DNB Working Paper
No 804/ February 2024

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DeNederlandscheBank
EUROSYSTEEM

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* Views expressed are those of the authors and do not necessarily reflect official positions of De Nederlandsche Bank.

Working Paper No. 804

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February 2024

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

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This draft: January 25, 2024

Abstract: How strong are peer effects on the beliefs and spending decisions of individuals? We use a randomized control study in which treated households are told about either average income or debt of individuals like them to assess how peer effects influence their beliefs and spending. The information treatments are successful at moving respondents' beliefs about peers' incomes and debt levels. We find that individuals with exogenously higher perceived relative income become more opposed to redistribution and increase the amount of time they spend socializing with peers. In addition, we find some evidence of reallocative "keeping up with the Joneses" on spending, as those who learn their peers earn more than they thought tend to reallocate their spending toward durable goods and away from non-durables. However, the quantitative magnitude of peer effects on spending is small in the months following the information experiment. Peer effects also matter for labor supply decisions and ex-post employment outcomes. Finally, believing that one earns more than peers causally leads to large positive effects on happiness, above and beyond effects coming from spending more time with peers, changing beliefs about redistribution, or changes in spending patterns.

JEL Codes: D3, D6, D1, E21
Keywords: Peer effects, surveys

The views expressed in this paper are those of the authors and do not necessarily reflect the views of the European Central Bank or of De Nederlandsche Bank. We are grateful to Ruediger Bachmann and participants of the NBB workshop on macroeconomics and survey data in Brussels. Ordering of authors is randomized.

"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 it may 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 level and that only relative income levels within a population matter for happiness (Easterlin 1974, Deaton 2008). The potential importance of perceived peer incomes has also been emphasized in other contexts. In macroeconomics and finance for example, theoretical and empirical work has argued that "keeping up with the Joneses" effects—in which spending patterns depend in part on how much peers are consuming, especially on conspicuous items—are strong and can account for empirical patterns like consumption inertia and help resolve asset pricing puzzles (Campbell and Cochrane 1999, 2002, 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 in separating causality from correlations. In the Manski (1993) terminology, one needs to identify "exogenous (or contextual) effects" rather than "endogenous effects".

In this paper, we provide new *causal* evidence on the role of perceived relative income and debt standings on a range of individual beliefs and actions. We use a novel approach that exogenously changes individual perceptions about relative standings by implementing a randomized controlled trial (RCT) in a representative survey of households in the Netherlands. We report five key findings. First, when individuals have exogenously higher beliefs about their income relative to their peers, they are less likely to believe that income and wealth differences are too large and less supportive of trying to reduce income inequality, consistent with earlier experimental evidence in Cruces et al. (2013). However, we show that this is not because they increasingly view income and wealth disparities as a fair outcome reflecting differences in work ethic across individuals but rather because they are increasingly viewing income differences as incentivizing. Second, perceived income standings also matter for the amount of time that individuals spend socializing with peers, a previously unexplored margin through which peer effects can influence behavior. Higher perceived relative income increases the amount of time spent socializing with peers in recreational settings but does not affect the extent to which individuals participate in political or other civic organizations. Third, we find that when individuals learn that their peers earn more than what they thought, these

individuals become more likely to be employed in subsequent months and their expected household income rises. Fourth, spending responds to perceived relative income standings in the qualitative manner described by "keeping up with the Joneses". However, the quantitative magnitudes are quite small in the months following the information experiment and far from those needed to rationalize macroeconomic and finance puzzles. Finally, believing that one makes more money relative to peers causally and meaningfully increases self-reported happiness, above and beyond effects coming from changes in beliefs about inequality, time spent with peers, or spending patterns.

To establish these results, we run several survey waves in 2020. In the first wave, respondents were asked demographic and economic information as well as questions about how much they thought their peers earned and how much debt they had. A randomly selected subset of these households was provided two weeks later with information about the actual average income or debt of their peers, with the latter group being identified through age, education, marital status, and, in the case of debt, homeownership. In follow-up waves done over three months after the first wave, respondents were then asked a number of additional questions about their beliefs and actions, which allow us to assess whether these were impacted by the information treatments. Key to our identification, therefore, is the randomized information treatment, which we show was successful in generating meaningful revisions in respondents' beliefs about their relative income and debt levels. 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 Wolhfart 2020, Cavallo et al. 2017), finance (Beutel and Weber 2023), political economy (Cruces et al. 2013, Stantcheva 2020), and labor economics (Jager et al. 2022) among others. We differ from much of this work in that information treatments are tailored to the individual's peer group, as in Hvidberg et al. (2023), where the peer group is defined based on age, education, and region. This yields an additional testable implication from Bayesian learning, namely that the coefficients on the provided individual-specific signal and their prior belief sum to zero, a prediction which is borne out in the data.

From this exogenous variation in perceptions about income and debt relative to peers, we can assess whether and how relative income or debt matter for individuals' beliefs and decisions. Existing research has found a link between relative income standing and borrowing, showing that in order to "keep up with the Joneses" households are willing to borrow more and assume a high debt burden that may induce financial stress (see Georgarakos et al. 2014 and Agarwal et al. 2020). However, little is known on whether our beliefs about how much our peers *borrow* relative to our debt matter. Shedding light to a possible relative debt channel can be insightful in view of private debt growth and proliferation of various borrowing instruments

for households over the past few decades. More borrowing on behalf of households may address real liquidity needs and support their finances but may also increase financial vulnerabilities if debt is used counterproductively e.g., to only finance conspicuous consumption. Thus, it is interesting to assess whether households partly base their behavior on views about their relative debt position.

Another motivation to look at these two channels separately is that views about relative income and debt standings may influence individual beliefs, e.g., about equity and redistribution, in quite different ways, especially as certain policies are seen as having different implications for different population segments. For example, the recent prolonged period of ultra-low interest rates has been often seen as rewarding borrowers while, at the same time, punishing savers and has thus created some sense of social injustice that likely differs across population groups. Another application is to the forgiveness of college debt, as done by the Biden administration in the U.S. Moreover, income and debt have differential effects on happiness and thus we are able to assess whether relative income and debt matter too.

We track a variety of individual-level outcomes and decisions by fielding follow-up surveys in the months after our information experiment. Combining the exogenous variation in perceived relative income and debt with the various outcomes allows us to speak directly to whether and how relative standing affect individual beliefs and decisions.

Our first application is to beliefs about inequality and redistribution. Prior work has also applied RCT strategies to address this topic and found mixed results. For example, 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 (i.e., "bad news") demand more redistribution whereas Hoy and Mager (2021) find that they demand less. Karajda et al. (2017) instead find that it is only those who are told their relative income is higher than they thought ("good news") who respond to the information, and they do so by demanding more redistribution. Fehr et al. (2022) find no link in the context of global inequality. Relative to this earlier work, we make three distinct contributions. First, we provide new evidence from the Netherlands and find that as perceived relative income rises, individuals are less likely to think that income and wealth inequality are too large and less likely to think policies should be used to make income more equal, i.e., the demand for redistribution falls with perceived relative income. In addition, we find no statistically significant difference between those receiving "good news" versus those receiving "bad news" about their relative income standing. Second, we are able to provide more nuance on *why* perceived relative income shapes the desire for redistribution. We find that when someone learns that their relative income is higher than they thought, it is *not* the case that they become more likely to think that income differences are fair or justified by hard

work. Rather, they become more likely to view income differences as incentivizing. Third, we go beyond relative income differences and study perceived relative debt standings. We find that while these are often unconditionally correlated with measures of the desire for redistribution, there is no causal link running from perceived debt standings to beliefs about desired levels of inequality or the need for redistribution.

Our second main result focuses on the link between relative income and time spent with peers. We provide novel, albeit tentative, evidence that as individuals perceive themselves as earning higher income relative to their peers, they tend to increase the amount of time spent outside of work with friends, neighbors and other peers. We do not find evidence that this occurs via increased participation in political or other civic organizations but instead through sports and other community centers. To the best of our knowledge, we are the first to identify any link, and especially a causal link, between perceived relative income and time spent socializing with peers.

Third, we consider the influence on peer effects on employment. Work by Card et al. (2012) as well as Jager et al. (2022) has found that when workers learn about what their peers earn, this leads them to engage in more job search. However, evidence on actual job outcomes has been more limited. Because our data have a panel structure, we are able to assess how exogenous variation in beliefs about peers' incomes affects actual employment decisions. We find an asymmetric response in line with Card et al. (2012). When workers learn their peers earn more than they thought, they become more likely to be employed in subsequent months or less likely to become non-employed. We also find positive effects on their expected income for that year.

Fourth, we study the link between perceived income and debt standings with respect to actual 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 (e.g., Cochrane and Campbell 1999, 2002) models but causal evidence on this mechanism is limited. Much of this evidence relies on the dynamics of aggregate or state-level consumption rather than individual-level outcomes and points toward a very high pass-through from others' spending to individual spending decisions (e.g., Korniotis 2010). Our follow-up surveys included questions about recent spending on non-durables and services, as well as about the extensive and intensive margins of durable goods purchases. We can therefore study directly any causal effects running from perceived relative income and debt into spending decisions. Consistent with the existence of reallocative "keeping up with Joneses" mechanism, we find that when individuals learn that their peers earn more than they thought, they tend to reallocate their consumption more toward durable goods and away from non-durable goods and services. The increase in "conspicuous" spending is consistent with earlier

evidence in Bertrand and Morse (2016), but we are able to utilize a framework that provides clearer causal identification. Our results also qualitatively support the results in Agarwal et al. (2020a, 2020b). However, quantitatively we find that these effects are quite small in the short run, and certainly much smaller than typical values commonly assumed in macroeconomics and finance. Our results are therefore a challenge to macroeconomic and finance models that rely on external habit formation to match empirical patterns and instead support other mechanisms that have been considered to match these facts (e.g., internal habit formation in macroeconomics, rare disasters in finance). When looking at perceived debt standings, we find effects that are statistically and economically indistinguishable from zero.

Our final main result focuses on the link between relative income and self-reported 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, but average happiness within a country does not increase with overall income, at least beyond a certain threshold level of income (Easterlin, 1974; Deaton, 2008). One possible explanation for this is the relative income hypothesis, the notion that happiness depends on how our income compares to that of our peers, not its overall level. Some prior work has documented strong correlations between relative income and happiness (e.g., Ferrer-i-Carbonell 2005, Luttmer 2005) although other work has questioned the importance of this effect (Stevenson and Wolfers 2008). Clear causal evidence for this mechanism is scant. One suggestive result is from Natali et al. (2018) who study the effect of large cash transfers to women in poor households and show that not only does their happiness increase as a result, but the amount of that increase is most strongly correlated with changes in their perceived relative poverty rather than by changes in their consumption. Pischke (2011) uses industry wage differentials to identify exogenous variation in income and finds evidence suggesting that higher income leads to higher well-being. Rather than identifying exogenous variation in income, we focus instead on generating exogenous variation in the perceived income of peers and using the resulting variation to study the effects on happiness. We provide new causal evidence that higher income relative to peers sharply increases individual happiness, the first evidence of this kind that we know of. Our IV estimates are three times larger than OLS estimates or unconditional correlations, and the resulting coefficient implies that relative income levels can account for a significant share of the observable variation in happiness in the data. Furthermore, our estimated effects are only somewhat dampened when we control for ex-post changes in beliefs about inequality, time spent with peers, or spending patterns, indicating that these channels through which perceived income standings affect decision-making do not play a primary role in accounting for the ultimate change in happiness. With respect to perceived relative debt

standings, we find that while these are negatively correlated unconditionally with self-reported happiness, there does not seem any causal link underlying their correlation: learning that your peers are more/less indebted than you thought does not bring a change in happiness.

The paper is organized as follows. Section 2 describes how the survey was implemented. Section 3 presents respondents' perceptions about their peers' income and debt levels. Section 4 focuses on the effectiveness of the information treatments in changing respondents' beliefs about their peers' income and debt levels. Section 5 presents our estimates of the causal effects of perceived relative income and debt standings on different beliefs and decisions of respondents. Section 6 concludes.

2. The Survey of Dutch Households

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

We use data from the Center Internet panel, which is sponsored by the Dutch National Bank (DNB) and maintained by Centerdata at Tilburg University. 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 during the course of a year in special purpose surveys.

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 follow-up week. Given that the survey allows for contacting respondents at a high frequency, we repeated (part of) the survey in February, March and May 2020 in order to track changes in beliefs and behavior in the post-treatment period. All surveys were done online. Our dataset includes 2,671 adult respondents from 2,201 households.[1]

[1] Note that parts of the follow-up survey waves overlap with the COVID 19 outbreak. The May wave occurred during the work-from-home period and the March wave coincided with the cancellation of large public events and the initiation of school

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 and debts of their peers. Specifically, for the income 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?'*

For the debt level of their peers, the question was phrased in an analogous manner:

> *"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?"*

The text explains that total debt includes 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. These income and debt questions are presented as open-ended questions 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 to the income and debt questions.

Apart from these questions, the first part of wave 1 (wave 1A) included questions on their own economic situation and attitude 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

closures. The February wave preceded the outbreak in the Netherlands. However, since the pandemic repercussions applied equally to control and treatment, it should not affect the relative outcomes across groups that are captured via treatment effects.

following question: "All in all, to what extent do you consider yourself a happy person?" with answers provided on a scale of 1 (very unhappy) to 5 (very happy).

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 and debt 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 participant of the survey was randomly assigned to one of three groups. One group was provided with 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"***

Another group was provided with the following statement:

> "*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***"

We refer to the first group (N=824) as having been treated with peer income while the second group (N=820) was treated with peer debt. The third group (N=825) receives no such information and serves as a control group. When respondents in the treated groups are shown the statements on the household income or debt 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 or debt versus the income or debt of a typical household like them and helps them to conceptualize their income and debt standings relative to their peers.[2]

When providing these information treatments, we follow Hvidberg et al. (2023) and focus on peer groups that are narrowly defined. Specifically, the values of *X* and *Y* provided to the respondents depend on the age of the respondent (age categories: <36, 36-45, 46-65, 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 the case of debt, whether the respondent is a homeowner or a renter. Appendix B provides specific values provided to each subgroup in the treatments.

