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# Household Debt and Social Interactions<sup>#</sup>

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## *Abstract*

Debt-induced crises, including the subprime, are usually attributed exclusively to supply-side factors. We uncover an additional factor contributing to debt culture, namely social influences emanating from the perceived average income of peers. Using unique information from a representative household survey of the Dutch population that circumvents the need to define the social circle, we consider collateralized, consumer, and informal loans. We find robust social effects on borrowing—especially among those who consider themselves poorer than their peers—and on indebtedness, suggesting a link to financial distress. We check the robustness of our results using several approaches to rule out spurious associations and handle correlated effects.

*Keywords:* Household finance, household debt, social interactions, mortgages, consumer credit, informal loans.

JEL Classifications: G11, E21

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## 1. Introduction

The recent financial crisis has demonstrated the potential for sizeable household groups to borrow at levels that expose them to subsequent difficulties in debt servicing and a nontrivial risk of default. Many U.S. households, for example, had exposed themselves to excessive mortgage debt prior to the subprime crisis, and some ended up with negative equity following the reversal of the historical house price trends. Yet the existing literature and public discussion have paid almost exclusive attention to the supply-side factors that may have contributed to this tendency, such as lax standards in the banking sector, the transfer of risk, and the resulting lack of discipline in applying sound banking standards.<sup>1</sup>

Far less attention has been devoted to understanding the demand-side factors that contribute to the spread of a debt culture. One such factor, specific to the subprime crisis, is borrowers' subjective belief—based on the long historical experience of price increases—that U.S. house prices could not fall.<sup>2</sup> Our paper, however, focuses on another major factor applicable to all types of debt: comparison with peers, especially among households who see themselves as having fewer resources than the average of their peers.

Although the role of perceived relative standing has been explored in many contexts, including consumption behavior, less attention has been paid to how “catching up” or “keeping up” with peers is financed, and hardly any to whether perceptions of relative standing are important for debt behavior. Yet, if such social influences are at work, regulation and monitoring of financial institutions may need to be combined with household measures (e.g., financial education, proper advice, product sale restrictions, appropriate default options)

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<sup>1</sup> See, for example, Mian and Sufi (2009), who show a shift in credit supply to be a key factor in the expansion of subprime mortgages in the U.S., and Demyanyk and Van Hemert (2012), who find that the quality of such loans deteriorated for six consecutive years prior to the crisis. Christelis, Georgarakos and Haliassos (forthcoming), using recently available data, show that shortly prior to the recent crisis, outstanding mortgages were substantially larger among older U.S. households than among their European counterparts with similar resources and characteristics.

<sup>2</sup> See, for instance, the contributions by Case (2012), Shiller (2012), and Smith (2012).

if the spread of a debt culture and the risks for future debt-induced financial crises are to be contained.

The importance of relative standing in the social circle has long been recognized. For instance, models with interdependent preferences have been applied to consumption (Duesenberry, 1949; Abel, 1990; Gali, 1994, Kuhn et al., 2011); asset pricing (Campbell and Cochrane, 1999); investing in assets (Duflo and Saez, 2002; Hong, Kubik and Stein, 2004; Kaustia and Knüpfer, 2012); supply of labor (Neumark and Postlewaite, 1998); work effort (Cohn et al., 2011); and short-run stabilization policy (Ljungqvist and Uhlig, 2000). To the best of our knowledge, however, this paper is the first to investigate the influence of social interactions and comparison effects on borrowing behavior.

More specifically, our paper exploits unique features of the Dutch National Bank Household Survey (DNBHS), which is representative of the entire Dutch population, to uncover a statistically and economically significant influence of perceived relative standing on household behavior with respect to collateralized loans, consumer (uncollateralized) loans, and informal loans. We find that, once we control for demographics, resources, region and time fixed effects, region-specific time trends, and other factors that typically determine borrowing needs, a higher average income in the social circle, as perceived by a household, increases this household's tendency to borrow.<sup>3</sup> Not only is this influence stronger among those who perceive their income to be below average for their social circle, it is also not confined to borrowing: it extends to the household's financial debt burden. We check the robustness of these results using several approaches, including instrumental variable estimation and placebo tests. Our aim is to rule out uninteresting alternative explanations of the peer income-own borrowing relation and address the potential for spurious correlation between the two because of similarity in unobserved characteristics with those of peers.

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<sup>3</sup> The estimated effects are sizeable for both collateralized and consumer debt: a 1,000 euro increase in the perceived monthly average household income of peers is estimated to raise the unconditional likelihood of having collateralized (uncollateralized) loans by 10% (7%).

Nonetheless, uncovering the effects of social interactions on borrowing behavior does pose special challenges not necessarily present in other domains. First, wealth surveys, being subject to a high degree of anonymization, do not typically provide information on social circle characteristics and tend to omit or heavily restrict information on location. Faced with this challenge, social interaction research on the asset side resorts to constructing hypothesized social circles based on sorting assumptions (e.g., age and education), focusing on specific products and groups with visible interactions (e.g., retirement plans in particular establishments), or considering the frequency of meetings. We, however, are able to overcome this limitation because of the unique nature of the DNBHS, which asks respondents to describe key features of their social circle (e.g., age, education, occupation), including the perceived average income of peers. It also reports the entire range of household assets and debts. An additional challenge is that, although households may derive some status from revealing their wages, consumption, or asset levels to their social circle (or may be unable to hide them), they tend to be quite averse to revealing debt levels, a reticence protected by bank confidentiality. Thus, the important channel through which peer effects are likely to operate is not the direct observation and emulation of borrowing behavior among peers but rather the observation of some key determinant of this behavior (e.g., resources or ability to spend).

It should nevertheless be stressed that the research question of social influences on debt is quite distinct from that relating to consumption: even if concern with relative standing leads to greater consumption, it may not lead to a greater tendency to borrow or to run into financial distress for at least three reasons. First, households can increase labor supply together with consumption, leaving room for an increase in both consumption and saving. For example, Neumark and Postlewaite (1998) find that married women in the U.S. are 16 to 25% more likely to work outside the home if their sisters' husbands earn more than their own. It should be noted that even a positive labor supply response can imply either more or less

saving/borrowing.<sup>4</sup> Second, households may choose to reduce saving but may not be willing or able to raise borrowing in response to status concerns. Third, even if borrowing is undertaken to keep up with peers, it does not necessarily lead to financial distress.

Our paper complements three different strands of literature: the effects of social interactions on asset choices, the relative income hypothesis and external habits, and “envy versus ambition.” To address the first, Duflo and Saez (2002) study the saving behavior of employees in different libraries at a large American university and demonstrate that individual participation in retirement investment plans is influenced by colleagues’ participation choices.<sup>5</sup> Hong, Kubik and Stein (2004), on the other hand, focus on sociability, showing that more sociable individuals (in terms of frequent communication with neighbors and church-going) are more likely to own stocks.<sup>6</sup>

As regards the second, the importance of peer income for consumption was first stressed in the (cross-sectional) formulation of Duesenberry’s (1949) relative income hypothesis, built on insights from Veblen (1899) and Smith (1759). According to this hypothesis, households whose incomes are below average in their social circle tend to consume a larger share of their income to keep up with peers. Modern analyses of consumption and asset pricing, however, make a fine distinction between “keeping up” and “catching up” with the Joneses.<sup>7</sup> Recent work by Kuhn et al. (2011) examines the effects on consumption of winning a Dutch postal code lottery. Using specially collected survey data on

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<sup>4</sup> Most existing theoretical models, which are based on an infinite-horizon representative agent, imply greater consumption, less leisure, and greater accumulation of assets in order to keep up with the Joneses both now and in the future (Liu and Turnovsky, 2005). When Alvarez-Cuadrado and Van Long (2008) consider overlapping generations in an infinite-horizon economy, however, they find less leisure but also lower saving.

