests. Furthermore, a player will receive an **additional** amount of 20 euro if he asks for exactly one euro less than the other player.

What amount of money would you request?

- ☐ 11 euro
- ☐ 12 euro
- ☐ 13 euro
- ☐ 14 euro
- ☐ 15 euro
- ☐ 16 euro
- ☐ 17 euro
- ☐ 18 euro
- ☐ 19 euro
- ☐ 20 euro
- ☐ I do not know

Appendix C: Derivations

This appendix provides the derivations underlying the comparative statics summarized in Table 3 and discussed in Section 3. We use the notation of the main text throughout. A household chooses durable consumption c_d , nondurable consumption c_n , work time l , socializing s , private leisure h , and borrowing b to maximize

$$U = \frac{C^{1-\sigma}}{1-\sigma} + \alpha \ln R + \beta s^\nu R^\delta + \varphi h^\nu - \frac{\kappa}{1+\varepsilon} l^{1+\varepsilon} - \frac{\xi}{1+\theta} b^{1+\theta},$$

subject to

$$p_d c_d + c_n = \lambda (wl)^{1-\tau} + b$$

and

$$l + s + h = 1.$$

Disposable income is $y = \lambda (wl)^{1-\tau}$ and relative income is $R = y/y^{peer}$. The consumption composite is

$$C = R^\gamma [(c_d R^{-\mu})^\rho + c_n^\rho]^{1/\rho}, \quad \epsilon = \frac{1}{1-\rho},$$

where ϵ is the elasticity of substitution between durable and nondurable consumption. To avoid confusion, ε denotes the curvature of labor disutility, while ϵ denotes the elasticity of substitution in consumption.

C.1 The consumption price index

Define the inner CES composite

$$Q \equiv [(c_d R^{-\mu})^\rho + c_n^\rho]^{1/\rho},$$

so that $C = R^\gamma Q$. Let $q_d \equiv c_d R^{-\mu}$. Purchasing one unit of q_d requires R^μ units of the physical durable good, so its effective price is $p_d R^\mu$. The unit-expenditure function for Q is therefore

$$\tilde{P}(R) = [(p_d R^\mu)^{1-\epsilon} + 1]^{1/(1-\epsilon)}.$$

Since one unit of C requires $R^{-\gamma}$ units of Q , the unit-expenditure function for the consumption composite is

$$P(R) = R^{-\gamma} [p_d^{1-\epsilon} R^{\mu(1-\epsilon)} + 1]^{1/(1-\epsilon)}.$$

Thus total consumption expenditure $X \equiv p_d c_d + c_n$ satisfies

$$X = P(R)C.$$

The durable expenditure share is

$$s_d \equiv \frac{p_d c_d}{X} = \frac{(p_d R^\mu)^{1-\epsilon}}{(p_d R^\mu)^{1-\epsilon} + 1}.$$

Differentiating the log price index with respect to relative income gives

$$\eta_P \equiv \frac{\partial \ln P(R)}{\partial \ln R} = -\gamma + \mu s_d.$$

The term $-\gamma$ is the keeping-up channel: a higher R raises the utility generated by a given physical consumption bundle and therefore lowers its effective price. The term μs_d is the conspicuous-consumption channel: a higher R raises the effective price of durable consumption through the $R^{-\mu}$ term in the aggregator.

C.2 Spending with work time fixed

Consider first the short run in which l , and hence current disposable income y , is fixed. Since $R = y/y^{peer}$,

$$d \ln R = -d \ln y^{peer}.$$

Conditional on R , the household can equivalently choose total expenditure X and finance any expenditure above current income through $b = X - y$. Substituting $C = X/P(R)$ into utility, the first-order condition for borrowing is

$$X^{-\sigma} P(R)^{\sigma-1} = \xi b^\theta.$$

Let

$$\omega_l \equiv \frac{y}{X},$$

so that $b/X = 1 - \omega_l$. Define $x \equiv d\ln X/d\ln R$ and $q \equiv d\ln b/d\ln R$. Log-differentiating the borrowing condition gives

$$-\sigma x + (\sigma - 1)\eta_P = \theta q.$$

Because y is fixed, $dX = db$, implying

$$x = (1 - \omega_l)q.$$

Combining these two expressions yields the exact short-run elasticity

$$\left. \frac{d\ln X}{d\ln R} \right|_{SR} = \frac{(1 - \omega_l)(\sigma - 1)}{\theta + \sigma(1 - \omega_l)} \eta_P = \frac{(1 - \omega_l)(\sigma - 1)}{\theta + \sigma(1 - \omega_l)} (\mu s_d - \gamma).$$