[2] Appendix Figure A1 shows an example for such charts.

The treatments were followed by repeating the questions asking for an estimate of the average net annual income and debt position in 2019 of households that respondents associate frequently with. This allows us to measure the effect of the information treatments on beliefs about peers' income and debt. Following this, and over the course of two subsequent waves, 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, 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 and debt levels affect their beliefs and decisions.

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. 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 infrequent but large when it occurs: the average spending over the previous month conditional on buying a durable good is €1,663 while the median is €350.

3. Perceived Income and Debt Standings of Respondents Compared to Peers

In this section, we describe how survey respondents perceive the income and debt levels of their peers, as well as how these perceptions correlate with views on equity and the need for redistribution.

3.1 Perceived Income and Debt Levels of Peers

In the survey, respondents are asked about how much they believe their peers earn as well as about the level of debt they believe their peers have. Given that we also ask survey participants about their own household income and debt levels, we can therefore measure their perceived relative income and debt standings compared to their peers. We plot the resulting distribution of each in Panels A and B of Figure 1 for relative income and debt levels, respectively. In each case, we can see that there is significant variation in where

households perceive themselves to be relative to their peers. For income, the average relative income ratio is less than one, 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. Panel B shows the respective pattern for relative debt levels. Households on average believe that they have less debt than their peers. There is even more variation in perceived relative debt levels than in income levels. This distribution also has a fat tail, again in the direction of a disproportionate number of households believing they have far less debt than their peers.[3] In Panel C, we show the correlation between perceived relative income and debt across respondents. There is a positive unconditional correlation between them, such that those who believe their income is relatively higher than their peers also tend to believe that their debt is relatively higher than that of their peers.

Panel A of 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. For those earning around €40,000, they perceive their peers to be earning the same income as they are 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, consistent with the "center bias" documented in Hvidberg et al. (2023), Karajda et al. (2017), and Fehr et al. (2022).[4]

Panel B of Figure 2 displays the equivalent correlation for debt levels. A similar pattern can be seen between actual debt levels and perceived debt levels. Those households with debt levels of around €175,000 believe that their peers have approximately the same debt on average. As we consider households with higher debt than this, they tend to perceive their peers as having higher debt levels as well, but the increase is less than one for one. Hence, higher debt individuals tend to believe that they are relatively more in debt than their peers, whereas lower debt individuals tend to believe that they have relatively less debt than their peers.

[3] Over 25% of respondents has zero debt and approximately 15% of respondents refuse to estimate their peers' debt.
[4] Jager et al. (2022) similarly find that workers in low-paying firms tend to underestimate wages elsewhere.

As shown in Figure 1, there is much more variation in perceived relative income and debt levels than what can be accounted for by individuals' own income and debt levels. To assess what other factors correlate with their perceived rankings, we regress perceived individual relative income and debt levels 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 having a working partner: those who have a working partner generally believe their peers have similar incomes whereas those who do not perceive larger income differentials between themselves and their peers on average.

Columns (2) and (4) present equivalent results for perceived relative debt levels and absolute debt differences relative to peers. The results for relative debt levels are very different from those for income. We now find a positive effect on age, indicating that older households generally perceive their peers as being relatively more indebted. We also now find a role for education, with those having higher education levels generally perceiving themselves as having less debt than their peers. Those with more children perceive themselves as being more indebted than their peers, with each child raising the perceived extra debt by 12% relative to their peers. Living with a partner is associated with perceiving oneself as relatively more indebted, but owning a home has no identifiable effect on perceived relative debt levels. When using absolute debt differences however, owning a home becomes a very strong predictor: those who own a home are much more likely to think their peers have a similar debt level as they do compared to those who do not own a home.

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, either in terms of income and debt, as well as individuals who think they are much worse off.

3.2 Peer Comparisons and Perceptions of Equity and the Need for Redistribution

To what extent are perceived income or debt standings related to individuals' views about the fairness of outcomes or the need for redistribution? As described in Section 2, our follow-up survey included a number of questions about the perceived returns to labor, the fairness of economic outcomes, and the need for redistribution. We therefore consider the extent to which such measures might be correlated with individuals' perceived income and debt standings relative to their peers.

Figure 3 plots binscatters relating relative income and debt standings and the extent to which respondents agree or disagree with a variety of statements. Panel A considers the statement "Income differences are too large." We find a strong correlation between both perceived relative income and debt standings and responses to this statement: those who perceive themselves as better off relative to their peers in terms of income tend to disagree more strongly with the statement than do those who perceive themselves as worse off relative to their peers. With relative debt, it is those with higher relative debt levels who disagree more with the statement. Panel B considers an analogous statement regarding wealth differences being too large. We find the same patterns hold with respect to wealth differences, although the correlation with perceived debt levels is weaker. Panel C considers a third statement regarding whether "Incomes should be made more equal." Again, we find those with higher perceived income and debt levels tend to disagree more strongly with the statement on average than those with lower perceived income and debt levels.

In the next three panels, we show correlations of perceived relative income and debt positions with whether individuals believe that income differences incentivize effort (Panel D), whether hard work brings a better life (Panel E), and whether success is a matter of luck rather than work (Panel F). In all three cases, we find strong correlations between relative income levels and beliefs about the returns to work: those who think they earn more than their peers tend to belief that income differences are incentivizing and that income rewards work. Correlations with perceived debt levels are much weaker. Hence, at least unconditionally, there are strong correlations between individuals' perceived relative income differences and views about equality and the role of work effort.

We also asked respondents two questions that reflect more general attitudes. The first is whether it is ever acceptable to cheat on taxes. As shown in Panel G, we find little systematic correlation between perceived income or debt standings and how flexible individuals' views are with respect to following tax laws. Finally, we asked respondents to rate their happiness level on a scale of 1 to 5, with 5 indicating very happy while 1 indicates very unhappy. While there is little correlation between happiness and relative debt levels, we find a strong positive correlation between perceived relative income levels and happiness (Panel H): those who think they earn more than their peers tend to also be more happy than those who

think they earn less than their peers. Of course, this relationship should not necessarily be interpreted as causal since there are many confounding factors. Nonetheless, the strength of the correlation is notable, consistent with other papers that have previously documented positive correlations between relative income and happiness (Ferrer-i-Carbonell 2005, Luttmer 2005).

In the same spirit, our survey included several questions that measured the social and civic engagement of respondents. One such question was ascertaining how often respondents met with friends, neighbors, colleagues or acquaintances outside of work per week. In Panel A of Figure 4, we plot correlations of these answers with participants' perceived relative income and debt levels. In both cases, there is a weak positive correlation, indicating that those who perceive themselves as having either higher income or higher debt than their peers also tend to report more social meetings outside of work. Panel B reports equivalent correlations with another measure of social activities, the frequency at which participants visit sports, social or other clubs. With respect to both perceived relative income and debt levels, we again find a positive correlation with social engagement. Finally, we asked participants about their engagement with political or social organizations. We find no correlation between relative income levels and engagement in these types of organizations (Panel C), but those who perceive themselves as having higher relative debt levels tend to be systematically less involved in these organizations.

These figures suggest some systematic patterns linking perceived income and debt standings of individuals with their attitudes toward work, their overall happiness and their social and civic engagement. However, these correlations, while suggestive, should not be interpreted as causal. To do so, one would need to be able to identify exogenous changes in the perceived income or debt standings of individuals. Because our survey provided information about the levels of peers' income or debt levels to randomly selected participants, this information experiment can therefore help address this identification issue, a question to which we now turn.

4. The Effects of Information Treatments on Perceived Peer Income and Debt Levels

At the heart of our empirical strategy is the information treatment to generate exogenous variation in the perceived relative income and debt standings of respondents. As described in Section 2, we implement this approach by randomly assigning survey participants to either a control group or one of the two general treatment groups. The control group is not provided with any additional information. The two treatment groups are provided either with information about the income level of their peers ("peer income treatment") or about the debt level of their peers ("peer debt 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 average income of Dutch individuals that are within the same age group, that have the same general education level, and that have a similar living situation with a partner. In the case of debt, we provide the average debt level of Dutch individuals with the same characteristics as for income but also the same housing characteristic, i.e., whether they own their home or rent. Within each of the two treatment groups, there are hence many different values or signals being provided about peers' income or debt 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 5. In Panel A, we plot a binscatter of the posterior beliefs of individuals about their peers' income levels against 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:

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

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 Panel A of Figure 5, this is precisely what seems to happen. For a given signal and demographic group, we see that those whose prior beliefs about their peers' income was higher than the signal, they tend to report posteriors that are much closer to the signal than their prior belief. This holds across different levels of the provided signal and for different levels of prior beliefs. The

same pattern holds for the debt treatment, as shown in Panel B. Individuals consistently revise their beliefs toward the provided information about peers' debt levels.

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

$$Posterior_i = b_0 + b_1 \times Prior_i + b_2 \times Prior_i \times \mathbb{I}(i \in Treatment) + b_3 \times \mathbb{I}(i \in Treatment) \times TreatmentSignal_i + error \quad (2)$$

where $\mathbb{I}(i \in Treatment)$ is an indicator variable for being in the treatment group. Posteriors and prior beliefs are either for peers' income or debt levels (in logs). Note that we depart from typical estimates of treatment effects (e.g., Weber et al. 2023) 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

$$Posterior_i = (1 - G) \times Prior_i + G \times Signal_i = Prior_i - G \times Prior_i + G \times Signal_i \quad (3)$$

where $G > 0$ for the treatment group when the signal is at least somewhat informative. If the signal varies across individual, 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 5, 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 treatment groups 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 3 using Huber regressions to control for outliers. Column (1) in Panel A 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 erroring 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 that peers' income was lower than they thought (column 2, "good news") or those who learned that peers' income was higher than they thought (column 3, "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 and therefore that our results are unlikely to be coming from survey demand effects.

Panel B presents equivalent results for beliefs about debt standings, albeit restricting our attention to those who report that they have positive debt levels. We find even stronger effects on beliefs in this case, consistent with Figure 5. Respondents place even more weight on the provided signals about their peers' debt levels than they do when provided with information about peers' income levels. Again, these results hold regardless of whether individuals learn that their peers have more debt than they thought ("good news") or less debt ("bad news")[5] or whether we focus on beliefs in follow-up waves when available.

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.

We can assess whether the peer income treatment affects perceived relative debt standings and whether the peer debt treatment equivalently affects perceived relative income standings. We do so by estimating the same regressions as in Table 3 but including priors and interactions of priors with both treatments simultaneously. We find evidence that both treatments affect both relative income and debt standings, but that the cross-treatment effects are quite small (Appendix Table A6). For example, providing information about peer income affects not just an individual's perceived standing in the income distribution but also how their debt compares to their peers. However, the latter effects are quantitatively small relative to the changes in debt standings explained by information about peers' debt levels. Because of this, we continue to focus on how each treatment affects the corresponding peer standing measure (i.e., debt treatment on perceived debt standing).

Jointly, these results indicate that our information treatments were successful in inducing powerful changes in respondents' perceptions about their peers' income and debt levels. As a result, we are also creating exogenous variation in the *relative* income and debt standings perceived by those respondents.

[5] We define that the news regarding relative debt is good when people learn that they have lower levels of debt than their peers. Correspondingly, the news is bad when people learn that they have higher levels of debt than their peers.

With this 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 it affects their employment, spending decisions and happiness.

5. The Causal Effects of Peer Comparisons

Does an individual's perceived income and debt standings causally affect their beliefs or behavior? In this section, we consider how the exogenous variation in beliefs about individuals' relative income and debt levels created by our information treatments affected a range of beliefs and decisions. We consider five distinct types of outcomes. The first is beliefs about equity and redistribution, following an extensive literature (e.g., Hvidberg et al. 2023). The second involves the amount of social and civic engagement of individuals. The third outcome that we study is employment and expected income. The fourth outcome that we assess is the spending of individuals. Finally, we consider whether happiness is affected by changes in perceived relative income and debt standings.

5.1 The Causal Effects of Peer Comparisons on Beliefs about Equity and Redistribution

With a clear source of exogenous variation in the perceived relative standings of individuals, one can assess the causal effect of peer comparisons on individuals' beliefs and decisions. We begin by considering how perceived peer standings shape the beliefs about the income distribution. To do so, we rely on the following empirical specification, when looking at the effects of relative income standings:

$$Belief_i^{Post} = b_1 \times \log\left(\frac{Own\ Income_i^{post}}{Peer\ Income_i^{post}}\right) + b_2 \times \log\left(\frac{Own\ Income_i^{prior}}{Peer\ Income_i^{prior}}\right) + Controls_i + error \quad (4)$$

where $Belief_i^{Post}$ is the belief about inequality or fairness of respondent i measured in wave 2. We instrument for the posterior belief about an individual's perceived relative income standing using an information treatment indicator variable and its interaction with the individual's prior belief about their relative income standing as well as an interaction of the treatment with the specific signal value, as in section 4.1. We control for a number of observable characteristics (age and age squared, educational attainment, gender, number of household members, number of children, employment status, homeownership status, and regional dummy variables)[6] as well as their prior belief about their relative income standing. Following

[6] We find similar results when we use a more extensive list of controls (e.g., indicator variables for each education and income group as well as their interactions). In Appendix Table A11, we apply the procedure of Romano and Wolf (2016) to adjust p-values for multiple hypothesis testing in Tables 4-8 and find no meaningful difference in results.

Coibion et al. (2023), we use Huber regression in the first stage, then apply a jackknife procedure to control for any remaining outliers in the second stage of the regression.