<sup>5</sup> Such endogenous social effects could come from learning about assets or from discovering relevant social norms, but it is difficult to distinguish the two.

<sup>6</sup> Brown et al. (2008) identify a causal influence of sociability on stockholding by instrumenting the average stock ownership of an individual’s community with past average ownership of the U.S. states in which the individual’s nonnative neighbors were born. Georgarakos and Pasini (2011) document similar effects of sociability on stockholding in Europe.

<sup>7</sup> In a more recent model developed by Roussanov (2010), which focuses particularly on private business ownership, utility is a function of relative wealth and households are characterized by a desire to “get ahead of the Joneses”.

expenditures and different assumptions about the social circle, the authors find that exogenous variations in income from winning the lottery tend to influence not only the durables purchased by winners but also the probability that neighbors will buy a new car.<sup>8</sup>

Whereas the level of consumption may be influenced by relative income, however, the composition of consumption may signal an individual's income relative to others. The literature on conspicuous consumption (see Charles, Hurst, and Roussanov, 2009, and the references therein), for example, stresses utility from status, defined as others' perceptions of one's own income. Since income is not directly observable, perceptions are based on the person's group (e.g., a racial group in a particular state) and on observed ("conspicuous") consumption. This desire to signal status can explain why minority races in the U.S. tend to spend larger fractions of their budgets on conspicuous consumption than do Whites with similar permanent income.<sup>9</sup> Such outlay is financed through lower minority spending on other items (particularly, health and education) and through savings, although the extant research does not investigate borrowing. A link between conspicuous consumption and borrowing is addressed, however, in the theoretical model of status developed by Rayo and Becker (2006), who argue that, in order to signal status to more people over a longer period of time, conspicuous consumption goods tend to be durables, which often require borrowing finance.

The third strand of literature addresses relative income and self-reported happiness or general well-being. Although a number of these studies demonstrate a negative influence of others' income on subjective well-being,<sup>10</sup> Hirschmann and Rothschild (1973) stress utility-

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<sup>8</sup> Their survey, however, collected no information on debts or on participants' perceptions of their peers.

<sup>9</sup> Conspicuous consumption rises with an individual's own income and falls with the average level of income of that person's group. Hence, for signaling purposes, although the poorest person in the group need not spend more on conspicuous consumption, all others need to do so.

<sup>10</sup> For example, Clark and Oswald (1996) show that workers' job satisfaction is negatively influenced by the income earned by other individuals in their reference group. Likewise, Ferrer-i-Carbonell (2005), using German panel data, finds that the larger an individual's income compared to acquaintances, the better off that individual tends to be.

enhancing “anticipatory feelings” (Caplin and Leahy, 2001), which they term the “tunnel effect.” This effect is analogous to an individual caught in a traffic jam in a tunnel who, on seeing another lane moving, anticipates that the own line will also move soon.<sup>11</sup>

The rest of the paper is organized as follows. Section 2 describes the unique features of our data set. Section 3 discusses possible channels through which peers might influence borrowing behavior and the econometric approach taken to address a number of challenges. Section 4 presents the main results on the relation between peers’ perceived income and debt behavior, including those from endogeneity tests and IV estimates. Section 5 reports the results of the placebo tests and additional robustness checks. Section 6 provides evidence of asymmetric effects on borrowing across households poorer and richer than the peer average and inspects likely channels through which peer income operates. Section 7 concludes the paper.

## **2. The Data**

The DNBHS, launched in 1993, provides a unique data set that includes information on work, pensions, housing, mortgages, income, assets, consumer loans, health, economic and psychological concepts, as well as personal characteristics. It thus allows the study of both the psychological and economic aspects of financial behavior. The initial survey was administered to around 2,790 Dutch households oversampled from the top 10% of the income distribution and weighted to be representative of the Dutch-speaking population. Since then, households have been re-interviewed annually, with new households added each year to counteract the non-negligible attrition and keep the cross-sectional sample representative. Because the survey underwent a major refreshing in 2001, resulting in a sample of 1,861

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<sup>11</sup> Senik (2004) finds empirical support for the “tunnel effect” using survey data from Russia and in later work, documents a negative comparison income effect in many “old” European countries and a positive one (i.e., consistent with a “tunnel effect”) in East European countries and the U.S., linked mainly to the degree of income mobility (Senik, 2008).



households, we pool data from the 2001 to 2008 waves, which cover a period of relatively stable employment rates and increasing housing prices.<sup>12</sup> We also employ survey weights to account for the overrepresentation of the wealthy. After excluding households with incomplete questionnaires or missing information on social circle characteristics, the pooled sample used in the baseline estimations consists of roughly 4,500 households.

This survey not only includes an extensive questionnaire on income and real and financial wealth holdings, it also asks specific debt-related questions. These responses allow us to distinguish between collateralized and uncollateralized debt, as well as informal loans from friends and relatives. Hence, in the following discussion, although we focus primarily on consumer debt and collateralized debt, we also report results for informal loans.

Table 1 provides summary statistics on the prevalence and the amounts outstanding among debt holders by survey year and by loan type. These figures suggest a relatively stable prevalence of all three types of loan over the years studied. Collateralized debts account for most household borrowing, being held by roughly 40% of the households, with a median conditional outstanding amount of about 98,000 euro. One out of five Dutch households has consumer loans with a median outstanding amount of roughly 4,000 euro.<sup>13</sup> On the other hand, only 4% of households report loans from friends and relatives, although almost 28% report that they can borrow from friends in the future.

In the absence of information on respondents' perceptions of peers, the empirical network literature typically constructs hypothesized social circles based on sorting assumptions (e.g., age-education cells or geographic proximity). One unique feature of the Dutch survey, most relevant for our purposes here, is that individuals are asked to explicitly

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<sup>12</sup> Unemployment rates in the Netherlands reached a minimum of 3% in 2008 but increased to 3.7 and 4.5% in 2009 and 2010, respectively. National housing prices increased on average by roughly 2% each year up to 2008, after which they declined by 2.8% in 2009 and by 3.4% in 2010.

<sup>13</sup> Extended lines of credit (unrelated to home equity) account for roughly 40% of the average outstanding volume of consumer loans, followed by almost 20% from checking account overdrafts. Student loans account for 17% and private loans for 12%, while only 6% relate to outstanding credit card debts.

report a number of characteristics of those with whom they “associate frequently, such as friends, neighbors, acquaintances, or maybe people at work.” This subjective information can be particularly helpful for understanding who interacts with whom and circumvents the key issue of defining the social circle. Indeed, Soetevent (2006) stresses the potential of such information for social interaction models, while Woittiez and Kapteyn (1998) exploit such subjective information (on peer age and education) from the DNBHS to assess peer effects on female labor supply.

DNBHS respondents also report their perceptions of the average annual total net household income among people in their social circle, recorded in one of eleven income brackets (see the appendix). Other questions cover the age category of most members of their social circle, as well as the average household size, average education, most prevalent type of employment, and average hours of work per week of their peers distinguished by gender. The survey also asks directly about respondents’ interactions with peers in relation to exchanging financial information or informal borrowing, perceptions of the social circle’s spending ability, and expectations for their own future income. We use this information in the empirical analysis to shed light on the process through which social interactions influence borrowing behavior.