The sign is therefore governed by $\eta_P = \mu s_d - \gamma$ when $\sigma > 1$. In particular, if $\gamma > \mu s_d$, then $\eta_P < 0$ and a decline in R raises total spending. A larger θ or a larger labor-income share ω_l attenuates the spending response.

For expositional simplicity, Table 3 reports the local approximation obtained when the induced short-run movement in expenditure is small relative to the curvature of borrowing costs, so that $\sigma(1 - \omega_l)/\theta$ is small:

$$\left. \frac{d\ln X}{d\ln R} \right|_{SR} \simeq (1 - \omega_l) \frac{\sigma - 1}{\theta} (\mu s_d - \gamma).$$

The exact expression and the approximation have the same sign and therefore deliver the same qualitative predictions in Table 3.

C.3 Composition of spending

The first-order condition for the allocation between durable and nondurable consumption can be obtained from cost minimization for the inner CES composite. The effective price of $q_d = c_d R^{-\mu}$ is $p_d R^\mu$, so standard CES demand implies

$$\frac{q_d}{c_n} = (p_d R^\mu)^{-\epsilon}.$$

Since $c_d = R^\mu q_d$,

$$\frac{c_d}{c_n} = p_d^{-\epsilon} R^{\mu(1-\epsilon)}.$$

It follows immediately that

$$\frac{d\ln(c_d/c_n)}{d\ln R} = \mu(1 - \epsilon) = -\mu(\epsilon - 1).$$

Thus, with $\mu > 0$, a decline in relative income raises durable consumption relative to nondurable consumption when $\epsilon > 1$. This compositional response does not depend on γ , which is why the durable-to-nondurable ratio helps separate the conspicuous-consumption channel from the broader keeping-up channel.

C.4 Socializing with work time fixed

Holding work time fixed, total non-work time $T \equiv 1 - l = s + h$ is fixed. The first-order conditions for socializing and private leisure are

$$\beta v s^{v-1} R^\delta = \eta, \quad \varphi v h^{v-1} = \eta,$$

where η is the multiplier on the time constraint. Dividing the two conditions gives

$$\left(\frac{s}{h} \right)^{v-1} = \frac{\varphi}{\beta R^\delta}.$$

Therefore,

$$\frac{d\ln(s/h)}{d\ln R} = \frac{\delta}{1 - v}.$$

Define the share of non-work time devoted to socializing as

$$\chi_s \equiv \frac{s}{s + h} = \frac{s}{1 - l}.$$

For fixed T , a change in the ratio s/h changes s according to

$$d\ln s = (1 - \chi_s) d\ln(s/h).$$

Hence

$$\left. \frac{d \ln s}{d \ln R} \right|_l = (1 - \chi_s) \frac{\delta}{1 - \nu}.$$

If $\delta > 0$, higher relative income raises time spent socializing, while a deterioration in relative standing reduces it.

C.5 Happiness

Let $V(R; l)$ denote optimized utility conditional on work time. By the envelope theorem, the derivative of optimized utility with respect to $\ln R$ can be evaluated holding the household's choices fixed. From the consumption aggregator,

$$\left. \frac{\partial \ln C}{\partial \ln R} \right|_{c_d, c_n} = \gamma - \mu s_d.$$

The consumption contribution to the utility derivative is therefore

$$U_C \frac{\partial C}{\partial \ln R} = C^{-\sigma} C (\gamma - \mu s_d) = C^{1-\sigma} (\gamma - \mu s_d).$$

Adding the direct status and socializing terms gives

$$\left. \frac{dV}{d \ln R} \right|_l = C^{1-\sigma} (\gamma - \mu s_d) + \alpha + \beta \delta s^\nu R^\delta.$$

This is the semi-elasticity reported in Table 3. The first term is the consumption channel, the second is direct status utility, and the third is the status-dependent utility from socializing. Because these components need not have the same magnitude, happiness can respond strongly even when the net response of total expenditure is small.