Panels A and G of Table 4 presents results for whether individuals agree that income differences (Panel A) and wealth differences (Panel G) are too large in the Netherlands.[7] In both cases, we find clear negative causal effects of the perceived relative income standing on beliefs about whether income and wealth differences are too large in the Netherlands. The information treatments serve to generate the exogenous variation in perceived relative income standings, and the F-statistics in both cases are close to 100, indicating that our treatments serve as powerful instruments. We can therefore establish that when individuals believe their own income is higher relative to their peers, they tend to become more accepting of income and wealth differences.

In Panel B of Table 4, we turn to whether relative perceived income standings affect the degree to which people believe that incomes should be made more equal, i.e., if there is need for policy interventions. We again find a strong causal effect: those who exogenously believe their income is higher relative to their peers become less likely to think that incomes should be more equal. The estimated coefficient is large. A one standard deviation increase in relative income leads to a 0.4 standard deviation decrease in the belief that incomes should be made more equal. This finding therefore points to an important causal link between where individuals perceive themselves in the income distribution and their belief in the need for income redistribution. Prior evidence has been mixed. For example, Cruces et al. (2013) find a similar result whereas Hoy and Mager (2021) find the opposite and Fehr et al. (2022) find no link in the context of global inequality.

Why would individuals disagree about the need for income redistribution? In Panels C-E, we consider different mechanisms through which this could arise. For example, in Panel C, we assess whether those who view themselves as having higher income than their peers view higher income as an incentive to work harder and find that indeed they do. However, this need not imply that they disproportionately believe that hard work yields higher incomes. In Panels D and E, we assess the latter through two different questions. Panel D tests whether those with exogenously higher perceived relative income standings tend to agree more with the statement that in the long run, hard work usually brings a better life. We find that we cannot reject the null of zero effect. Similarly, in Panel E, we test whether those with exogenously higher perceived relative income believe that success is due more to luck than hard work, and again we find that we cannot reject the null of zero effect. Thus, while those who view themselves as richer than their peers tend to oppose redistribution more than those who view themselves as poorer than their peers,

[7] We find similar results for subsamples split by various demographic variables; see Appendix Table A9.

it is not because they think that their higher income is a result of working harder than their peers. Rather, they become more motivated by income differences. Our results therefore suggest a more nuanced view of why richer individuals tend to oppose redistribution than just that their greater income is the result of them working harder. Instead, they seem to view income differences as more motivating.

In Panel F, we assess the potential causal effect of relative peer standings on individuals' beliefs about whether it is ever acceptable to break rules, specifically cheat on taxes. If income differences were perceived as somehow unfair, one might expect individuals who learn that they are poorer relative to their peers than they thought before to perhaps think that it is more acceptable to break rules such as tax laws, and vice versa for those who learn that they are richer. We find no robust evidence of such an effect. Those with exogenously higher perceived relative income standings do not seem to systematically think that it is more or less acceptable to cheat on taxes.

Following Hvidberg et al. (2023), we also consider whether results differ for those who receive "good news" (those who learn that their relative peer standing is higher than they thought) and those who receive "bad news" (those who learn that their relative peer standing is lower than what they thought). In columns (2) and (3) of Table 4, we re-estimate the effect of perceived peer income standings on views about inequality and redistribution for each subset of participants, with results for "bad news" in column (2) and "good news" in column (3). Overall, we find little evidence that those receiving "good news" about their peers' income respond differently than those receiving "bad news."

Overall, we find strong causal effects of perceived relative income levels on beliefs about income inequality and redistribution. Exogenous increases in perceived income standing make individuals less concerned about income and wealth inequality and less likely to think something should be done about it. However, this does not come about because people think that their higher relative incomes are a reflection of their hard work but rather because they are more likely to view income differences as incentivizing to work harder. Finally, exogenous changes in relative income standings do not lead to differences in views about whether it is acceptable to break rules such as tax laws. Our results broadly confirm evidence by Cruces et al. (2013) and Hvidberg et al. (2023), but whereas they find that peer effects alter the desire for redistribution only for those who get good news, we find no difference between those who receive either good news or bad news. Furthermore, we can provide novel evidence on why these effects take place in terms of underlying perceptions about why inequality matters.

In addition to being able to assess the effect of relative income on beliefs about equity, a novel feature of our setting is that we can also study whether perceived relative *debt* standings affect individual beliefs

and decisions. We do so by re-estimating the same specifications but now using relative debt standing as the RHS variable and using the debt treatments in the first stage in an equivalent manner. We report results in columns (4)-(6) of Table 4 for different beliefs about equality and redistribution. Overall, we find no detectable causal impact of perceived relative debt standings on any of the reported beliefs about equality or redistribution. This is despite the fact that there were some strong unconditional correlations between beliefs about relative debt levels and inequality documented in Figure 3. We interpret this as indicating that those unconditional correlations were not causal and instead due to other correlated possibly unobserved factors. Moreover, this evidence can be linked to some recent debate about policies such as the prolonged period of ultra-low interest rates and their implications that are often seen as quite asymmetric between savers and borrowers. Our findings suggest that consumers do not associate their relative debt standing with their perceptions about equality in society.

5.2 The Causal Effects of Peer Comparisons on Social and Civic Engagement

Do relative income or debt standings affect the amount of time individuals spend with their peers or the way in which they do so? Because our follow-up surveys included questions on how much time respondents spent with peers, as well as the settings in which they do so, we have an ideal setting to answer this previously unexplored question. To do so, we use the same empirical specification (4) but now with respondents' answers about their time spent with peers as dependent variables. For example, in Panel A of Table 5, columns (1)-(3), we report estimates of the effect of perceived relative income standings on the number of meetings respondents had with neighbors, friends and coworkers outside of work. We find tentative evidence of a positive effect: households who perceive themselves as earning relatively more than their peers tend to increase the amount of time they spend with them. However, there is significant uncertainty around the point estimates and we can only reject the null of no effect at the 10% level. In Panels B and C, we explore two ways in which this greater social time could be occurring. Specifically, we assess whether this increased time comes from going to sports and other community centers more frequently (Panel B) or whether it comes from greater involvement in political and other civic organizations (Panel C). There is suggestive evidence that those who have higher relative income become more engaged in sports and other community centers, but again these estimates are surrounded by significant uncertainty. With respect to spending more time in political or other civic organizations, we find no evidence that changes in perceived relative income levels affect engagement with political organizations. Hence, while we find that relative peer income standings have clear effects on beliefs about

inequality and the need for redistribution, we do not observe any detectable effect on political activism among respondents based on those same relative peer income standings. To the extent that relative income affects time allocation, the effects appear to be limited to time spent socializing with peers. The results are similar for various subsamples (Appendix Table A10).

In columns (4)-(6) of Table 5, we explore equivalent estimates but now focusing on the effects of perceived relative debt standings. We find no effect of relative debt standings on the number of meetings spent with peers outside of work, but we do find a positive effect of these standings on the number of visits to sports or other community centers. This indicates that those who learn that they have relatively more debt than their peers increase the number of outings to sports or other centers. The estimated effect is reversed when we limit our sample to those getting good news about their relative level, but this sub-sample is very small and we are hesitant to place much weight on this single estimate. Within the sub-sample of those getting bad news, the effect is positive but imprecise. Given this ambiguity in results, we do not view the estimates as providing a robust sense of how relative debt affects visits to sports and related events. Finally, we find no clear effect of relative debt on political activities either.

Overall, we interpret our results as pointing to a potential positive causal link between relative income standings and the extent to which individuals are willing to engage with their peers socially, but no such effect for relative debt standings. One interpretation of the result on relative income standings is that it provides a potential channel that can explain why relative income affects happiness: those with higher perceived relative income devote more time to socializing with peers, which can raise happiness.[8]

5.3 The Causal Effects of Peer Comparisons on Employment and Expected Income

In a well-known paper, Card et al. (2012) document that when workers become informed about their coworkers' pay, those who learn that they are paid less than their peers report lower levels of job satisfaction and an increased willingness to search for new jobs. However, effects on actual employment outcomes were ambiguous. Jager et al. (2022) similarly find that when workers are told that others earn more than they thought, their expected job search and wage demands rise. Because respondents report employment outcomes and expected income in follow-up waves, we can assess whether relative income standings affect actual employment outcomes as well as income expectations.

In both initial and follow-up waves, respondents were asked whether they were employed. We therefore estimate the effect of relative income on employment using equation (4) but using as dependent

[8] The extent to which social interactions actually contribute to happiness is unclear, as discussed in a recent meta-analysis (Folk and Dunn 2023).

variable an indicator variable equal to one if the respondent reports being employed in the follow-up wave and zero otherwise. We also control for their employment status in the prior wave using the same indicator variable approach. The results for perceived relative income are presented in Panel A of Table 6.[9] Across all respondents, we find no statistically significant effect of perceived relative income on employment. However, this masks differential responses across those who received "good" news versus "bad" news. For those who learn that their income is below their peers ("bad" news), we find a strong negative relationship between perceived relative income and subsequent employment outcomes. Thus, learning that other households have higher income than expected makes it more likely that respondents will be employed in subsequent months. This is consistent with the increased search behavior documented in Card et al. (2012). The implied magnitude is fairly large: a one-standard deviation increase in relative income lowers the probability of being employed by about 15 percentage points, which only captures movements from non-employment to employment and vice-versa over a two-to-three-month horizon. Also, qualitatively consistent with Card et al. (2012) is the fact that for those receiving good news, there is no subsequent effect on employment.

In addition to their employment status, households in the follow-up wave were asked to predict their net household income for 2020, thereby providing us with a measure of their expected income. We can therefore estimate the effect of peers' incomes on a respondent's expected household income using the following two-stage specification in which the first stage is:

$$\begin{aligned}\log\left(Peer\ Income_i^{posterior}\right) = b_0 + b_1 \times \log\left(Peer\ Income_i^{prior}\right) \\ + b_2 \times \log\left(\ Peer\ Income_i^{prior}\right) \times \mathbb{I}(i \in Treatment) \\ + b_3 \times \mathbb{I}(i \in Treatment) \times \log\left(\ TreatmentSignal_i\right) \\ + controls_i + error_i\end{aligned} \tag{5}$$

and the second stage is given by:

$$\begin{aligned}\log(ExpIncome_i) = c_0 + c_1 \times \log\left(\ Peer\ Income_i^{posterior}\right) \\ + c_2 \times \log\left(\ Peer\ Income_i^{prior}\right) + controls_i + error_i\end{aligned} \tag{6}$$

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. $\log(ExpIncome_i)$ is the expected income for 2020 of household *i*. Unlike in sections 5.1 and 5.2, we do not use perceived relative income of individuals in the regressions but rather the log of perceived peer

[9] When using perceived relative debt standings, our approach to controlling for outliers fails to converge, which is why we do not present results for this outcome.

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

We report results in Panel B of Table 6. As with employment, the effect across all respondents is insignificantly different from zero. However, in the case of households receiving "bad news," we find a positive effect of peers' incomes on expected household income. The estimated pass-through from peers' income into household income is about one-half, but the standard errors are quite large so we cannot estimate this effect with much precision. Nonetheless, this finding is consistent with what we observed for employment effects of peers' incomes.

Our results therefore complement the findings of Card et al. (2012) and Jager et al. (2022). Whereas they can identify the effects of relative income on job satisfaction and search, we identify an effect on ex-post employment and expected incomes outcomes consistent with their results. We also find the same asymmetry as Card et al. (2022): it is those who learn that their peers are doing relatively better than they thought who change their labor supply decisions, whereas those who learn their peers are doing relatively worse do not seem to respond in terms of labor supply. Furthermore, these complementary findings provide external validity to RCT identification of causal effects of peer standings on labor supply decisions.

5.4 The Causal Effects of Peer Comparisons on Spending

We now consider the effect of peers' income and debt levels on individual spending behavior, i.e., testing the strength of "keeping up with the Joneses". This mechanism, albeit typically formulated in terms of peers' consumption rather than income ("external habit formation"), is at the heart of many macroeconomic (e.g., Smets and Wouters 2007) and finance (e.g., Campbell and Cochrane 1999) models. However, to the best of our knowledge, there is no direct causal evidence on the actual extent to which individual spending responds to the spending of others. Bertrand and Morse (2016) study whether rising incomes at the top of the income distribution in the U.S. led those at the middle and the top of the distribution to spend more than they otherwise would have. They argue that this is indeed the case, especially for conspicuous categories of goods (e.g., cars). Agarwal et al. (2020a, 2020b) focus on peer effects of lottery winnings and bankruptcies. Relative to these papers, our environment is unique in that we have both exogenous and measurable variation in peers' perceived income and debt (unlike e.g., Bertrand and Morse 2016 and Agarwal 2020b) and direct spending and borrowing measures (unlike e.g., Agarwal 2020a).

Our measures of spending come from follow-up waves that asked respondents to report their combined monthly spending on non-durables and services, as well as if they had purchased large durable

goods in the previous months and, conditional on a purchase, how much they had spent on those. Respondents were also asked about whether they had taken out any new loans or mortgages in recent months. Hence, we observe the level of spending on non-durables and services up to 3 months after the treatment, their durable goods spending, and the extensive margin of borrowing.

To assess the effect of peers' perceived income and debt on spending and borrowing decisions, we use a similar IV strategy as for expected income, in which the first stage specification is given by:

$$\begin{aligned}\log\left(Peer\ Income_i^{posterior}\right) = b_0 + b_1 \times \log\left(\ Peer\ Income_i^{prior}\right) \\ + b_2 \times \log\left(\ Peer\ Income_i^{prior}\right) \times \mathbb{I}(i \in Treatment) \\ + b_3 \times \mathbb{I}(i \in Treatment) \times \log\left(\ TreatmentSignal_i\right) \\ + controls_i + error_i\end{aligned} \tag{7}$$

and the second stage is given by:

$$\begin{aligned}\log(Spending_i) = c_0 + c_1 \times \log\left(\ Peer\ Income_i^{posterior}\right) \\ + c_2 \times \log\left(\ Peer\ Income_i^{prior}\right) + controls_i + error_i\end{aligned} \tag{8}$$

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. As in section 5.3 for expected income, we do not use perceived relative income or debt of individuals in the regressions but rather the log of perceived peer income. This specification therefore gives us a direct estimate of the elasticity of peers' income into individual spending.