### **3. Effects of the Social Circle on Debt Behavior**

#### **3.1 Possible Channels**

The asset market participation and holdings of peers may influence any member of the peer group via direct observation of financial behavior, information sharing, and dissemination of social norms. Peer effects in borrowing behavior, however, are much less likely to emanate from direct observation of peers’ loans or even from discussions with them about their indebtedness. That is, loans, unlike assets, are not directly observable by third

parties and can only become known if borrowers decide to reveal them, but borrowers are less likely to discuss their loans than exhibit their assets because of embarrassment or shame.<sup>14</sup> Nonetheless, financial advice and consultation with members of the social circle may inform households about the process of obtaining formal or informal loans and/or about the social norms of borrowing; it may even lead to informal loans from the social circle. We can explicitly take this possible channel of effects into account because our data allow us to identify households that consult family, friends, and acquaintances about financial decisions and that can borrow from their social circle in the future.

Even households that do not consult their social circle may still be influenced by that circle's observable behavior when deciding whether to take out a loan and how much to borrow. Households form perceptions of acquaintances' average disposable income based on social interaction sources that range from direct knowledge of acquaintances' pay scales to open discussions with friends and family, and inference of income levels from observed spending or asset accumulation patterns.<sup>15</sup> Because the DNBHS asks respondents directly about the perceived average income of their acquaintances, we can directly assess these perceptions' influence on their borrowing behavior. In doing so, we assume that perceptions of higher peer income may contribute positively to borrowing through three possible channels: trying to emulate the spending or living standards of acquaintances (a comparison effect), inferring that more can be borrowed directly from them in the future, and inferring that one's own future income is likely to move in the same direction as that of one's social circle (an expectation or "tunnel" effect).

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<sup>14</sup> Such considerations have been shown to be important even in countries with underdeveloped credit markets, where informal borrowing is quite widespread. For example, Collins et al. (2009), using data from Bangladesh, India, and South Africa, find that many indebted households feel ashamed to ask relatives for additional credit or do not wish to reveal their financial situation to close acquaintances.

<sup>15</sup> Indeed, imputation of incomes on the basis of spending items or asset accumulation is sometimes used by tax authorities to fight tax evasion in countries in which these phenomena are widespread.

With reference to the first channel, spending behavior or visible accumulation of assets (like housing) by members of the social circle may well induce a household to borrow in order to match (or exceed) it. Our data allow us to observe respondents' perceptions of peers' spending ability and to take them into account in our estimation. Since we do not observe respondents' perceptions of peers' housing or living standards, we impute them from the data reported by households that could belong to the same peer group, as they share some key characteristics. The data do allow us to directly address the second channel, using household responses on whether they are in a position to borrow a significant amount of money from friends and relatives. As regards the third channel, we first use households' reported expectations for their own future income and then examine robustness of our results using respondent's permanent income rather than expected future income.

### **3.2 Econometric Specification**

In the benchmark specification, we examine whether households' tendency to borrow and the size of the loans conditional on borrowing are influenced by the average income they perceive their peer group to have. We derive the latter from responses to the following survey question: "If you think of your circle of acquaintances, how much do you think is the average total net income per year of those households?" The possible answers are presented in brackets. In our reported results, we use the midpoints of these bands, adjusted for inflation, while in the appendix, we detail a number of robustness checks that employ different specifications of this variable.<sup>16</sup>

The extant literature on social interactions in consumption or asset holdings focuses on uncovering what Manski (1993) terms "endogenous social effects"; namely, the direct effects of observing others' behavior (e.g., consumption or asset holdings) on one's own

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<sup>16</sup> We experiment, for example, with using dummy variables for income bands and a flag dummy for "don't know" responses, but the results are insensitive to these variations.

actions.<sup>17</sup> Econometric modeling in this context must address the reflection problem generated when the behavior of households in a group is expressed as a function of the average behavior of the group that includes them. Given that debts, as discussed earlier, are typically unobservable by other social circle members, our primary focus is on uncovering exogenous or contextual effects; that is, influences on debt behavior that emanate from observing (or forming perceptions of) not debts but key peer characteristics relevant for debt behavior (e.g., income).

In this setting, the two major challenges are to rule out (i) spurious links between peer income and the respondent's own borrowing behavior that have little to do with a comparison effect and (ii) correlated effects, an association between these two variables stemming from similarities in the unobserved characteristics of the respondent or respondent's environment and those of peers.

In our regressions, we control separately for the net income of peers and the effects of household resources in the form of net income, net financial wealth, and net real wealth.<sup>18</sup> We also take into account the respondent's age (through a second-order polynomial), gender, and educational attainment, as well as marital status and number of children.

One standard, albeit uninteresting, potential source of an effect of peers' higher perceived income on borrowing relates to an adverse idiosyncratic shock; that is, once income is controlled for, the higher the perceived average income of peers, the greater the chance that the household has experienced a bad idiosyncratic shock during this period. In such a case, standard models would prescribe more borrowing to smooth any adverse transitory shock. We control for this possibility by including in our specifications self-

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<sup>17</sup> For thorough reviews of methodological issues in social interaction models, see Soetevent (2006) and Durlauf and Ioannides (2010).

<sup>18</sup> We allow for nonlinear effects of household net income, financial and real wealth, and peers' net income (all of which have skewed distributions) by means of an inverse hyperbolic sine (IHS) transformation (i.e.,  $\log(x+(x^2+1)^{1/2})$ ). The advantage of this near-logarithmic transformation is that it is defined for zero and negative values (see also Pence, 2006). Our results are robust to alternative specifications of the aforementioned covariates (e.g., dummies denoting quartiles).

reported health, labor market status dummies, and, most especially, answers to a direct question on whether last year's income was "unusually low." In addition, as described in Section 5, we estimate models that control for a household permanent income proxy that is more resilient to temporary shocks.

Another possibility is that the respondent's perception of higher income in the social circle partly reflects a macro or regional shock, so that perceptions could improve simply based on better performance in the macro-economy or the region in which most of the social circle is located. We take these two channels into account flexibly by including both year and province<sup>19</sup> fixed effects in all our specifications. To account for any region-specific time trends that may correlate with trends in both peer income and borrowing (e.g., more rapid housing price appreciation in certain regions), we condition our specifications on a full set of interaction terms between province and year fixed effects.<sup>20</sup>

A more complex potential channel for a positive association between peer income and borrowing is associated with correlated effects; that is, there may be unobserved factors that influence both the desire to borrow and the desire to associate with high-income peers and acquaintances. In the case of informal loans, this link could be very direct: respondents would be more likely to associate with high-income peers and relatives in order to borrow from them. In the case of formal loans, the link could be more subtle. For any given need to borrow, higher income friends could provide more informal loans, reducing the need for formal borrowing. However, unobserved factors could also make respondents more likely to borrow using any type of loan while simultaneously encouraging them to associate with peers

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<sup>19</sup> We are able to use disaggregated information about place of residence for twelve Dutch provinces; namely, Groningen, Friesland, Drenthe, Overijssel, Flevoland, Gelderland, Utrecht, Noord-Holland, Zuid-Holland, Zeeland, Noord-Brabant, and Limburg.

<sup>20</sup> When modeling collateralized borrowing, we also conduct a series of robustness checks to examine the sensitivity of our results to inclusion of certain relevant regional indicators and to a specific functional form of time trends (i.e., instead of the general form adopted in the baseline regressions).

perceived as wealthy and in a position to provide informal loans. In this case, a spurious positive relation might be generated between peer income and all types of loans.