C.6 Work adjustment and persistence of relative income

The main text does not impose a particular structural sign on the work response. Instead, define the reduced-form elasticity

$$\Phi \equiv \frac{d \ln l}{d \ln R}.$$

A negative Φ means that a deterioration in relative standing induces additional work. A positive Φ means that lower relative standing reduces work. The exact sign can depend on the competing effects of relative standing on the marginal value of resources and on the value of non-work time; the subsequent comparative statics only require the elasticity Φ itself.

Because

$$y = \lambda (wl)^{1-\tau},$$

we have

$$d \ln y = (1 - \tau) d \ln l.$$

Using $R = y/y^{peer}$,

$$d \ln R = (1 - \tau) d \ln l - d \ln y^{peer}.$$

Substituting $d \ln l = \Phi d \ln R$ gives

$$[1 - (1 - \tau)\Phi] d \ln R = -d \ln y^{peer},$$

and therefore

$$\frac{d \ln R}{d \ln y^{peer}} = -\frac{1}{1 - (1 - \tau)\Phi}.$$

If $\Phi = 0$, peer income maps one-for-one and with the opposite sign into relative income. If $\Phi < 0$, the denominator exceeds one and endogenous work adjustment attenuates the initial decline in R . If $\Phi > 0$, the response can amplify the initial shock, provided the denominator remains positive.

1. Spending after work adjustment

Once work is allowed to adjust, $d \ln y / d \ln R = (1 - \tau)\Phi$. The budget identity implies

$$\frac{d \ln X}{d \ln R} = \omega_l (1 - \tau)\Phi + (1 - \omega_l) \frac{d \ln b}{d \ln R}.$$

Combining this identity with the log-differentiated borrowing condition gives the exact elasticity

$$\frac{d\ln X}{d\ln R} = \frac{\omega_l(1-\tau)\Phi + (1-\omega_l)\frac{\sigma-1}{\theta}(\mu s_d - \gamma)}{1 + \frac{\sigma(1-\omega_l)}{\theta}}.$$

Under the same local approximation used for the short-run expression in Table 3, this becomes

$$\frac{d\ln X}{d\ln R} \simeq \omega_l(1-\tau)\Phi + (1-\omega_l)\frac{\sigma-1}{\theta}(\mu s_d - \gamma),$$

which is the expression discussed in Section 3.3. The first term is the endogenous labor-income channel, while the second is the short-run financing channel.

2. Socializing after work adjustment

Let $T = 1 - l$. Since $s = \chi_s T$,

$$d\ln s = d\ln \chi_s + d\ln T.$$

The within-non-work-time allocation gives

$$\frac{d\ln \chi_s}{d\ln R} = (1 - \chi_s) \frac{\delta}{1 - \nu},$$

while

$$\frac{d\ln T}{d\ln R} = -\frac{l}{1-l}\Phi.$$

Therefore,

$$\frac{d\ln s}{d\ln R} = (1 - \chi_s) \frac{\delta}{1 - \nu} - \frac{l}{1-l}\Phi.$$

The first term is the direct reallocation of non-work time toward socializing as relative status changes; the second captures the endogenous change in the total amount of non-work time generated by the labor response.

C.7 Extension: equity and preferred redistribution

To formalize the redistribution discussion in Section 3.4, suppose households take the tax system as given when choosing consumption, work, and time allocation, but also have preferences over the degree of tax progressivity τ . The extension separates the mechanical private gain from redistribution from a perceived efficiency cost.