We present the results from this specification in Panel A of Table 7. Across all participants, we find a negative but statistically insignificant effect of perceived peer income on spending. However, when we split this into two sub-samples based on whether treated individuals received good news (their relative income was higher than they thought) or bad news (their relative income was lower than they thought), coefficients on peers' income become significant in each case. This sensitivity likely reflects noise in self-reported spending data. We interpret this result as indicating that when individuals believe they earn less relative to their peers, they tend to decrease their monthly spending on non-durables and purchases.

In Panel B, we consider spending on durable goods, including both those who purchased goods and those who did not, whereas in Panel C we focus on the extensive margin of durable goods purchases, using as a dependent variable an indicator equal to one if they purchased any durable good and equal to zero otherwise. We find that higher perceived income of peers leads to higher spending on durable goods as well as a higher likelihood of purchasing any durable good, but the effects are most clearly identified

for those receiving bad news and otherwise insignificant across all participants. Thus, our results suggest that when participants realize that their peers earn more than they thought, they tend to engage in more durable goods spending (which is often visible to others, e.g., cars) but reduce their spending on non-durables and services (which is typically invisible to others) to compensate. In Panel D, we find no evidence that households take on new loans over this period, indicating that the additional spending on durables is largely offset by the reduction in spending on non-durables and services. Qualitatively, these results are therefore consistent with the notion that "keeping up with the Joneses" induces households to increase their spending on the most visible and conspicuous items, which are generally durables, when their peers get richer, a pattern also observed in Bertrand and Morse (2016) and Agarwal et al. (2020a).

However, the implied net effect on total spending is quite small. To see this, note that evaluated at the median, the increase in durable goods spending corresponds to an extra €13 while the decrease in non-durable spending at the median is €15, so the net effect is a decline of €2 per month. This corresponds to a 0.2% decline in total spending when a respondent learns that their peers earn 1% more than what they thought previously. The short-run pass-through of relative income into total spending over three months therefore appears to be quite low and of the opposite sign compared to predictions of "keeping up with Joneses" models in macroeconomic and finance. For example, macroeconomic estimates of external habit formation typically involve a pass-through of the consumption of others (which should be closely tied to income) into overall spending of 60-90% (see e.g., Smets and Wouters 2007 and Havranek et al. 2017). We also do not find any pass-through into borrowing decisions (Panel D). When looking at the effects of relative debt standings for spending patterns (Panel B of Table 7), we again do not find evidence of any pass-through coming from perceived relative debt positions into any spending or borrowing decisions. Therefore, our combined results suggest that while *reallocative* "keeping up with the Joneses" effects on spending likely exist, at least for some agents or for some relative income changes, their economic implications in terms of aggregate spending appear to be small.

One interpretation of these results is to cast doubt on the use of external habit formation in specifying utility functions in macroeconomics and finance as well as on prior results that have relied on this specification. In macroeconomics, for example, consumption inertia can be induced through internal habit formation or rule-of-thumb households as in Christiano, Eichenbaum and Evans (2005) or Campbell and Mankiw (1989) respectively. In finance, allowing for rare disasters (e.g., Barro 1989) or Epstein-Zinn preferences (Bansal and Yaron 2004) can address some of the same issues as "keeping up with Joneses" preferences. Another interpretation of our results could be that they point to a small short-run pass-through

of peer incomes into consumption but not necessarily a small long-run pass-through. If the external habit stock evolves only gradually to changes in peers' income or consumption, as in Ravn, Schmitt-Grohe and Uribe (2006), then the short-run pass-through may be low, as we observe, while the long-run pass-through may still be high.

Last, we find no significant role for relative debt standing on spending. While earlier research has found that relative income standing incentivizes not only more spending but also higher borrowing (Georgarakos et al. 2014 and Agarwal et al. 2020) we do not find evidence that learning about peers' debt per se affects own spending directly. This is the case both for those who learn that their peers have more debt than they had originally believed (which could have encouraged more own borrowing and more spending) and those who learn that peers' debt is below their priors (which could have triggered less spending to repay own debts).

5.5 The Causal Effects of Peer Comparisons on Happiness

Do perceived relative income standings matter for reported levels of happiness? Earlier work has frequently found positive correlations between the two (e.g., Stevenson and Wolfers 2008), as we did in Figure 3. Through our information treatment, we can now assess this in a causal framework. We do so by re-estimating specification (4) but now using self-reported happiness as the outcome variable. The results, presented in Panel A of Table 8, indicate a strong causal effect of perceived relative income on happiness. The coefficient is large: a one standard deviation increase in perceived relative income leads to a 0.4 standard deviation increase in happiness. For comparison, the OLS estimate is reported in column (1) and indicates a much weaker effect. In Appendix Tables A7 and A8, we show that the effect of relative income on happiness appears to be primarily driven by those receiving "good news" about their relative income. The effect for those in the "bad news" group is close to zero and statistically insignificant whereas the one in the "good news" group is more than twice as high as the average estimated effect. Note that happiness is measured in the follow-up wave two months (wave II) after the information treatment, which suggests that our information treatments have a persistent effect on happiness as well as relative perceived income levels.

In Panel B of Table 8, we report the effect of perceived relative debt levels on reported happiness. We cannot reject the null of no effect, and the coefficient is quite small. In Appendix Tables A7 and A8, we also do not find any effect when we restrict our attention to the "bad news" or "good news" subgroups. Hence, this suggests that relative debt levels do not have any meaningful causal effect on happiness.

To the best of our knowledge, this is the first clear causal evidence of how perceived relative economic standings affect happiness. Our results contribute to a long literature that has considered how

income and relative income matter for happiness. While we cannot speak separately for the importance of each of them (since we do not have exogenous variation in individual income), we find clear evidence that the relative income channel is important for happiness, both statistically and economically.

Why does happiness increase with perceived relative income? One channel is of course that income relative to peers matters directly for happiness, as suggested in e.g., Clark et al. (2008). But other channels could be at work as well, especially since happiness is measured several months after the treatment. For example, if higher relative income induces individuals to view less need for redistribution (as found in Table 4), then this more general satisfaction with the status quo could be what raises individuals' happiness. Or, if higher relative income leads individuals to spend some more time with peers (as tentatively found in Table 5) and more social time raises happiness levels, this could be another mechanism at work. Changes in spending following from changes in perceived relative income could also drive changes in happiness.

To assess the potential importance of these different channels, we report in columns (3)-(6) of Table 8 augmented specifications of column (2) in which we control for ex-post changes in each of these factors in turn. For example, column (3) reports results from the same specification as in column (2) but controlling for ex-post measures of whether people thought income inequality was too large. We find no meaningful change in the coefficient on peer income in this case, indicating that the effect on happiness is not likely operating through changing beliefs about inequality. In column (4), we control for time spent with peers. In this case, the coefficient on happiness declines by around ten percent, indicating that the additional time spent with peers likely accounts for some of the increased happiness associated with higher relative income, but much of the effect is nonetheless unexplained. Controlling for ex-post spending levels also reduces the coefficient on peer income somewhat. When we control for all three channels in column (6), we continue to find that much of the effect of relative income on happiness remains. Hence, while we cannot rule out other channels, our evidence supports the notion that relative income matters directly for the level of happiness of individuals.

In contrast to the role of relative income standing, we find no evidence that relative debt standing affects happiness. Existing literature in psychology and economics has emphasized the strong links between indebtedness (and especially delayed payments) with mental health and depression (see, e.g., Gathergood 2012 and Richardson et al. 2013 for a large-scale meta-analysis of studies in psychology). While borrowing is a factor that impacts subjective well-being, our findings suggest that a comparison of own debt to that of peers cannot influence happiness per se. This suggests that households view own debt

as a personal obligation that they have to meet independently of the debt burden of their peers, thus their relative debt standing does not affect their psychological outlook.

6. Conclusion

How important are peer effects? One interpretation of our findings with respect to spending is that peer effects are not very important, certainly not as important as is commonly assumed in macroeconomics and finance. Indeed, while peers' perceived income levels affect spending, the quantitative net effects are small. But this does not imply that peer effects do not matter overall. Indeed, we find quite strong effects on overall happiness, as well as on beliefs about redistribution, employment and expected household income, and, more tentatively, on time spent socializing with peers.

Reconciling the absence of strong peer effects on consumption with the strength of peer effects on happiness requires different formulations of utility than what is commonly assumed in "keeping up with the Joneses". In the latter, utility depends on consumption of the agent relative to consumption of others. Such a specification implies that strong peer effects on happiness would necessarily entail strong effects on consumption as well, at least in the long run. Our results instead indicate that peer effects on happiness can be better captured by relative incomes than relative consumption, at least in the short run. As a result, explanations for asset pricing puzzles that rely on rare disasters (e.g., Barro 2009, Bai et al. 2019) or internal habit formation (Parker and Julliard 2005) are more in line with our results than those relying on external habits or "keeping up with the Joneses" formulations of utility, except for specifications in which habits evolve very gradually over time.

More generally, the large causal effect of relative income for happiness that we identify paints a somewhat dismal outlook for society. In a world where not everyone can be above average, these types of relative income effects imply that some of the population will always be more unhappy than is warranted by the level of their income. One possible caveat from our experiment is the existence of an asymmetry: those who received good news about their relative income became happier while those who received bad news did not, at least on average. Since the average individual in our survey was overly pessimistic about their relative income, our results suggest that better information about peers' incomes could at least weakly increase happiness across the board.

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Table 1: Descriptive Statistics

	Mean	Percentiles			St.dev
		P25	P50	P75	
	(1)	(2)	(2)	(3)	(4)
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 durables 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
Peer estimated total debt, '000€	116.29	13.83	100.00	175.00	153.26
Share of respondents not being able to report peer estimated income	0.22				
Share of respondents not being able to report peer estimated debt	0.32				

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?". "Peer estimated total debt" is based on the following question: "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." Moments for "Spending on durable goods in the last month, €" are conditional on observing a positive spending.

Table 2: Determinants of Perceived Standing Relative to Peers

	$100 \times \log(own/peer)$		$100 \times \lvert\log(own/peer)\rvert$	
	Income	Debt	Income	Debt
	(1)	(2)	(3)	(4)
female	-6.77***	-5.40*	1.14*	-1.81
	(0.99)	(3.16)	(0.68)	(2.00)
age	-0.19	-1.52**	0.18	0.85*
	(0.23)	(0.76)	(0.15)	(0.48)
age^2/100	0.20	0.93	-0.17	-0.28
	(0.21)	(0.75)	(0.14)	(0.48)
Education (omitted: Primary education)				
Prevocational secondary	0.46	-14.15	-1.03	-4.46
	(3.32)	(12.22)	(2.21)	(9.04)
Senior general secondary/pre-university	-3.19	-22.33*	-1.56	10.79
	(3.68)	(13.28)	(2.47)	(9.53)
Senior secondary vocational	-2.72	-37.49***	-2.34	11.98
	(3.45)	(12.67)	(2.33)	(9.28)
Higher vocational	0.53	-26.77**	-1.61	5.72
	(3.44)	(12.58)	(2.30)	(9.17)
University	-1.67	-18.97	0.20	5.15
	(3.93)	(13.83)	(2.61)	(9.83)
Employed	12.82***	-7.30	-4.88***	-4.53
	(1.34)	(4.47)	(0.91)	(2.80)
HH head lives with a partner	22.04***	19.30***	-10.59***	-4.28
	(2.37)	(7.07)	(1.60)	(4.86)
Own home	10.17***	-8.72	-1.72**	-44.39***
	(1.25)	(7.98)	(0.86)	(5.48)
# of HH members	-1.65	-6.79	-0.12	-0.10
	(2.02)	(5.86)	(1.34)	(4.15)
# of children	0.79	12.44**	0.42	-2.69
	(2.03)	(5.92)	(1.34)	(4.19)
Social class (omitted: high=1)				
2	-7.86***	-7.51	-2.72**	-4.63
	(1.70)	(5.44)	(1.20)	(3.37)
3	-9.78***	-4.82	-2.80**	-0.78
	(1.81)	(5.97)	(1.26)	(3.66)
4	-14.31***	-18.63**	-2.20	4.21
	(2.17)	(7.31)	(1.48)	(4.53)
5 (low)	-13.59***	-192.79***	-7.04**	98.75***
	(4.40)	(30.88)	(3.35)	(25.91)
Region (omitted: Three major cities)				
Rest west	-3.66**	-11.25*	-2.36**	-4.97
	(1.63)	(5.98)	(1.12)	(3.66)
North	-3.41*	-21.51***	-0.41	-1.16
	(2.00)	(7.02)	(1.35)	(4.34)
East	-3.92**	-4.61	-2.45**	-12.67***
	(1.70)	(6.14)	(1.16)	(3.73)
South	-1.96	-7.57	-1.27	-6.29*
	(1.70)	(6.12)	(1.15)	(3.68)
Observations	2,436	1,203	2,377	1,175
R-squared	0.27	0.10	0.17	0.20

Notes: the table report results of regressing perceive relative income (columns (1) and (3)) and debt (columns (2) and (4)) on observable household and individual characteristics. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 3: Effects of Treatments on Perceived Relative Peer Standing

VARIABLES	Wave 1B			Wave 2	Wave 3
	All	Bad news	Good news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer income					
$Prior_i$	0.812***	0.813***	0.858***	0.408***	0.784***
	(0.015)	(0.021)	(0.028)	(0.018)	(0.030)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.268***	-0.229***	-0.322***	-0.298***	-0.620***
	(0.020)	(0.037)	(0.045)	(0.021)	(0.036)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.271***	0.233***	0.321***	0.301***	0.623***
	(0.020)	(0.038)	(0.044)	(0.021)	(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
Panel B. Peer debt					
$Prior_i$	0.969***	0.960***	1.082***	0.685***	
	(0.022)	(0.039)	(0.035)	(0.043)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.751***	-0.811***	-0.650***	-0.341***	
	(0.036)	(0.073)	(0.067)	(0.057)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.776***	0.799***	0.678***	0.358***	
	(0.037)	(0.069)	(0.070)	(0.059)	
Observations	854	211	635	801	
R-squared	0.762	0.776	0.770	0.305	
F-stat	222.4	74.17	47.58	18.27	