We also consider the possibility of reverse causality: households may borrow with the specific aim of associating with people whom they perceive as earning more. We, however, consider this explanation less compelling a priori than the possibility that households borrow because they perceive their peers as having larger income. Nonetheless, there is still a distinct possibility that it is because households borrow that they come to think of their peers as having more income. It is worthwhile, therefore, to consider both possibilities: correlated effects and reverse causality.

One possible approach to addressing the former would be to allow for peer group fixed effects; however, as this method cannot also handle potential reverse causality, it is not the most suitable choice for our data. Moreover, its application would require that the peer groups be either known or assumed and that the (observable) variation within them be sufficient to identify the causal relation of interest.<sup>21</sup> A key advantage of our data is that they allow us to avoid making arbitrary assumptions about the identity of peers or respondent perceptions of peers, as they ask respondents directly about their perceptions of peer characteristics. However, our data record perceptions regarding average characteristics of peers and record only average characteristics—not variation between peers. Thus, applying this method would be inconsistent with exploiting the strengths of our data. Rather, we pursue two alternative ways to address identification in the presence of correlated effects: instrumental variable estimation, which also addresses concerns of possible reverse causality, and estimation of peer income's influence using a series of placebo regressions.

A valid instrumental variable must have two attributes: it should be correlated with the covariate of interest (i.e., perceived average income of the social circle), and its effects on

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<sup>21</sup> See, for example, Lundborg (2006) and Soetevent and Kooreman (2007) who use school grade (or class) fixed effects and exploit the variation within classmates in order to assess health behavior among adolescents in schools.

an individual's own borrowing should run through peer income but not other unobserved factors. Our identification strategy exploits variations in local labor market conditions and the asymmetric effects that these can have for the incomes of households with different educational backgrounds. That is, a given difference in educational attainment between respondent and peers can imply a larger difference in (perceived) incomes in regions with better conditions for highly educated workers. We thus use as our instrument the *interaction* between regional employment rates in high-tech sectors and the difference in educational attainment between each respondent and the peers.

At the same time, we control for the respondent's own educational attainment and occupational status, for peers' average level of educational attainment, for province and time fixed effects, and for the entire set of interactions between province and time fixed effects.<sup>22</sup> The identification assumption is that the educational gap between respondent and peers will raise the respondent's perception of peers' average income, and even more so when the regional employment share in high technology occupations, for which education matters greatly, is larger.<sup>23</sup> Indeed, results from the auxiliary regressions (presented in Appendix Table D.1) suggest a strong positive association between our instrument and the perceived income of peers.

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<sup>22</sup> Province fixed effects serve to absorb any regional disparities (e.g., due to development, unemployment, or bank diffusion) that are likely to have a direct influence on individual borrowing. Time fixed effects absorb any common time trend, while their interactions with provinces take into account any region-specific time trend. Peers' average level of education takes into account the educational attainment of peers that can be relevant for finding jobs in the high-tech sector (i.e., college or post-secondary education). "High-tech sector" refer to both high-tech manufacturing industries (the manufacture of basic pharmaceutical products and pharmaceutical preparation computer, electronic, and optical products) and high-tech knowledge-intensive services (motion picture, video and television program production, sound recording and music publishing activities; programming and broadcasting activities; telecommunications; computer programming; consultancy and related activities; information service activities; scientific research and development).

<sup>23</sup> Virtually all heads of households in the sample have completed full time education and were not attending any (full or part time) education program at the time of interview. Hence, it is quite unlikely that relatively less-educated individuals living in regions with a high fraction of high-tech sector jobs would decide to borrow in order to invest in their human capital and thereby improve their career prospects.



#### 4. Results for the Role of Peer Income in Borrowing Behavior

To model the likelihood of having loans and the (log) amount of loans outstanding, we estimate a series of probit and tobit models, respectively. Standard errors are adjusted for heteroskedasticity, allowing for clustering at the household level. To show the economic significance of our findings, we report average marginal effects for the probit models, and average marginal effects conditional on having the loan type under study for the tobit models. In this section, we report the results for our baseline specification and IV tests for the three different loan types considered: informal loans, collateralized formal loans, and consumer (uncollateralized) formal loans.

Table 2 reports the results for loans from the social circle, including the average marginal effects from a probit regression modeling the probability that the respondent believes he or she can borrow from friends or relatives in the future if needed (col. 1). The estimated marginal effect of the perceived average income of the social circle is positive and significant at 1%. This figure implies that an assumed increase of the (perceived) annual household income of peers by 12,000 euro (i.e., 1,000 on a monthly basis) is associated with a 2.2 percentage point (pp) higher probability that the respondent reported a future likelihood of borrowing from the social circle. Our instrument is highly significant at 1% in the relevant auxiliary regression, with an F-test well above the rule of thumb threshold of 10. We fail to reject exogeneity, but only marginally so ( $p$ -value: .14). The marginal effect estimated using the IV probit model is statistically significant and higher than the one derived under the simple probit model (7.0 pp, significant at 5%).<sup>24</sup> Our estimation of the probability that respondents currently have informal loans also shows a positive association with average peer

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<sup>24</sup> Given that the original model is nonlinear with one (potentially) endogenous covariate that is continuous, we use standard maximum likelihood routines that fit discrete choice models with one endogenous covariate (e.g., `ivprobit` in Stata). These produce the consistent estimated coefficients and associated standard errors necessary for the computation of marginal effects. An alternative way to test and correct for endogenous covariates in nonlinear regression models is the two-step procedure of Rivers and Vuong (1988), summarized in Wooldridge (2002, p. 473). Application of this procedure to all the models presented in Tables 2, 3, and 4 produces results that are entirely consistent with those reported above.

income on the order of .8 pp<sup>25</sup> ( $p$ -value .06), while we fail to reject the null of exogeneity by a wide margin ( $p$ -value:  $>.75$ ). The results from the tobit regressions on outstanding informal loan amounts paint a similar picture.

Table 3 presents the results for collateralized formal loans, where we additionally control for intentions to borrow from friends in the future and whether the respondent receives financial advice from friends and relatives. We find no significant association between an individual's borrowing behavior and these additional two covariates. If anything, the results suggest a negative association (significant at 10% in the tobit specification) between receiving advice from friends and outstanding levels of collateralized debt. We also estimate the statistically significant effects of the perceived average household income of the social circle (based on an assumed 12,000 euro annual increase) both on the likelihood of having a collateralized loan and on the (conditional) outstanding amount. The estimated marginal effect from probit is 4.2 pp, implying an almost 10% net contribution to the unconditional likelihood of having a mortgage. The estimated effect from tobit suggests a conditional elasticity of 0.48, corresponding roughly to a 15,000 euro increase in the amount borrowed by a typical household with collateralized debt.<sup>26</sup> We again fail to reject the null of peer income exogeneity (see bottom of table), now with  $p$ -values ranging from .19 to .24. Instrumental probit and tobit models yield qualitatively similar marginal effects.<sup>27</sup>

Because our baseline specification allows for interactions between year and province fixed effects, it is flexible enough to account for any region-specific time trends that may influence individual borrowing decisions. Nonetheless, since the period under study is marked by a dramatic appreciation in housing prices and an increase in the home ownership rates relevant for collateralized debt, we perform additional tests on the robustness of our

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<sup>25</sup> This figure corresponds to an almost 20% increase in the unconditional probability of borrowing from friends.