Let pretax earnings be $z_i = w_i l_i$. Under the disposable-income schedule used in the main model,

$$y_i = \lambda(\tau) z_i^{1-\tau}.$$

Holding the pretax earnings distribution fixed when calculating the mechanical redistributive effect, government budget balance implies

$$\lambda(\tau) = \frac{\int z_i di}{\int z_i^{1-\tau} di}.$$

It follows that

$$\frac{d\ln \lambda}{d\tau} = \frac{\int z_i^{1-\tau} \ln z_i di}{\int z_i^{1-\tau} di} \equiv \overline{\ln z}_\tau.$$

Thus the mechanical effect of greater progressivity on household i 's disposable income is

$$\frac{d\ln y_i}{d\tau} = \overline{\ln z}_\tau - \ln z_i.$$

A household with earnings below the relevant weighted mean therefore gains mechanically from greater progressivity. If the household's peer group is approximately representative of this benchmark, then

$$\overline{\ln z}_\tau \simeq \ln z^{peer}.$$

Because relative disposable income satisfies

$$\ln R = (1 - \tau)(\ln z_i - \ln z^{peer}),$$

we obtain

$$\frac{d\ln y_i}{d\tau} \simeq -\frac{\ln R}{1-\tau}.$$

For $R < 1$, this term is positive: households below their peers stand to gain from greater progressivity.

Let ζ denote the marginal utility value of one additional euro of disposable resources. From the consumption and borrowing problem,

$$\zeta = X^{-\sigma} P(R)^{\sigma-1}.$$

With work time fixed, its elasticity with respect to relative income is

$$\eta_\zeta \equiv \frac{d\ln \zeta}{d\ln R} = -\sigma \frac{d\ln X}{d\ln R} \Big|_{SR} + (\sigma - 1)\eta_P.$$

Using the exact short-run spending elasticity derived above,

$$\eta_\zeta = \frac{(\sigma - 1)\theta}{\theta + \sigma(1 - \omega_l)} \eta_P.$$

Hence, when $\sigma > 1$ and $\eta_P < 0$, we have $\eta_\zeta < 0$: a deterioration in relative standing raises the marginal utility value of additional resources.

To capture the perceived efficiency cost of redistribution, let $D(\tau)$ be increasing and convex, with $D'(\tau) > 0$ and $D''(\tau) > 0$. We allow perceived distortion costs to depend on relative standing through R^ω , where $\omega > 0$. This reduced-form term captures the empirical pattern that households with higher relative income place greater weight on the incentive role of income differences. After normalizing the positive scale factor in the mechanical transfer gain into the units of D , the preferred progressivity τ^* satisfies

$$-\zeta(R)\ln R = R^\omega D'(\tau^*).$$

The left-hand side is the private marginal benefit from greater redistribution; for $R < 1$ it is positive. The right-hand side is the perceived marginal efficiency cost.

Define the elasticity of the slope of the distortion cost as

$$\eta_D \equiv \frac{\tau^* D''(\tau^*)}{D'(\tau^*)} > 0.$$

Differentiating the optimality condition with respect to $\ln R$ and using the optimality condition to eliminate ζ yields

$$\frac{d\ln \tau^*}{d\ln R} = \frac{\eta_\zeta - \omega + 1/\ln R}{\eta_D}.$$

This expression isolates the three channels described in Section 3.4. First, for $R < 1$, $1/\ln R < 0$: households further below their peers have a larger private gain from redistribution. Second, $-\omega < 0$: if higher relative standing increases the perceived incentive cost of redistribution, then lower relative standing makes redistribution appear less costly. Third, when $\eta_P < 0$ and $\sigma > 1$, $\eta_\zeta < 0$: lower relative standing raises the marginal utility value of transfers. Under these conditions,

$$\frac{d\ln \tau^*}{d\ln R} < 0.$$

Finally, the experimental treatment moves perceived peer income rather than own income. Combining this comparative static with the work-adjustment mapping gives

$$\frac{d\ln \tau^*}{d\ln y^{peer}} = \frac{d\ln \tau^*}{d\ln R} \left[-\frac{1}{1 - (1 - \tau)\Phi} \right].$$

Provided $1 - (1 - \tau)\Phi > 0$, a negative response of preferred progressivity to relative income implies that higher perceived peer income raises the preferred degree of redistribution. The same perceived-cost channel provides a natural interpretation for the finding that higher relative standing increases the perceived incentive value of income differences without necessarily changing beliefs about whether higher incomes are deserved.

Appendix D: Vignette Questionnaire

1) How much do you think the total net **annual income** (that is, after taxes and deductions) of an average household in the Netherlands was in 2025? Please provide an estimate.