Notes: the table reports estimation results for specification (2). Column (3) reports estimates for the subsample where the income (debt) reported in the information treatment is greater than the prior peer estimated income (debt). Column (2) reports estimates for the subsample where the income (debt) reported in the information treatment is smaller than the prior peer estimated income (debt). Columns (1), (4) and (5) are estimated on full samples. All regressions are estimated with Huber robust method. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 4: Effects of Perceived Standing Relative to Peers on Beliefs about Equity and Redistribution

Dep. Var. title of the panel To what extend do you agree with the statement	Posterior for relative income or debt					
	log (HH income/Peer HH income)			log (HH debt/Peer HH debt)		
	Full	Bad news	Good news	Full	Good news	Bad news
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Differences in incomes in the Netherlands are too large.						
Posterior about relative income or debt	-2.15***	-1.33	-2.36	-0.35	-0.15	-0.23
	(0.66)	(1.07)	(1.96)	(0.21)	(0.21)	(0.97)
	[-3.5, -0.9]	[-3.6, 1.0]	[-7.1,2.1]	[-0.9, 0.1]	[-0.6, 0.3]	[-4.0, 6.1]
Observations	1,119	448	607	434	87	330
1st stage F-stat	91.37	27.56	9.75	55.71	37.77	4.599
Panel B. Incomes should be made more equal.						
Posterior about relative income or debt	-1.80**	-3.16**	1.04	-0.32	0.04	0.25
	(0.71)	(1.29)	(2.08)	(0.20)	(0.32)	(0.98)
	[-3.1, -0.4]	[-6.1, -0.6]	[-3.3, 6.0]	[-0.8, 0.1]	[-0.6, 0.8]	[-∞,∞]
Observations	1,115	448	611	438	80	332
1st stage F-stat	95.29	31.34	11.50	73.99	21.20	4.217
Panel C. Differences in income are an incentive for individual effort.						
Posterior about relative income or debt	1.60***	1.37	2.80*	0.27	-0.77**	0.67
	(0.60)	(0.99)	(1.65)	(0.22)	(0.36)	(0.82)
	[0.5, 2.8]	[-0.5, 3.6]	[-0.6, 6.8]	[-0.2, 0.8]	[-3.4, 0.1]	[-∞,∞]
Observations	1,117	444	616	438	83	323
1st stage F-stat	112.4	38.09	15.96	54.48	16.08	6.835
Panel D. In the long run, hard work usually brings a better life.						
Posterior about relative income or debt	0.73	0.59	-4.75*	-0.01	-0.17	-0.65
	(0.68)	(0.93)	(2.65)	(0.22)	(0.25)	(1.21)
	[-0.7, 2.0]	[-1.4, 2.5]	[-12.2,0.5]	[-0.6, 0.4]	[-∞,∞]	[-∞,∞]
Observations	1,115	446	610	432	88	318
1st stage F-stat	91.08	35.61	7.6	54.48	31.63	2.382
Panel E. Hard work does not generally bring success – it is more a matter of luck and connections.						
Posterior about relative income or debt	-0.19	1.97*	3.34*	0.03	0.08	-1.21
	(0.69)	(1.10)	(1.94)	(0.20)	(0.19)	(1.18)
	[-1.6, 1.2]	[-0.2, 4.6]	[-0.8,7.8]	[-0.4, 0.5]	[-0.2, 4.2]	[-∞,1.652]
Observations	1,115	447	608	433	79	332
1st stage F-stat	97.66	27.98	13.62	57.87	33.54	4.063
Panel F. Cheating on taxes if you have a chance can never be justified.						
Posterior about relative income or debt	0.95	-2.30*	2.35	-0.24	0.62	1.22
	(0.76)	(1.32)	(2.20)	(0.27)	(0.50)	(1.91)
	[-0.6, 2.5]	[-5.4, 0.3]	[-1.9,8.2]	[-1.0, 0.3]	[-0.6, 2.0]	[-∞,∞]
Observations	1,115	441	611	435	88	335
1st stage F-stat	114.4	32.0	15.18	50.78	18.21	1.999
Panel G. Differences in wealth in the Netherlands are too large.						
Posterior about relative income or debt	-1.61**	-1.90*	0.95	-0.36	0.80	-2.22*
	(0.64)	(1.03)	(1.67)	(0.24)	(0.51)	(1.25)
	[-2.9, -0.4]	[-4.3, 0.2]	[-2.6,4.7]	[-1.1, 0.1]	[-0.6, 0.3]	[-∞, 0.1]
Observations	1,111	446	611	439	79	331
1st stage F-stat	100.8	30.18	17.22	53.44	5.913	3.861

Notes: The table reports estimated b_1 in specification (4) where posterior beliefs about relative income or debt positions are instrumented as in specification (2). Panel titles indicate which measure is used as an outcome variable. Columns (1)-(3) use relative income as the regressor of interest. Columns (4)-(6) use relative debt as the regressor of interest. Columns (1) and (4) reports estimates for the full sample. Columns (2) and (5) reports estimates for the subsample where the income/debt reported in the information treatment is greater than the prior peer estimated income/debt. Columns (3) and (6) reports estimates for the subsample where the income/debt reported in the information treatment is smaller than the prior peer estimated income/debt. 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.

Table 5: Effects of Perceived Standing Relative to Peers on Socialization

Dep. Var. title of the panel "How often in the last month have you…"	Posterior for relative income or debt					
	log (HH income/Peer HH income)			log (HH debt/Peer HH debt)		
	Full	Bad news	Good news	Full	Good news	Bad news
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Meetings with friends, neighbors, acquaintances or colleagues outside working hours.						
Posterior about relative income or debt	6.52*	0.29	8.59	0.06	0.34	2.65
	(3.78)	(2.80)	(7.82)	(0.26)	(0.67)	(3.42)
	[-0.5,16.7]	[-5.8,7.5]	[-∞,∞]	[-0.5,0.5]	[-0.9,2.8]	[-∞,∞]
Observations	1,116	459	601	441	81	317
1st stage F-stat	11.16	9.408	2.654	89.68	22.92	1.370
Panel B. Visits of a sports club, community center or other club.						
Posterior about relative income or debt	4.39	-3.05	8.20*	1.35***	-0.93**	3.58
	(3.64)	(3.09)	(4.92)	(0.48)	(0.45)	(4.98)
	[-2.5, 13.7]	[-13.0, 4.4]	[-1.4, 24.2]	[0.5, 2.6]	[-1.9, 0.2]	[-∞,∞]
Observations	1,124	451	603	439	82	324
1st stage F-stat	12.89	6.437	6.988	54.11	62.82	1.094
Panel C. Participation in a political or social organization/activities.						
Posterior about relative income or debt	1.00	0.33	0.64	0.02	-0.14	1.93
	(0.99)	(0.97)	(1.95)	(0.13)	(0.16)	(1.26)
	[-1.0, 3.3]	[-1.6, 2.8]	[-13, 11]	[-0.3, 0.3]	[-3.5, 0.3]	[-∞,∞]
Observations	1,129	465	609	438	82	336
1st stage F-stat	13.95	13.26	4.244	67.37	47.74	1.421

Notes: The table reports estimated b_1 in specification (4) for various measures of socialization where posterior beliefs about relative income or debt positions are instrumented as in specification (2). Panel titles indicate which measure is used as an outcome variable. Columns (1)-(3) use relative income as the regressor of interest. Columns (4)-(6) use relative debt 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 code as 0 ("0 times"), 1.5 ("1-2 times"), 3 ("3-4 times"), 7 ("5-9 times"), 12.5("10 times or more"). Columns (1) and (4) reports estimates for the full sample. Columns (2) and (5) reports estimates for the subsample where the income/debt reported in the information treatment is greater than the prior peer estimated income/debt. Columns (3) and (6) reports estimates for the subsample where the income/debt reported in the information treatment is smaller than the prior peer estimated income/debt. 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 6: Effects of Perceived Standing Relative to Peers on Employment and Expected Income

Panel A:	Employment status		
	Full	Bad news	Good news
	(1)	(2)	(3)
Log (HH income/Peer HH income)	0.04	-0.35***	0.18
	(0.08)	(0.11)	(0.27)
	[-0.05, 0.2]	[-0.6, -0.2]	[-0.6,0.8]
Observations	1,236	500	677
1st stage F-stat	119	54.99	11.94
Panel B:	Log (HH income)		
	Full	Bad news	Good news
	(1)	(2)	(3)
Log (Peer HH income)	0.48	0.57*	0.68
	(0.30)	(0.31)	(0.50)
	[-0.02, 1.2]	[-0.1,1.5]	[-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 (4) 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 (6) 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.

Table 7: Effects of Perceived Peer Income and Debt Standing on Spending Decisions

Dep. Var. title of the panel	Peer estimated income			Peer estimated debt		
	Full	Bad news	Good news	Full	Good news	Bad news
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Spending on non-durable goods, log(spending).						
Posterior about peer income or debt	-1.86	-2.32**	-3.76*	0.01	0.10	0.15
	(1.31)	(1.01)	(2.00)	(0.17)	(0.31)	(0.19)
	[-5.3, 0.8]	[-7.5, -0.6]	[-19.7,-0.6]	[-0.3, 0.3]	[-0.6, 11.4]	[-0.1, 0.5]
Observations	2,965	1,225	1,632	1,934	513	1,392
1st stage F-stat	13.14	12.33	5.621	36.32	2.986	26.64
Panel B. Spending on durable goods, log(spending).						
Posterior about peer income or debt	2.77	6.88***	-4.26	-0.18	-0.25	-0.16
	(1.92)	(2.52)	(3.33)	(0.24)	(0.46)	(0.28)
	[-1.2, 7.5]	[3.6, 15.4]	[-27.4, 6.2]	[-0.6, 0.4]	[-2.6, 12.6]	[-0.7, 0.5]
Observations	3,150	1,305	1,723	2,036	523	1,508
1st stage F-stat	14.07	8.500	3.640	31.07	3.671	21.21
Panel C. Spending on durable goods, extensive margin, linear probability model.						
Posterior about peer income or debt	0.36	1.29***	-1.40*	-0.01	-0.05	-0.00
	(0.36)	(0.48)	(0.77)	(0.04)	(0.08)	(0.05)
	[-0.4, 1.2]	[0.7, 3.1]	[-18.3, 0.2]	[-0.1, 0.1]	[-∞,∞]	[-0.1, 0.1]
Observations	3,263	1,354	1,782	2,140	539	1,566
1st stage F-stat	13.47	7.990	2.960	29.97	3.493	21.89
Panel D. Take out a new loan or mortgage, linear probability model.						
Posterior about peer income or debt	-0.10	0.02	0.25	0.00	-0.07	0.01
	(0.25)	(0.24)	(0.24)	(0.04)	(0.09)	(0.03)
	[-0.6, 0.3]	[-0.5, 0.5]	[-0.14,1.1]	[-∞,∞]	[-∞,∞]	[-∞,∞]
Observations	1,142	472	629	757	193	560
1st stage F-stat	12.64	7.936	6.946	32.72	2.768	31.14

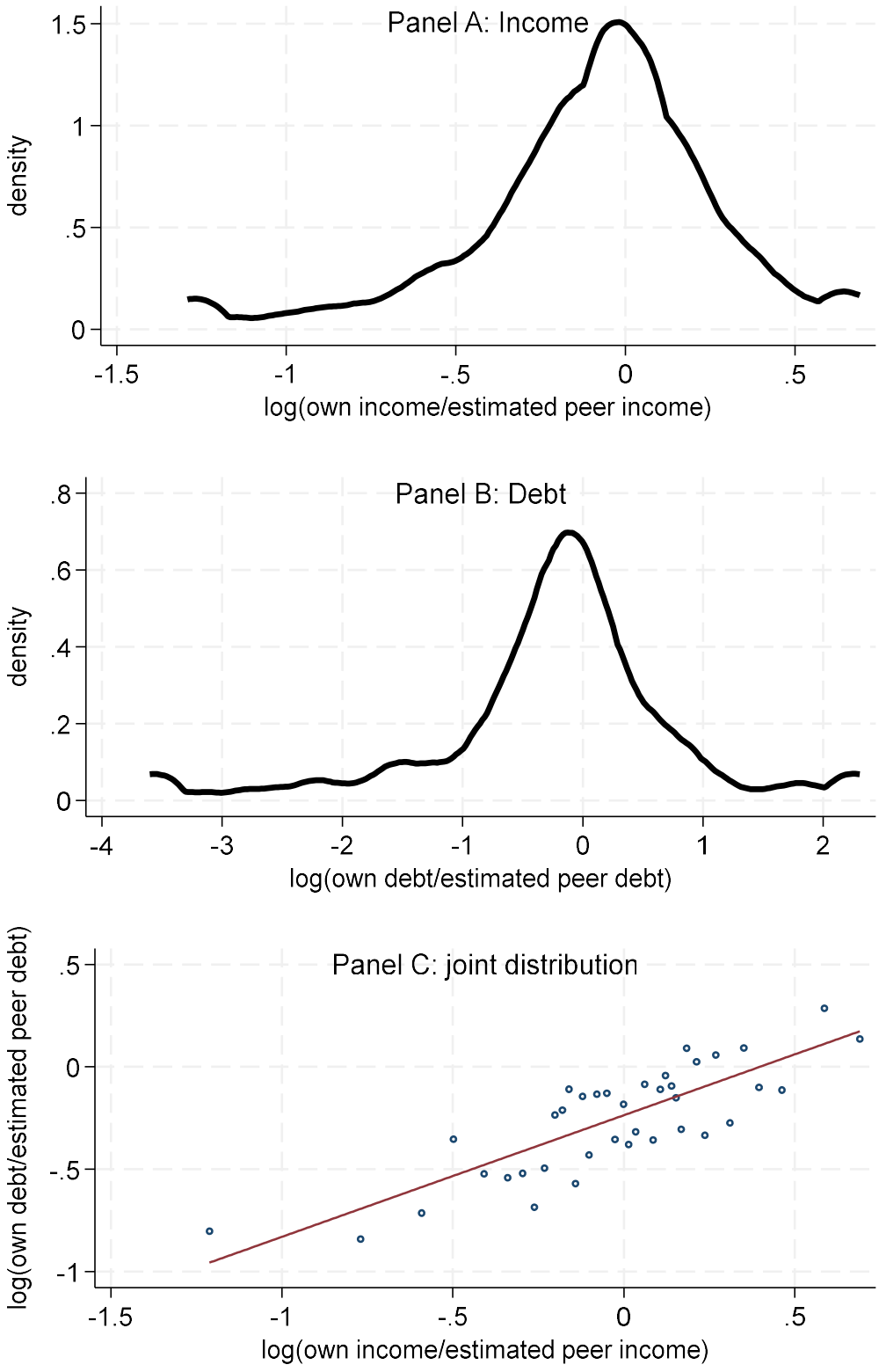
Notes: The table reports estimated coefficient c_1 in specification (8) pooled across all follow-up waves for various measures of consumer spending and loan taking. Columns (1)-(3) use peer income as the regressor of interest. Columns (4)-(6) use peer debt as the regressor of interest. The posteriors are instrumented as in equation (7). Panel titles indicate which measure is used as an outcome variable. Columns (1) and (4) report estimates for the full sample. Columns (2) and (5) report estimates for the subsample where the income/debt reported in the information treatment is greater than the prior peer estimated income/debt. Columns (3) and (6) report estimates for the subsample where the income/debt reported in the information treatment is lower than the prior peer estimated income/debt. 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.