<sup>26</sup> This calculation is based on conditional medians of collateralized debt (98,000 euro) and of peers' income (34,500 euro) among households with outstanding collateral loans.

<sup>27</sup> The estimated marginal effect ( $p$ -value) from the instrumental variable probit model is 9.1 pp (.013); the corresponding estimated conditional elasticity from the instrumental variable tobit models is .95 (.025).

findings. Specifically, we estimate various specifications that control for year and province fixed effects, as well as for province-specific time trends of certain housing indicators. Table C.2 lists the specifications with province-specific yearly growth rates of housing prices, housing stock, and aggregate homeownership rates. We also estimate a specification with a quadratic time trend by province. In all cases, the results from both the probit and tobit models are very similar to those derived under our baseline specification, which allows for any region-specific time trends by taking into account the interaction between province and time fixed effects.

We then test the sensitivity of our findings to the difference between collateralized borrowing for house purchase and that for home equity extraction. Because our data do not allow for a direct distinction, we use the outstanding amount of the first mortgage on the main residence as a lower-bound estimate of the former. Our estimates of the marginal effects of peer income on the first mortgage on the main residence are very similar to those for total collateralized debt.<sup>28</sup>

Table 4 reports the estimates relating to (uncollateralized) formal consumer loans. Here, we estimate a positive marginal effect of peer income on the probability that consumer loans are on the order of 1.6 pp (i.e., contributing about 7% to the unconditional likelihood of having such loans). The corresponding elasticity of the size of consumer loans, conditional on borrowing, to peer income is on the order of 0.24, which implies an increase of approximately 400 euro in the amount borrowed by a typical borrower.<sup>29</sup> Using our instrument, we fail to reject the null of peer income exogeneity in both probit and tobit models by a wide margin (i.e.,  $p$ -values  $> .70$ ). Interestingly, receiving financial advice from

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<sup>28</sup> The conditional median (mean) outstanding amount of the first mortgage on the main residence is 83,194 euro (102,921 euro) with an average prevalence of 36%. Hence, this first mortgage accounts for most of the collateralized borrowing over the period under study. The estimated marginal effect ( $p$ -value) from probit on peer income is 4.1 pp ( $p$ -value less than .001), while the corresponding estimated conditional elasticity from tobit is .49 ( $p$ -value less than .001).

<sup>29</sup> Based on the conditional medians of uncollateralized debt (4,000 euro) and of peers' income (26,000 euro) among households with consumer loans.

the social circle is negatively related to obtaining a consumer loan and to borrowing larger amounts conditional on obtaining the loan. This suggests that financial advice from peers, rather than providing information on how to obtain a formal consumer loan, tends to discourage respondents from doing so.

## **5. Placebo Tests and Additional Robustness Exercises**

To further investigate the issue of endogenous peer income by exploiting the richness of the data, we perform a series of placebo tests to guard against the possibility of unobserved factors that influence both income and the borrowing choices of those of similar age and education living in the same province. The underlying rationale is that if such factors were important, they would operate for any social circle sharing those characteristics and not only for the respondent's specific social circle. To conduct the placebo test, we construct cells based on respondent age, education, province of residence, and interview year and then assign to each respondent in a given cell the acquaintances of another, randomly selected, respondent in that same cell.

The results from these placebo regressions for formal loans are summarized in Table 5. Unlike the income of the respondents' actual social circle, the randomly assigned income of acquaintances is insignificant across all specifications (with  $p$ -values well above .40 and estimated magnitudes that are economically unimportant). We also perform additional placebo tests based on cells constructed using various combinations of the aforementioned traits and respondent gender. In no cases do we find any significant effects of the (randomly assigned) incomes of acquaintances. These results further support the premise that the estimated effects of average peer income in our baseline specification reflect the effects of comparison to peers, rather than being an artifact of social group characteristics.

We next consider the possibility of unobserved factors that systematically influence both the propensity to borrow and the association with more affluent peers. If such factors exist, it would seem plausible that perceived peer income would have a stronger effect on borrowing among those who have received financial advice from friends and/ or who plan to borrow from them in the future, two attributes directly recorded in the DNBHS data. We therefore test for this possibility in formal loans by introducing into the baseline models interaction terms between peer income and two dummies representing these attributes. In all re-estimations, the two interaction terms are jointly insignificant.

Our third approach to assessing the potential relevance of unobserved factors for peer income is to take into account the entire set of peer characteristics asked about in the survey. To do so, we re-estimate the baseline models for formal loans (see Tables 3 and 4) including as additional covariates the average age, education, household size, and employment status of the social circle. In all cases, the estimated effects on peer income in terms of magnitude, sign, and significance remained unchanged, while the additional social circle characteristics prove mostly statistically insignificant.

Lastly, we check the sensitivity of our findings to an income measure that is less volatile to temporary idiosyncratic shocks and local time trends than the current household income used in our baseline specifications. We follow Kapteyn, Alessie, and Lusardi (2005), who apply a standard lifecycle permanent income hypothesis model to DNBHS data,<sup>30</sup> and we estimate a measure of permanent income for each household that represents the discounted present value of its future lifetime resources. More specifically, we regress noncapital income on an age spline, the interactions between age and education, gender, and family size while controlling for household fixed effects and then compute a measure of household permanent income by deriving predicted expected incomes at different ages for

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<sup>30</sup> We are grateful to Rob Alessie for providing the code to calculate the permanent household income.

each household.<sup>31</sup> We use this measure (instead of current household income) to re-estimate the regression models presented in Section 4, but our results remain unaffected.

## **6. Investigating the Nature of the Peer Income Effects**

It is plausible to suppose that the effects of perceived social circle income on loan behavior depend on whether the individual's own income is above or below that perceived level. In other words, we would expect that people who perceive themselves as poorer than their peers tend to be more responsive to changes in peer income than those who feel richer. We allow for such asymmetry by replacing peer income in our baseline models (Tables 2, 3, and 4) with two terms denoting positive and negative differences between respondent and peer income. The results are given in Table 6. For respondents who are poorer than they perceive their acquaintances to be, an assumed increase in their social circle's annual income of 12,000 euro (which raises the income gap relative to their peers), increases the probability of obtaining a collateralized loan by 3.5 pp and a consumer loan by 1 pp. In fact, it is only the effects for those who perceive themselves as poorer than their social circle that are statistically significant, whether with respect to participation or to conditional amounts.<sup>32</sup>

Our results therefore suggest that acquaintances' income and how it compares to the household's own income tend to influence borrowing, not only from friends and family but also from the financial sector. This increased tendency to obtain consumer loans and make them larger conditional on their provision is presumably aimed at boosting consumer spending. The corresponding tendency for collateralized loans, in contrast, stems from efforts to acquire collateral assets of higher value. We thus look for evidence that at least part of the

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<sup>31</sup> As in Kapteyn et al. (2005), we assume a constant interest rate of 3% and a life expectancy of 80 years (which roughly corresponds to the average life expectancy in the Netherlands between 2000 and 2010).

<sup>32</sup> The only exception is the likelihood of future borrowing from the social circle, suggesting that households consider such a possibility even when their own income exceeds that of their peers and more so when their peers become richer, which leads to narrowing of the income gap.

peer income effect comes from a comparison with peers' ability to spend, either on consumer goods or on collateral assets.