- ☐ .. euro
- ☐ I do not know

(If Answer 1 = "I do not know")

1b) Please note that there is no right or wrong answer to this question. We are interested in your view of the net **annual income** (that is, after taxes and deductions) of an average household in the Netherlands in 2025. Please provide your estimate using the categories listed below?

- ☐ 10,000 – 14,999 euro
- ☐ 15,000 – 19,999 euro
- ☐ 20,000 – 24,999 euro
- ☐ 25,000 – 29,999 euro
- ☐ 30,000 – 34,999 euro
- ☐ 35,000 – 39,999 euro
- ☐ 40,000 – 44,999 euro
- ☐ 45,000 – 49,999 euro
- ☐ 50,000 – 59,999 euro
- ☐ 60,000 – 69,999 euro
- ☐ 70,000 – 79,999 euro
- ☐ 80,000 – 89,999 euro
- ☐ 90,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 499,999 euro
- ☐ 500,000 euro or more
- ☐ I do not know

2. What was the total net annual income (that is, after taxes and deductions) of your household in 2025? Please provide an estimate.

- ☐ Net annual income was negative
- ☐ No income
- ☐ 1 – 4,999 euro
- ☐ 5,000 – 9,999 euro
- ☐ 10,000 – 14,999 euro
- ☐ 15,000 – 19,999 euro
- ☐ 20,000 – 24,999 euro
- ☐ 25,000 – 29,999 euro
- ☐ 30,000 – 34,999 euro
- ☐ 35,000 – 39,999 euro
- ☐ 40,000 – 44,999 euro
- ☐ 45,000 – 49,999 euro
- ☐ 50,000 – 59,999 euro
- ☐ 60,000 – 69,999 euro
- ☐ 70,000 – 79,999 euro
- ☐ 80,000 – 89,999 euro
- ☐ 90,000 – 99,999 euro
- ☐ 100,000 – 149,999 euro
- ☐ 150,000 – 199,999 euro
- ☐ 200,000 – 499,999 euro
- ☐ 500,000 euro or more
- ☐ I do not know

3. All things considered, how satisfied are you with your life as a whole nowadays?

- ☐ Extremely satisfied
☐ Satisfied
☐ Neither satisfied nor dissatisfied
☐ Dissatisfied
☐ Extremely dissatisfied
☐ I do not know

4. Imagine that you learn the net **annual income** of your household is 20% (**lower/higher**) than the average net annual income of households that you associate frequently with (friends, neighbors, acquaintances, or people at work). **How fair does this seem to you?** Please provide an answer on a scale from 1 to 7, where 1 means “completely unfair”, 4 means “neither fair nor unfair” and 7 means “completely fair”.

1 (completely unfair)	2	3	4	5	6	7 (completely fair)
☐	☐	☐	☐	☐	☐	☐

5. Imagine that you learn the net **annual income** of your household is 20% (**lower/higher**) than the average net annual income of households that you associate frequently with (friends, neighbors, acquaintances, or people at work). **How happy does this information make you feel?** Please provide an answer on a scale from 1 to 7, where 1 means “very unhappy”, 4 means “neither happy nor unhappy” and 7 means “very happy”.

1 (very unhappy)	2	3	4	5	6	7 (very happy)
☐	☐	☐	☐	☐	☐	☐

6. Imagine that you learn the net **annual income** of your household is 20% (**lower/higher**) than the average net annual income of households that you associate frequently with (friends, neighbors, acquaintances, or people at work). **How does knowing this make you feel about your financial situation?** Please provide an answer on a scale from 1 to 7, where 1 means “much worse”, 4 means “neither worse nor better” and 7 means “much better”.

1 (much worse)	2	3	4	5	6	7 (much better)
☐	☐	☐	☐	☐	☐	☐

7. Imagine that you learn the net annual income of your household is 20% (**lower/higher**) than the average net annual income of households that you associate frequently with (friends, neighbors, acquaintances, or people at work). **Would you like to change the income taxes that we have in Netherlands today, and if so in what way?** Please provide an answer on a scale from 1 to 3, where 1 means “lower taxes”, 2 means “no change” and 3 “increase taxes”.