Table 8: Effects of Perceived Peer Income and Debt Standing on Happiness

	Dependent variable: Happiness					
	OLS	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Peer income						
Posterior log (HH income/Peer HH income)	0.21***	0.60***	0.65***	0.53***	0.55***	0.55***
	(0.05)	(0.20)	(0.20)	(0.19)	(0.20)	(0.20)
Income inequality in the Netherlands is too large			-0.01			-0.01
			(0.01)			(0.01)
Time spent with friends/colleagues/neighbors				0.02***		0.02***
				(0.00)		(0.01)
Spending on non-durables goods, log(spending)					-0.01	-0.01
					(0.01)	(0.01)
Spending on durables goods, log(spending)					0.01*	0.01*
					(0.01)	(0.01)
Observations	1,101	1,111	1,111	1,110	994	995
R^2	0.09	0.07	0.07	0.08	0.08	0.10
Panel B. Peer debt						
Posterior log (HH debt/Peer HH debt)	0.03	0.06	0.06	0.06	0.07	0.07
	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)
Income inequality in the Netherlands is too large			-0.01			-0.01
			(0.01)			(0.01)
Time spent with friends/colleagues/neighbors				0.02***		0.02***
				(0.01)		(0.01)
Spending on non-durables goods, log(spending)					0.01	-0.00
					(0.02)	(0.02)
Spending on durables goods, log(spending)					0.00	-0.00
					(0.01)	(0.01)
Observations	437	438	438	436	398	399
R^2	0.06	0.04	0.04	0.07	0.05	0.07

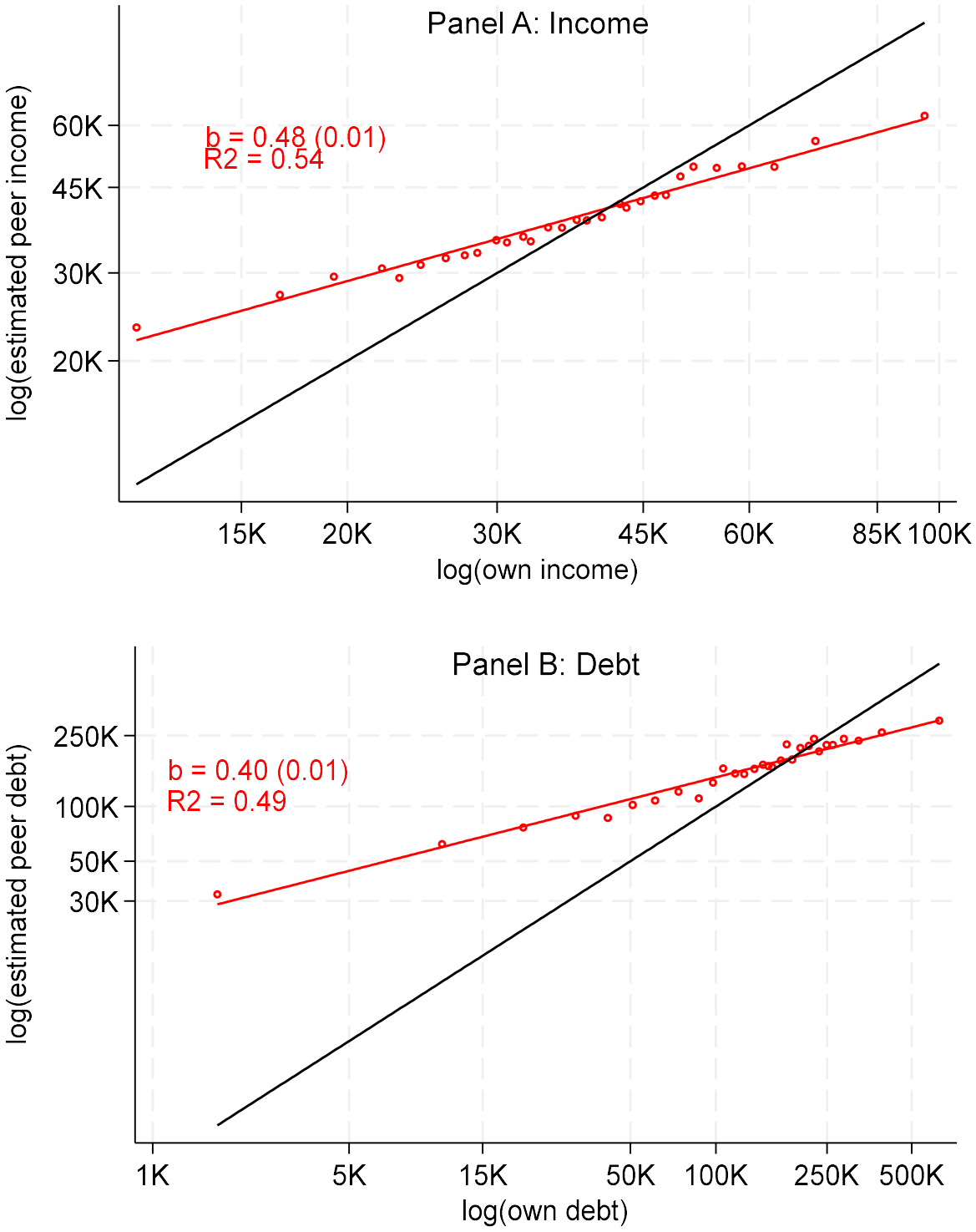
Notes: The table reports estimated coefficient b_1 in specification (4). All IV specifications are estimated using instrumental variables approach where posteriors are instrumented as in equation (2). 1st stage F-stat report the F-test for the coefficients on instrumental variables being equal to zero. 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.

Figure 1: Distributions of Perceived Standings Relative to Peers



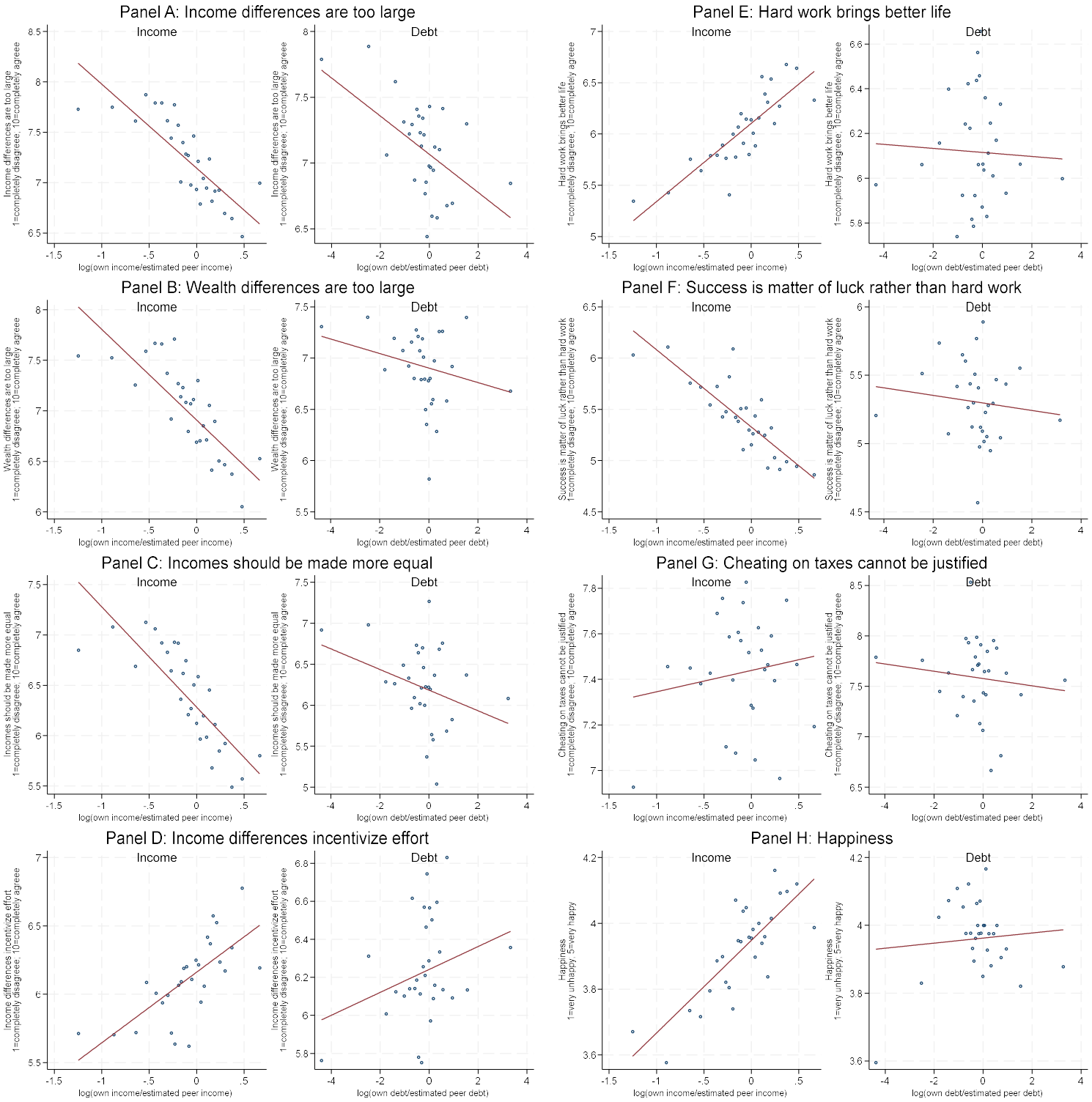
Notes: Panels A and B plot kernel densities for perceived relative income and debt (priors). Panel C shows the binscatter plot of relative income vs relative debt, both variables are measured before treatments (priors).

Figure 2: Perceived Peer Income and Debt vs. Own Income and Debt



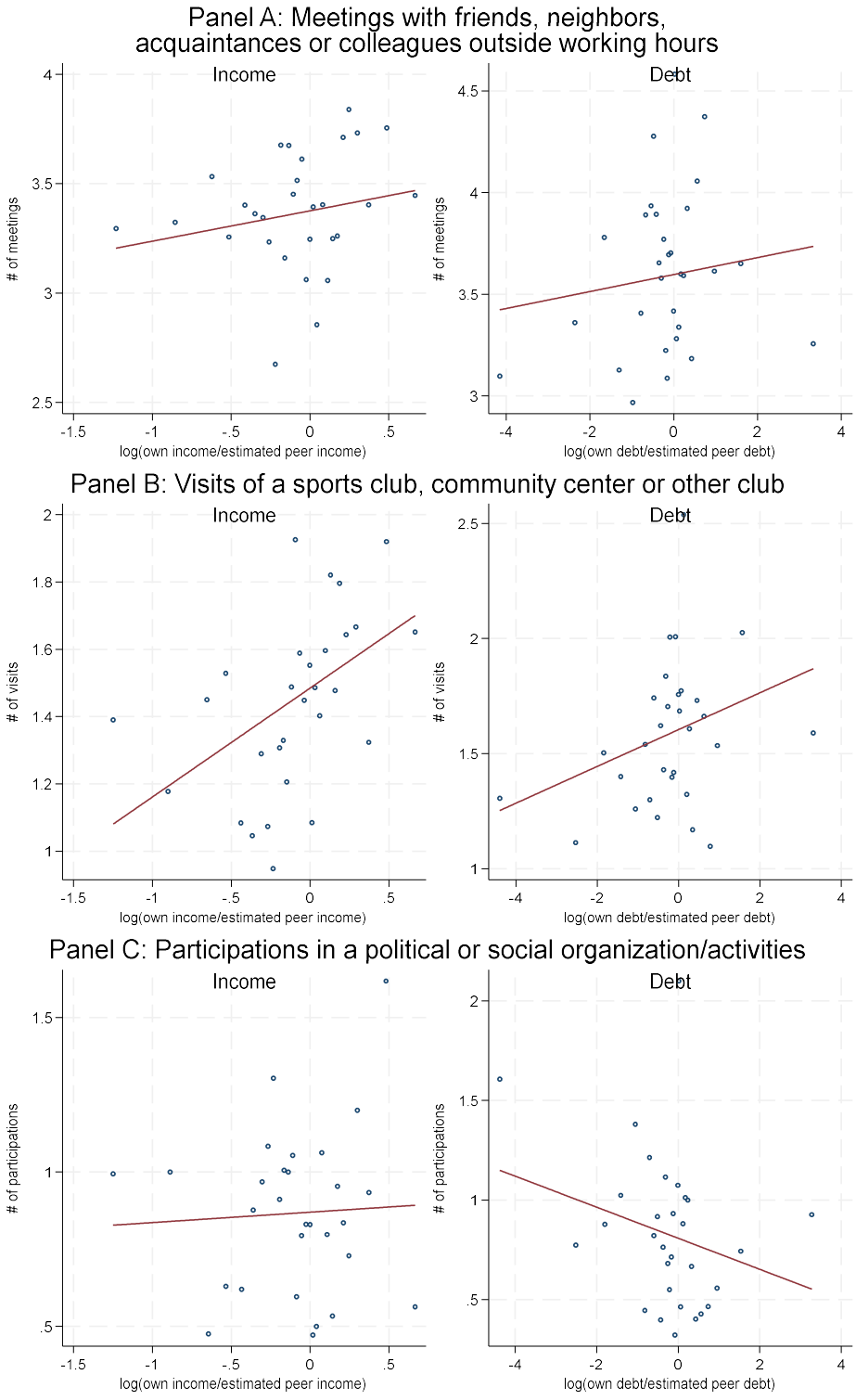
Notes: The figures show binscatter plots of own income (debt) vs perceived peer income (debt). All variables are measured before treatments (priors). The solid black line is the 45°-degree line.