To do so, we use responses to a direct survey question on whether respondents see their acquaintances as having "more money to spend" than they do, coded on a seven-point ordinal scale from "strongly disagree" to "strongly agree". This reference to "money to spend" invites respondents to consider not only income but also the basic inelastic expenditure needs of their acquaintances (e.g., household size). For our part, this focus on others' spending ability allows assessment of whether the intensity of such a perception has an independent influence on borrowing behavior. We thus add an ordinal variable denoting agreement that "acquaintances have more money to spend than I" to our baseline specification for (uncollateralized) consumer loans. As is clear from Table 7 (panel B), in all cases, the marginal effects are positive and statistically significant, both for participation and for conditional amounts.

In the case of collateral assets, such as primary residence, acquaintances' ability to spend is indicated by the size and other observable attributes of the house owned. Our data set includes objective information on respondents' homes, including the size (in square meters) of the living room in the main residence. Not only is this size readily observable to most of the social circle, it is likely to reflect the household's standard of living and priorities in enjoying life. Our data, however, provide no direct information on the respondents' perceptions of the social circle's living arrangements. Therefore, for each respondent's social circle, we compute an average of living room square meters by age/education cells. We also take into account the square meters of the respondent's living room to be sure of estimating effects that are net of the respondent's own living standards. As Table 7 (panel B) shows, once the size of the respondent's own living room is controlled for, the constructed variable on the social circle's average living room size is positive and statistically significant at 5%,

both in the participation (probit) regression and in the conditional size of collateral loan (tobit) regression.

Overall, these results using proxies for direct comparison with the spending or living standards of peers support the hypothesis that such comparison partially influences the tendency of households to have collateralized and uncollateralized loans outstanding, without making perceived peers' income insignificant.

Probing further into the nature of the comparison effect of average peer income, we also examine whether part of the effect is linked to a “tunnel effect”; that is, likely to arise because higher peer income signals the potential for one's own higher income in the future. To this end, we consider two alternative measures. First, we take into account respondents' reported expectations about minimum possible income in the next year. Second, we control for a measure of permanent income that represents discounted lifetime resources (see Table 7).<sup>33</sup> Both measures imply a positive and statistically significant effect in the specifications modeling collateralized debt, thereby supporting the presence of a “tunnel effect” for this type of loan. Nonetheless, the fact that including additional controls results in somewhat smaller but still significant marginal effects of perceived average peer income on formal borrowing suggests that the “tunnel effect” does not fully explain the influence of peer income. The remaining effects may well reflect alternative considerations, like envy or concern about status, that are not fully captured by the comparison effect proxies included in our regressions.

Finally, to examine the extent to which the perceived income of peers is associated with measures of overindebtedness, we regress loan-to-value ratios, as well as debt-service ratios, on the perceived average income of peers and on the rich array of socioeconomic covariates used in our baseline specifications. The average marginal effects from the tobit

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<sup>33</sup> The construction of the measure of permanent income is discussed in Section 5. Estimated marginal effects on permanent income refer to changes net of current household income, which enters as an independent control in the specification.



regressions, outlined in Table 8, imply that an assumed 12,000 euro increase in the perceived annual income of the peers contributes 1.2 pp to an average loan-to-value ratio of 18% and 0.3 pp to an average debt-service ratio of 6%.<sup>34</sup> The endogeneity tests (bottom of the table) show no evidence of endogenous peer income in either equation ( $p$ -values  $> .5$ ). These findings suggest that the social interaction effect uncovered is not confined to an individual's own borrowing behavior but is also likely to have implications for financial distress. For instance, in the country and time period considered, there was an upward trend in housing prices and relatively stable labor market conditions. Nonetheless, factors like the perceived income of peers, which induce additional borrowing during times of expansion, could well become key determinants of distress during recessions. Indeed, if such reversals are present, it may well be worthwhile to extend the logic of “Minsky moments” to household borrowing.

## 7. Concluding Remarks

As discussed in the introduction, although social influences can have quite distinct implications for consumption versus borrowing, extant research tends to focus on identifying the social effects for the former. In this paper, in contrast, we use unique information from the DNB's household survey, which is representative of the entire Dutch population, to assess the effects of social interactions on the tendency to take on different types of debt and the size of loans conditional on obtaining them. Most particularly, we exploit respondents' directly elicited perceptions of the average income in their social circle and the ability of their peers to spend, thereby circumventing the need to construct a hypothesized social circle based on arbitrary assumptions about its members' characteristics.

We find that the higher the perceived income of the social circle, the greater the tendency of respondents to have outstanding loans and in sizeable amounts. This finding

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<sup>34</sup> Calculations are based on a median peer income of 33,000 euro.

holds true for both collateralized loans and uncollateralized consumer loans once such factors as household resources, reception of financial advice from the social circle, and belief in future borrowing from its members are controlled for. The effect is stronger for those who see themselves as having lower income than their social circle.

We also find that, once the perceived average income of the social circle is controlled for, the tendency of households to take out uncollateralized and collateralized loans is partly related to the perceived spending ability or (computed) housing assets of social circle members. Moreover, a household's expectations about the next period's (minimum) income are statistically significant for collateralized loans—suggesting a “tunnel effect”—but do not render peers' perceived income insignificant. This finding is consistent with the assumption that borrowing behavior is influenced by peer income not only because it conveys information about the respondent's own future but also because of a comparison or envy effect. Nor is the role of comparison confined to the tendency to borrow and the outstanding borrowing amount: it also seemingly extends to financial distress.

Overall, our study reveals a clear potential for social influences on borrowing. Most particularly, after observing that others have higher average incomes, a household not only tries (as earlier studies show) to emulate their spending but also decides to borrow more—and only partly because of expectations of higher future income. Such decisions are likely to be encouraged by a massive and unprecedented housing boom associated with high collateral values and expectations of continuing house price trends. Hence, our finding that social comparison matters for debt behavior once fundamental household characteristics and region-time trends are controlled for suggests that there is a need to decouple perceptions of relative standing from any decisions to borrow without proper accounting of the associated risks. This provides additional motivation and focus for efforts to promote debt literacy and unbiased financial advice.

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## Appendix A. Definitions of Variables and Summary Statistics

### Types of Debt:

*Ability to borrow from social circle.* Yes to “Are you currently in a position to borrow a substantial sum of money from family or friends?” (LENEN=1)

*Loans from social circle:* Loans from family and friends.

*Collateralized loans:* Debts on hire-purchase contracts; debts based on payment by installment; equity based loans; debts with mail-order firms; shops or other retail business; mortgages on main house, second house and other pieces of real estate.

*Outstanding uncollateralized debt:* Private loans; extended lines of credit (unrelated to home equity); study loans; credit card debts; other loans.

### Questions on characteristics of the social circle:

The following questions concern your circle of acquaintances, that is, the people with whom you associate frequently, such as friends, neighbors, acquaintances, or maybe people at work.

KENLTD. If you think of your circle of acquaintances, into which age category do MOST of these people go? Please select the answer that is closest to reality. Age (in years) is mostly: under 16; 16 – 20; 21 - 25; 26 - 30; 31 - 35; 36 - 40; 41 - 45; 46 - 50; 51 - 55; 56 - 60; 61 - 65; 66 - 70; 71 or over.

KENHH. The people in your circle of acquaintances may live alone or share a household with other people (for example with a partner and children). Of how many persons do MOST households of your acquaintances consist? one person; two persons; three persons; four persons; five persons; six persons or more.

KENINK. How much do you think is the AVERAGE total net income per year of those households? less than € 8,000 per year; € 8,000 – 9,500; € 9,500 – 11,000; € 11,000 – 13,000; € 13,000 – 16,000; € 16,000 – 20,000; € 20,000 – 28,000; € 28,000 – 38,000; € 38,000 – 50,000; € 50,000 – 75,000; € 75,000 or more; don’t know.