8. For this next question, we would like you to think about the ways in which your perception about the average net annual income of households that you associate frequently with (friends, neighbors, acquaintances, or people at work) affect your labor decisions. Imagine the net annual income of your household is 20% (**lower/higher**) than that average. **Would you change the weekly number of hours you work relative to today?**

- ☐ I would increase the number of hours I work per week
☐ I would decrease the number of hours I work per week
☐ I would not change the number of hours I work per week

By how many hours per week would you increase? ____

By how many hours per week would you decrease? ____

9. For this next question, we would like you to think about the ways in which your perception about the average net annual income of households that you associate frequently (friends, neighbors, acquaintances, or people at work) affects the time you spend socializing. Imagine that the net annual income of your household is 20% (lower/higher) than that average. **Would you change the weekly number of hours you socialize with peers relative to today?**

- ☐ I would increase the number of hours I socialize per week
- ☐ I would decrease the number of hours I socialize per week.
- ☐ I would not change the number of hours I socialize per week.

By how many hours per week would you increase? _____

By how many hours per week would you decrease? _____

10. For this next question, we would like you to think about the ways in which your perception about the average net **annual income** of households that you associate frequently with (friends, neighbors, acquaintances, or people at work) affect your economic decisions. Imagine that you hear the net **annual income** of your household is 20% (**lower/higher**) than that average. **What would you do in response to this information?**

*Non-durable goods and services include for instance food, tobacco, alcohol, clothing, haircuts, gasoline, utilities, transportation, and other small services and nondurable goods that do not last in time

- Buy a durable good (house, car, big item)	1() More likely
- Increase my non-durable spending	2() No change
-Save	3() Less likely
-Borrow	4() Don't know

[randomize: after ONE of questions 4-10, ask]

Could you please explain your answer in your own words?

11. Imagine that you hear the net **annual income** of your household is 20% (lower/higher) than the average net **annual income** of households that you associate frequently with. What conclusion, if any, would you draw about the **income** of other people in the country that are not necessarily people you associate with:
- I would assume their income is higher than I thought before,
 - I would assume their income is the same as what I thought before,
 - I would assume their income is lower than I thought before.

INFORMATION TREATMENT

;RANDOMLY ASSIGN RESPONDENTS TO ONE OF 2 GROUPS:

;Group 1 (1/2 of the sample): go straight to final questions

;Groups 2: (1/2 of the sample)

;SHOW IF Group 1:

You are almost done with the survey.

;SHOW IF Group 2:

You are almost done with the survey. Before the final questions, we would like you to know the following:

; keep this screen for 5 seconds

The total net **annual income** (that is, after taxes and deductions) of an average household in the Netherlands was approximately €62,800 in 2025.

12. How much do you think the total net **annual income** (that is, after taxes and deductions) of an average household in the Netherlands will be in 2026? Please provide an estimate.

☐ .. euro

☐ I do not know

(IF Answer 1 = "I do not know")

11b) Please note that there is no right or wrong answer to this question. We are interested in your view of the net **annual income** (that is, after taxes and deductions) of an average household in the Netherlands in 2026. Please provide your estimate using the categories listed below?

☐ 10,000 – 14,999 euro

☐ 15,000 – 19,999 euro

☐ 20,000 – 24,999 euro

☐ 25,000 – 29,999 euro

☐ 30,000 – 34,999 euro

☐ 35,000 – 39,999 euro

☐ 40,000 – 44,999 euro

☐ 45,000 – 49,999 euro

☐ 50,000 – 59,999 euro

☐ 60,000 – 69,999 euro

☐ 70,000 – 79,999 euro

☐ 80,000 – 89,999 euro

☐ 90,000 – 99,999 euro

☐ 100,000 – 149,999 euro

☐ 150,000 – 199,999 euro

☐ 200,000 – 499,999 euro

☐ 500,000 euro or more

☐ I do not know

13. All in all, to what extent do you consider yourself a happy person?

☐ Very happy

☐ Happy

☐ Neither happy, nor unhappy

☐ Unhappy

☐ Very unhappy

☐ I do not know