Figure 3: Correlations of Perceived Standings Relative to Peers and Views on Equity and Redistribution



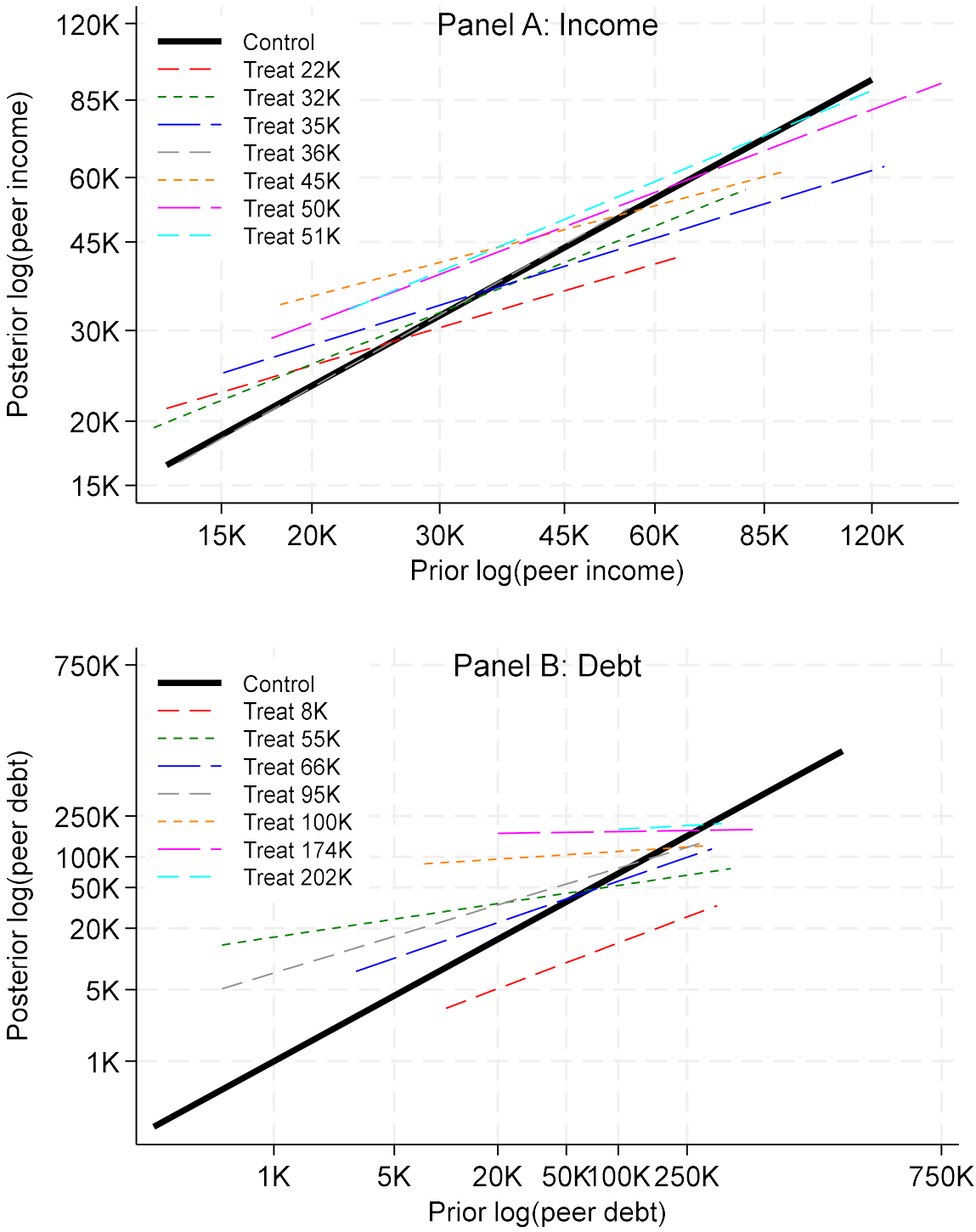
Notes: the figures show binscatter plots of various beliefs vs relative income or debt.

Figure 4: Correlation of Perceived Standings Relative to Peers and Socialization



Notes: the figures show binscatter plots of respondents' social and civic engagement vs relative income or debt.

Figure 5: Effects of Information Treatments on Perceived Income and Debt of Peers



Notes: the figures show fitted linear relationships between posterior and prior beliefs about peer income or debt for the control group (solid, thick, black line) and selected treatment groups.

ONLINE APPENDIX

Appendix A: Additional Tables and Figures

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

Female	Wave 1B			Wave 2	Wave 3
VARIABLES	All	"good" news	"bad" news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer income					
$Prior_i$	0.807***	0.802***	0.860***	0.372***	0.819***
	(0.022)	(0.030)	(0.042)	(0.023)	(0.041)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.250***	-0.199***	-0.299***	-0.311***	-0.663***
	(0.027)	(0.047)	(0.060)	(0.027)	(0.050)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.254***	0.203***	0.300***	0.314***	0.666***
	(0.027)	(0.048)	(0.059)	(0.027)	(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
Panel B. Peer debt					
$Prior_i$	0.993***	1.046***	1.108***	0.707***	
	(0.029)	(0.044)	(0.052)	(0.059)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.767***	-0.958***	-0.576***	-0.353***	
	(0.054)	(0.085)	(0.101)	(0.090)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.796***	0.942***	0.601***	0.375***	
	(0.055)	(0.082)	(0.105)	(0.093)	
Observations	377	86	291	353	
R-squared	0.791	0.860	0.783	0.351	
F-stat	108.7	68.51	16.64	8.459	
Male	Wave 1B			Wave 2	Wave 3
VARIABLES	All	"good" news	"bad" news	All	All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer income					
$Prior$	0.818***	0.827***	0.855***	0.448***	0.747***
	(0.021)	(0.028)	(0.038)	(0.025)	(0.040)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.291***	-0.265***	-0.379***	-0.294***	-0.579***
	(0.029)	(0.055)	(0.067)	(0.032)	(0.051)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.294***	0.269***	0.376***	0.296***	0.581***
	(0.029)	(0.056)	(0.066)	(0.033)	(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
Panel B. Peer debt					
$Prior$	0.945***	0.860***	1.056***	0.665***	
	(0.031)	(0.057)	(0.047)	(0.059)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.736***	-0.659***	-0.720***	-0.330***	
	(0.048)	(0.110)	(0.086)	(0.075)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.757***	0.654***	0.751***	0.343***	
	(0.049)	(0.105)	(0.089)	(0.077)	
Observations	477	125	344	448	
R-squared	0.735	0.683	0.759	0.270	
F-stat	118.2	23.47	35.24	9.850	

Notes: See notes to Table 3. 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 All	Wave 1B "good" news	Wave 1B "bad" news	Wave 2 All	Wave 3 All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer income					
$Prior_i$	0.812***	0.792***	0.880***	0.417***	0.798***
	(0.027)	(0.033)	(0.050)	(0.031)	(0.046)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.271***	-0.263***	-0.353***	-0.270***	-0.621***
	(0.033)	(0.058)	(0.094)	(0.036)	(0.052)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.276***	0.269***	0.353***	0.276***	0.629***
	(0.033)	(0.060)	(0.092)	(0.036)	(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
Panel B. Peer debt					
$Prior_i$	0.963***	1.038***	1.051***	0.574***	
	(0.033)	(0.054)	(0.067)	(0.055)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.775***	-0.938***	-0.672***	-0.288***	
	(0.052)	(0.090)	(0.098)	(0.087)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.800***	0.926***	0.700***	0.301***	
	(0.053)	(0.088)	(0.100)	(0.088)	
Observations	302	80	221	278	
R-squared	0.798	0.881	0.757	0.248	
F-stat	118.3	56.09	25.57	5.876	
Age>50 VARIABLES	Wave 1B All	Wave 1B "good" news	Wave 1B "bad" news	Wave 2 All	Wave 3 All
	(1)	(2)	(3)	(4)	(5)
Panel A. Peer income					
$Prior_i$	0.787***	0.800***	0.822***	0.367***	0.735***
	(0.020)	(0.028)	(0.036)	(0.022)	(0.039)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.256***	-0.230***	-0.284***	-0.286***	-0.589***
	(0.025)	(0.050)	(0.054)	(0.024)	(0.046)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.259***	0.235***	0.283***	0.287***	0.589***
	(0.026)	(0.050)	(0.053)	(0.024)	(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
Panel B. Peer debt					
$Prior_i$	0.943***	0.862***	1.064***	0.717***	
	(0.030)	(0.055)	(0.044)	(0.057)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.718***	-0.743***	-0.637***	-0.355***	
	(0.049)	(0.100)	(0.092)	(0.074)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.743***	0.725***	0.666***	0.378***	
	(0.050)	(0.093)	(0.096)	(0.077)	
Observations	552	131	414	523	
R-squared	0.717	0.658	0.750	0.312	
F-stat	111.8	33.76	23.96	12.11	

Notes: See notes to Table 3. Estimates are for those aged less than 50 in top panels and those aged above 50 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***	0.812***	0.888***	0.429***	0.683***
	(0.028)	(0.038)	(0.048)	(0.030)	(0.049)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.284***	-0.268***	-0.475***	-0.282***	-0.561***
	(0.037)	(0.067)	(0.083)	(0.042)	(0.056)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.286***	0.273***	0.470***	0.283***	0.560***
	(0.037)	(0.068)	(0.081)	(0.042)	(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
Panel B. Peer debt					
$Prior_i$	0.930***	0.944***	1.061***	0.768***	
	(0.033)	(0.040)	(0.077)	(0.072)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.784***	-0.835***	-0.735***	-0.315***	
	(0.058)	(0.083)	(0.126)	(0.105)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.810***	0.836***	0.769***	0.340***	
	(0.058)	(0.079)	(0.130)	(0.107)	
Observations	333	106	225	292	
R-squared	0.710	0.763	0.670	0.301	
F-stat	104.8	63	18.40	5.765	
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***	0.781***	0.827***	0.356***	0.779***
	(0.021)	(0.029)	(0.040)	(0.023)	(0.040)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.236***	-0.206***	-0.249***	-0.275***	-0.590***
	(0.025)	(0.045)	(0.054)	(0.025)	(0.049)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.240***	0.209***	0.250***	0.278***	0.595***
	(0.025)	(0.046)	(0.054)	(0.025)	(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
Panel B. Peer debt					
$Prior_i$	0.990***	0.993***	1.096***	0.633***	
	(0.029)	(0.059)	(0.042)	(0.052)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.714***	-0.796***	-0.621***	-0.370***	
	(0.046)	(0.109)	(0.079)	(0.066)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.737***	0.766***	0.647***	0.385***	
	(0.047)	(0.103)	(0.082)	(0.069)	
Observations	521	105	410	509	
R-squared	0.773	0.773	0.789	0.287	
F-stat	121.9	28.54	31.34	15.75	

Notes: See notes to Table 3. 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 income					
$Prior_i$	0.765***	0.794***	0.802***	0.445***	0.512***
	(0.026)	(0.033)	(0.053)	(0.033)	(0.043)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.223***	-0.168***	-0.314***	-0.313***	-0.482***
	(0.036)	(0.063)	(0.086)	(0.037)	(0.048)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.225***	0.171***	0.313***	0.315***	0.484***
	(0.036)	(0.065)	(0.084)	(0.037)	(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
Panel B. Peer debt					
$Prior_i$	1.001***	0.951***	1.117***	0.788***	
	(0.040)	(0.047)	(0.060)	(0.081)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.795***	-0.787***	-0.792***	-0.387***	
	(0.059)	(0.102)	(0.094)	(0.100)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.815***	0.783***	0.818***	0.406***	
	(0.061)	(0.099)	(0.097)	(0.103)	
Observations	446	114	330	379	
R-squared	0.658	0.664	0.667	0.260	
F-stat	90.35	34.46	35.73	7.788	
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 income					
$Prior_i$	0.750***	0.694***	0.828***	0.298***	0.452***
	(0.025)	(0.038)	(0.044)	(0.026)	(0.045)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.243***	-0.229***	-0.295***	-0.237***	-0.380***
	(0.027)	(0.043)	(0.061)	(0.025)	(0.046)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.246***	0.233***	0.295***	0.239***	0.379***
	(0.027)	(0.044)	(0.060)	(0.025)	(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
Panel B. Peer debt					
$Prior_i$	0.925***	0.968***	1.035***	0.562***	
	(0.029)	(0.054)	(0.049)	(0.055)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.712***	-0.844***	-0.592***	-0.279***	
	(0.045)	(0.098)	(0.088)	(0.070)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.744***	0.820***	0.629***	0.295***	
	(0.046)	(0.092)	(0.092)	(0.073)	
Observations	408	97	305	422	
R-squared	0.772	0.805	0.779	0.248	
F-stat	133.8	42.60	24.78	8.193	