KENOPL. Which level of education do MOST of your acquaintances have? primary education; junior vocational training; lower secondary education; secondary education/pre-university education; senior vocational training; vocational colleges/first year university education; university education.

KENWERK. What kind of employment do MOST of your acquaintances have? self-employed; practicing a free profession; working in the family business; employed on a contractual basis; mostly no paid job.

MANUUR (VROUWUUR). If you think of the MEN (WOMEN) among your acquaintances, how many hours per week do they work on average?

**Other questions:**

*Get financial advice from friends.* When answering “parents, friends or acquaintances” to the following question: “What is your most important source of advice when you have to make important financial decisions for the household?” (ADVIES=1).

*Social circle has more money to spend than I.* “Other people in my environment have more money to spend than I. Please indicate to what extent you agree or disagree” (SITUAT3: 1.totally disagree...7.totally agree).

*Last year income: unusually low.* “Is the income your household earned in the past 12 months unusually high or low compared to the income you would expect in a ‘regular’ year, or is it regular?” (INKNORM= 1.“Unusually low”).

*Perceived lower bound on next period’s income.* “What do you expect to be the LOWEST total net monthly income your household may realize in the next 12 months? (HOOG).

**Table A1. Summary statistics: various demographics**

<b>Variable</b>	<b>Average</b>	<b>Std. Dev.</b>	<b>Number of Observations</b>
Age	48.23	15.41	14,893
Male	0.53	0.50	14,892
Couple	0.64	0.48	14,893
Number of Children	0.64	1.03	14,893
<i>Education dummies:</i>			
Less than high school	0.27	0.44	14,815
High School	0.34	0.48	14,815
College Degree	0.38	0.48	14,815
Other Education	0.01	0.11	14,815
<i>Labour market status dummies:</i>			
Unemployed	0.02	0.14	14,889
Employed	0.54	0.50	14,889
Self employed	0.04	0.20	14,889
Retired	0.17	0.37	14,889
Other status	0.23	0.42	14,889
Last year income: unusually low	0.07	0.25	11,342
Health poor/ fair	0.28	0.45	11,791
Ability to borrow from soc. circle	0.28	0.45	8,782
Get financial advice from friends	0.34	0.47	11,454
Soc. circle has more money to spend than I	3.85	1.47	8,939
Own living room sq. meters	35.98	25.26	12,013
Avg living room sq. meters of friends	36.54	3.37	14,892
Loan-to-value ratio	0.18	0.30	13,081
Debt servicing ratio	0.06	0.15	10,215

*Note:* Weighted statistics from waves 2001-2008 of DNBHS data.



**Table A2. Summary statistics: various economic indicators**

<b>Variable</b>	<b>Average</b>	<b>Std. Dev.</b>	<b>25th perc</b>	<b>Median</b>	<b>75th perc</b>	<b>Number of obs</b>
Avg. peer income	31,807	13,955	24,000	33,000	36,941	6,872
Net hh income	27,617	23,638	15,943	24,687	35,886	10,031
Net financial wealth	36,137	100,092	1,393	10,847	36,430	11,412
Net real wealth	102,417	179,408	0	11,913	163,576	13,245
Perceived lower bound on next period's income	17,500	36,683	2,134	14,434	26,387	11,049

*Note:* Weighted statistics from waves 2001-2008 of DNBHS data. Amounts refer to constant 2008 euro.

## Appendix B. Calculation of average marginal effects via Monte Carlo simulation

Given that marginal effects are non-linear functions of the estimated parameters,  $\hat{\beta}$  (either from probit or tobit models), we compute their point estimates and standard errors via Monte Carlo simulation (Train, 2003) by using the formula:

$$E(g(\beta)) = \int g(\beta) f(\beta) d\beta$$

where  $g(\beta)$  denotes the magnitude of interest and  $f(\beta)$  the joint distribution of all the elements in  $\beta$ . We implement this simulation estimator by drawing 500 times from the joint distribution of the estimated vector of parameters  $\hat{\beta}$  under the assumption that it is asymptotically normal with mean and variance-covariance matrix equal to the maximum likelihood estimates. Then, for a given parameter draw  $j$  we generate the magnitude of interest  $g(\hat{\beta}^j)$ . We first calculate this magnitude for each household in our sample, and then calculate the average marginal effect as the weighted average of the effect across all households in our sample, using survey weights.<sup>35</sup> We then estimate  $E(g(\beta))$  and its standard error as the mean and standard deviation, respectively, of the distribution of  $g(\hat{\beta}^j)$  over all parameter draws. Details on the formulae used to derive unconditional and conditional marginal effects after the tobit estimation can be found in Green (2000, Chapter 22).

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<sup>35</sup> We do not evaluate marginal effects at sample means since this practice can lead to severely misleading results (see Train, 2003, pp. 33-34).

## Appendix C. Further Robustness Checks

We performed a number of checks in addition to those presented in the main text in order to ensure the robustness of our findings. First, about 20% of households answer “don’t know” to the question regarding the perceived average income of their peers and thus they are not used in our baseline regressions. To examine the sensitivity of our findings to the inclusion of these missing observations we have re-estimated all our baseline models presented in Tables 2, 3, and 4 and add a flag dummy to denote households answering that they do not know the income of their peers. For these observations, missing incomes of the peers are replaced by zeros. Estimated average marginal effects and associated standard errors for the income of the peers from this larger sample of households are presented in Table C.1. Notably, the estimated magnitudes across all specifications are very similar to those we estimate in our baseline models.

Second, we experimented with different specifications that employ quartiles to model the income of the peers and our results are robust to such transformations. Our results are also insensitive to functional forms that use quartiles to model own income and/ or own financial and real wealth.

Third, our modeling strategy of borrowing behavior is quite standard in the household finance literature and in line with life-cycle portfolio models in which households decide every period on the allocation of their resources and the amount of borrowing. Yet, one may argue that for many households with collateralized loans outstanding in a given period, the decision to take up such loans (especially mortgages) was made many years prior to the interview. To examine the sensitivity of our results to this issue, we have re-estimated our probit model for collateralized loans focusing only on households that take up such loans (i.e., switch borrowing status) during the period covered by our data. Specifically, we use the sample of households without collateralized loans in 2001 (i.e., the initial observation period in our sample) and estimate the probability of taking up such a loan in any of the subsequent seven waves. This probit model conditions on the same set of covariates as the one used in our baseline specification (presented in Table 3). The estimated marginal effect on the income of the peers is 2 pp, significant at 1%, and contributes almost 20% to the unconditional probability of taking up a collateralized loan in this sample. Thus, estimated effects on the

income of the peers from this ‘inflow’ sample are economically important and relatively stronger than those in our baseline specification.

Fourth, one might argue that the estimated effects of income of the peers on collateralized loans are partly due to expectations about future housing market conditions. To investigate this issue, we estimated specifications of collateralized debt behavior that take into account, apart from peers’ income and expectations about next year’s own income, various expectations regarding future conditions in housing and mortgage markets. These include whether respondents expect housing prices to go up, whether they anticipate an increase in mortgage interest rates, and whether they think that tax deductibility of mortgage interest rates will be limited in the future. Results (available upon request) suggest a significant negative relationship between an expected increase in mortgage interest rates and collateralized debt, but they do not affect our baseline findings regarding the significant role of income of peers or of expectations about next year’s own income (shown in Table 7, panel A).