Notes: See notes to Table 3. 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 income					
$Prior_i$	0.828***	0.824***	0.905***	0.467***	0.736***
	(0.020)	(0.029)	(0.038)	(0.027)	(0.042)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.277***	-0.243***	-0.386***	-0.296***	-0.546***
	(0.029)	(0.057)	(0.068)	(0.038)	(0.049)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.281***	0.249***	0.385***	0.300***	0.546***
	(0.029)	(0.058)	(0.067)	(0.038)	(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
Panel B. Peer debt					
$Prior_i$	0.892***	0.860***	1.002***	0.540***	
	(0.060)	(0.120)	(0.070)	(0.116)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.718***	-0.735***	-0.737***	-0.444***	
	(0.078)	(0.154)	(0.125)	(0.118)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.738***	0.736***	0.762***	0.469***	
	(0.080)	(0.151)	(0.129)	(0.120)	
Observations	395	79	314	322	
R-squared	0.393	0.456	0.373	0.064	
F-stat	42.85	13.38	17.43	8.239	
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 income					
$Prior_i$	0.754***	0.753***	0.797***	0.332***	0.674***
	(0.025)	(0.036)	(0.043)	(0.026)	(0.048)
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.240***	-0.193***	-0.251***	-0.252***	-0.558***
	(0.029)	(0.052)	(0.067)	(0.028)	(0.055)
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.242***	0.195***	0.250***	0.253***	0.561***
	(0.029)	(0.053)	(0.066)	(0.028)	(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
Panel B. Peer debt					
$Prior_i$	0.883***	0.942***	0.967***	0.659***	
	(0.028)	(0.055)	(0.047)	(0.058)	
$Prior_i \times \mathbb{I}(i \in Treatment)$	-0.740***	-0.841***	-0.673***	-0.283***	
	(0.042)	(0.097)	(0.072)	(0.074)	
$\mathbb{I}(i \in Treatment) \times TreatmentSignal_i$	0.762***	0.814***	0.706***	0.300***	
	(0.043)	(0.087)	(0.076)	(0.077)	
Observations	353	113	234	373	
R-squared	0.743	0.745	0.766	0.295	
F-stat	160.2	52.33	43.53	7.538	

Notes: See notes to Table 3. 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 Income		Peer Debt	
	Wave 1B	Wave 2	Wave 1B	Wave 2
	(1)	(2)	(3)	(4)
$Prior\ Income_i$	0.711***	0.396***	0.341***	0.456***
	(0.016)	(0.018)	(0.065)	(0.121)
$Prior\ Income_i \times \mathbb{I}(i \in Treatment\ Income)$	-0.188***	-0.275***	-0.211**	-0.294
	(0.023)	(0.024)	(0.100)	(0.184)
$Prior\ Income_i \times \mathbb{I}(i \in Treatment\ debt)$	-0.012	-0.016*	-0.042	-0.079
	(0.008)	(0.010)	(0.055)	(0.093)
$\mathbb{I}(i \in Treatment\ Income) \times TreatmentIncomeSignal_i$	0.209***	0.297***	0.370***	0.488***
	(0.023)	(0.024)	(0.094)	(0.186)
$Prior\ Debt_i$	0.030***	0.025***	0.903***	0.627***
	(0.005)	(0.006)	(0.037)	(0.064)
$Prior\ Debt_i \times \mathbb{I}(i \in Treatment\ Income)$	-0.017**	-0.017**	-0.138***	-0.166*
	(0.007)	(0.008)	(0.048)	(0.095)
$Prior\ Debt_i \times \mathbb{I}(i \in Treatment\ debt)$	-0.032***	-0.013	-0.697***	-0.278***
	(0.007)	(0.009)	(0.053)	(0.095)
$\mathbb{I}(i \in Treatment\ Debt) \times TreatmentDebtSignal_i$	0.046***	0.029***	0.762***	0.368***
	(0.005)	(0.007)	(0.039)	(0.061)
Observations	1,493	1,512	1,286	1,223
R-squared	0.711	0.400	0.732	0.294
F-stat: Income treatment	33.64	54.49	9.467	3.644
F-stat: Debt treatment	25.01	8.646	133.1	13.14

Notes: The table reports results when the specification (3) includes both income and debt variables at the same time. See notes to Table 3.

Appendix Table A7. Effects of Relative Perceived Income and Debt on Happiness: Those Receiving "Bad" News

	Dependent variable: Happiness					
	OLS	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. **Peer** income						
Posterior log (HH income/Peer HH income)	0.28***	0.10	0.11	0.17	0.05	0.22
	(0.09)	(0.31)	(0.30)	(0.30)	(0.35)	(0.37)
Income in the Netherlands are too large			-0.00			-0.00
			(0.02)			(0.02)
Time spent with friends/colleagues/neighbors				0.02***		0.02***
				(0.01)		(0.01)
Spending on non-durables goods, log(spending)					-0.01	-0.02
					(0.02)	(0.02)
Spending on durables goods, log(spending)					0.01	0.01
					(0.01)	(0.01)
Observations	442	445	444	445	399	399
R^2	0.09	0.10	0.10	0.11	0.11	0.14
Panel B. Peer debt						
Posterior log (HH debt/Peer HH debt)	0.04	0.07	0.06	0.05	0.15**	0.11
	(0.06)	(0.06)	(0.07)	(0.06)	(0.08)	(0.08)
Income in the Netherlands are too large			0.05***			0.03
			(0.02)			(0.02)
Time spent with friends/colleagues/neighbors				0.01		0.02*
				(0.01)		(0.01)
Spending on non-durables goods, log(spending)					0.02	-0.02
					(0.03)	(0.03)
Spending on durables goods, log(spending)					0.01	0.01
					(0.02)	(0.02)
Observations	87	88	77	78	75	70
R^2	0.14	0.48	0.52	0.50	0.41	0.49

Notes: See notes to Table 8.

Appendix Table A8. Effects of Relative Perceived Income and Debt on Happiness: Receiving "Good" News

	Dependent variable: Happiness					
	OLS	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Peer income						
Posterior log (HH income/Peer HH income)	0.25***	1.44***	1.32***	1.20***	0.64	-0.17
	(0.07)	(0.50)	(0.45)	(0.43)	(0.50)	(0.52)
Income in the Netherlands are too large			0.00			-0.03**
			(0.02)			(0.01)
Time spent with				0.01**		0.01
friends/colleagues/neighbors				(0.01)		(0.01)
Spending on non-durables goods,					-0.01	-0.00
log(spending)					(0.02)	(0.02)
Spending on durables goods, log(spending)					0.02**	0.02**
					(0.01)	(0.01)
Observations	599	606	606	608	537	535
R^2	0.11	0.03	0.05	0.06	0.13	0.14
Panel B. Peer debt						
Posterior log (HH debt/Peer HH debt)	0.01	-0.16	-0.42	-0.03	0.84	-1.05
	(0.04)	(0.31)	(0.43)	(0.19)	(1.09)	(1.43)
Income in the Netherlands are too large			-0.01			0.01
			(0.02)			(0.02)
Time spent with				0.03***		0.03**
friends/colleagues/neighbors				(0.01)		(0.01)
Spending on non-durables goods,					-0.03	-0.05
log(spending)					(0.06)	(0.08)
Spending on durables goods, log(spending)					-0.01	-0.01
					(0.01)	(0.01)
Observations	330	330	326	328	296	297
R^2	0.08	0.07	0.05	0.10	-0.05	-0.16

Notes: See notes to Table 8.

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

Dep. var. is indicated in the title of the panel	Gender		Age		Education		Income		Debt	
To what extent do you agree with	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 (HH income/Peer HH income)	-2.17**	-2.31***	-3.08**	-1.80**	-2.41**	-2.27***	-1.11	-2.21**	-1.94**	-1.91**
	(0.90)	(0.87)	(1.20)	(0.75)	(1.00)	(0.86)	(1.37)	(0.89)	(0.98)	(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 (HH income/Peer HH income)	-1.96*	-1.78*	-2.15*	-1.80**	-2.07**	-1.88*	-0.27	-2.31***	-0.97	-2.51**
	(1.00)	(0.92)	(1.20)	(0.84)	(1.00)	(0.96)	(1.62)	(0.88)	(1.05)	(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 (HH income/Peer HH income)	2.16**	1.43*	1.94**	1.52*	1.09	1.97**	3.61***	0.82	0.93	2.05**
	(0.96)	(0.81)	(0.89)	(0.80)	(0.84)	(0.83)	(1.37)	(0.77)	(0.89)	(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 (HH income/Peer HH income)	-0.27	1.92*	0.08	1.35*	1.48	0.40	0.29	0.74	0.14	0.80
	(0.82)	(1.03)	(1.30)	(0.80)	(0.97)	(0.88)	(1.50)	(0.90)	(0.98)	(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 (HH income/Peer HH income)	-0.70	0.19	1.01	-0.76	-0.05	-0.15	0.42	0.07	0.19	-0.61
	(0.93)	(0.97)	(1.19)	(0.83)	(1.12)	(0.85)	(1.46)	(0.85)	(1.02)	(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 (HH income/Peer HH income)	0.44	1.54	0.27	1.28	1.03	0.76	2.54	0.18	1.56	-0.73
	(0.96)	(1.16)	(1.35)	(0.90)	(1.03)	(1.03)	(1.68)	(1.00)	(1.03)	(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 (HH income/Peer HH income)	-2.49***	-1.12	-2.90**	-1.26*	1.60	-1.88**	-0.60	-2.17***	-1.70*	-1.63*
	(0.89)	(0.89)	(1.19)	(0.75)	(0.98)	(0.83)	(1.44)	(0.25)	(0.90)	(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 (HH income/Peer HH income)	0.56**	0.67**	0.54	0.67***	0.38	0.80***	0.40	0.60**	0.34	0.55*
	(0.28)	(0.28)	(0.40)	(0.23)	(0.26)	(0.27)	(0.43)	(0.25)	(0.29)	(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 Table 4 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 (HH income/Peer HH income)	4.25	8.18	10.38**	2.87	9.20*	5.22	-6.92	7.81**	3.58	3.67
	(4.68)	(5.88)	(5.11)	(5.07)	(5.55)	(5.14)	(9.19)	(3.84)	(3.82)	(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 (HH income/Peer HH income)	2.46	6.93	1.49	4.31	8.16	4.97	10.12	3.14	1.09	6.55
	(5.68)	(4.65)	(5.63)	(5.09)	(6.43)	(4.36)	(9.95)	(3.26)	(4.34)	(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 (HH income/Peer HH income)	-1.24	2.66*	0.79	2.11	1.62	0.62	1.31	0.47	2.68**	-1.78
	(1.33)	(1.58)	(1.13)	(1.73)	(1.54)	(1.26)	(2.38)	(0.92)	(1.18)	(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 4 by subsamples.

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

	Income		Debt	
	Raw	Adj.	Raw	Adj.
	(1)	(2)	(3)	(4)
Table 4				
Differences in incomes in the Netherlands are too large.	0.0019	0.0010	-	-
Incomes should be made more equal.	0.0132	0.0040	0.7511	0.9710
Differences in income are an incentive for individual effort.	0.0953	0.1289	0.5835	0.9191
In the long run, hard work usually brings a better life.	0.3245	0.4126	0.5019	0.9191
Hard work does not generally bring success – it is more a matter of luck and connections	0.9634	0.9610	0.4758	0.9191
Cheating on taxes if you have a chance can never be justified	0.2216	0.4126	0.3086	0.7632
Differences in wealth in the Netherlands are too large	0.0127	0.0040	0.5329	0.9191
Table 5				
Meetings with friends, neighbors, acquaintances or colleagues outside working hours	0.0171	0.0040	0.8411	0.9710
Visits of a sports club, community center or other club	0.0552	0.0559	0.0190	0.0100
Participation in a political or social organization/activities	0.4815	0.7093	0.9167	0.9710
Table 6				
Employment status	0.0188	0.0040	na	na
Log(HH income)	0.0000	0.0010	na	na
Table 7				
Spending on non-durable goods, log(spending).	0.0041	0.0020	0.0000	0.0010
Spending on durable goods, log(spending).	0.0020	0.0010	0.1096	0.2038
Spending on durable goods, extensive margin, linear probability model.	0.0022	0.0010	0.0545	0.0569
Take out a new loan or mortgage, linear probability model.	0.2309	0.4126	0.2428	0.6484
Table 8				
Happiness	0.8494	0.9610	0.3371	0.7812

Notes: the table reports("raw") p-values for coefficients reported in Tables 4-8 as well as p-values adjusted ("Adj.") for multiple hypothesis testing (Romano and Wolf 2016).

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

Notes: Graphs show to every treated respondent their own self-reported level of total household income (upper graph) or debt (lower graph) versus the income or debt of a typical household like them.

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, 36-45, 46-65, 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
Panel A. Net household income per year for singles (in euro)				
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
Panel B. Net household income per year for couples (in euro)				
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
Panel A.1 Total household debt for singles, renters (in euro)				
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
Panel A.2 Total household debt for singles, homeowners (in euro)				
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
Panel B.1 Total household debt for couples, renters (in euro)				
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
Panel B.2 Total household debt for couples, homeowners (in euro)				
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 ours that I work are 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 = "Unemplyed 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)
[ ]	[ ]	[ ]	[ ]	[ ]	[ ]	[ ]

(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 Netherland'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

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