**Table C.1. Effects of Peer Income (taking into account ‘do not know’ responses)**

<b>Panel A. Loans from Social Circle</b>					
	<b>Pr(perceived ability to borrow from social circle in</b>		<b>Pr(Loans from social circle&gt;0)</b>		<b>E(log(Loans from soc. circ.)) Loans from soc. circ.&gt;0</b>
	<b>Marg. Eff. std. error</b>		<b>Marg. Eff. std. error</b>		<b>Marg. Eff. std. error</b>
IHS(avg. peer income)	0.0256	0.0081 ***	0.0085	0.0039 **	0.2735 0.1110 **
Log likelihood	-3,372.8		-1,016.7		-1,860.1
Number of Observations	6,375		7,213		7,405
<b>Panel B. Collateralized Loans</b>					
	<b>Pr(Collateralized Loans&gt;0)</b>			<b>E(log(Colat. Loans)) Colat. Loans&gt;0</b>	
	<b>Marg. Eff. std. error</b>			<b>Marg. Eff. std. error</b>	
IHS(avg. peer income)	0.0402 0.0085 ***			0.4589 0.0890 ***	
Log likelihood	-3,759.7			-14,445.7	
Number of Observations	6,373			6,373	
<b>Panel C. Consumer Loans</b>					
	<b>Pr(Consumer Loans&gt;0)</b>			<b>E(log(Cons. Loans)) Cons. Loans&gt;0</b>	
	<b>Marg. Eff. std. error</b>			<b>Marg. Eff. std. error</b>	
IHS(avg. peer income)	0.0155 0.0055 ***			0.2274 0.0812 ***	
Log likelihood	-2,374.4			-7,647.0	
Number of Observations	6,373			6,373	

*Note:* Selected marginal effects from probit regressions modeling the probability of having a loan and marginal effects from tobit regressions on the log amount of the loan outstanding conditional on having such loan. Reported marginal effects are based on a 12,000 euro annual increase of peer income. Specifications in panels A, B, and C condition on the same set of covariates used in the baseline specifications in Tables 2, 3, and 4, respectively, and a flag dummy denoting households that answer ‘do not know’ to the peer income question. Standard errors are corrected for heteroscedasticity and clustered at the household level. \*\*\*, \*\*, \* denote significance at 1%, 5% and 10% respectively.

**Table C.2. Collateralized Loans (controlling for various province-specific housing indicators)**

<b>Panel A. Probit: Prob(Collateralized Loans)&gt;0</b>										
	<b>(1)</b>		<b>(2)</b>		<b>(3)</b>		<b>(4)</b>			
	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>
<b>IHS(avg. peer income)</b>	0.0414	0.0090 ***	0.0429	0.0082 ***	0.0428	0.0089 ***	0.0428	0.0083 ***		
Socio-economic controls	yes		yes		yes		yes			
Province dummies	yes		yes		yes		yes			
Year dummies	no		yes		yes		yes			
Quadratic time trend x province dummies	yes		no		no		no			
Housing prices by province (yearly growth rate)	-		0.0093	0.0110	-		-			
Stock of houses by province (yearly growth rate)	-		-		0.0062	0.0569	-			
Home ownership rates by province (yearly growth rate)	-		-		-		0.0001	0.0020		
Log likelihood	-2,672.2		-2,681.7		-2,682.0		-2,682.0			
Number of Observations	4,523		4,523		4,523		4,523			
<b>Panel B. Tobit: E(log(Colat. Loans)) Colat. Loans&gt;0</b>										
	<b>(1)</b>		<b>(2)</b>		<b>(3)</b>		<b>(4)</b>			
	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>
<b>IHS(avg. peer income)</b>	0.4692	0.0895 ***	0.4808	0.0845 ***	0.4799	0.0886 ***	0.4798	0.0843 ***		
Socio-economic controls	yes		yes		yes		yes			
Province dummies	yes		yes		yes		yes			
Year dummies	no		yes		yes		yes			
Quadratic time trend x province dummies	yes		no		no		no			
Housing prices by province (yearly growth rate)	-		0.0984	0.1219	-		-			
Stock of houses by province (yearly growth rate)	-		-		0.1558	0.6087	-			
Home ownership rates by province (yearly growth rate)	-		-		-		-0.0021	0.0215		
Log likelihood	-10,401.5		-10,409.9		-10,410.2		-10,410.2			
Number of Observations	4,523		4,523		4,523		4,523			

*Note:* Housing indicators are drawn from the Construction & Housing theme tables section of the Statline database of Statistics Netherlands (Centraal Bureau voor de Statistiek, <http://statline.cbs.nl/StatWeb>). House prices have 2005 as base year.

**Table D.1: Auxiliary Regressions**

	(1)			(2)			(3)		
Age	0.0212	0.0041 ***		0.0212	0.0039 ***		0.0218	0.0041 ***	
Age^2	-0.0002	0.0000 ***		-0.0002	0.0000 ***		-0.0002	0.0000 ***	
Male	-0.0150	0.0195		-0.0246	0.0184		-0.0166	0.0194	
Couple	0.1808	0.0198 ***		0.1812	0.0191 ***		0.1794	0.0197 ***	
Numb of Children	0.0112	0.0082		0.0137	0.0079 *		0.0115	0.0083	
High School Education	0.2401	0.0245 ***		0.2433	0.0234 ***		0.2375	0.0246 ***	
College Degree	0.4194	0.0303 ***		0.4217	0.0288 ***		0.4155	0.0305 ***	
Employed	0.1145	0.0261 ***		0.1268	0.0247 ***		0.1148	0.0262 ***	
Self employed	0.1096	0.0575 *		0.1099	0.0541 **		0.1081	0.0575 *	
Retired	0.0636	0.0310 **		0.0684	0.0286 **		0.0651	0.0312 **	
Unemployed	0.1202	0.0602 **		0.1128	0.0590 *		0.1231	0.0605 **	
Last year income: unusually low	-0.1270	0.0336 ***		-0.1220	0.0318 ***		-0.1246	0.0335 ***	
Health poor/ fair	-0.0294	0.0171 *		-0.0227	0.0161		-0.0280	0.0171	
IHS(net hh income)	0.0144	0.0029 ***		0.0124	0.0026 ***		0.0142	0.0029 ***	
IHS(net fin wealth)	0.0030	0.0011 ***		0.0027	0.0011 **		0.0028	0.0011 **	
IHS(net real wealth)	0.0069	0.0017 ***		0.0073	0.0016 ***		0.0068	0.0017 ***	
Percv. ability to borrow from social circle in the future	-			-			0.0292	0.0160 *	
Get advice from soc. circle	-			-			-0.0040	0.0154	
avg. peer Education	0.0182	0.0182		0.0195	0.0171		0.0176	0.0182	
Province dummies	yes			yes			yes		
Year dummies	yes			yes			yes		
Province x Year dummies	yes			yes			yes		
Constant	9.5153	0.1229 ***		9.5488	0.1156 ***		9.4976	0.1253 ***	
(avg. peer Educat. - own Educat.)*Regional empl. % in high tech	0.0364	0.0031 ***		0.0374	0.0029 ***		0.0362	0.0031 ***	
F-statistic - instruments ( <i>p-value</i> )	140.42	0.00 ***		169.49	0.00 ***		138.99	0.00 ***	
Number of Observations	4,363			4,899			4,362		

**Table 1. Prevalence and Amount of Borrowing by Loan type**

<b>Panel A. Loans from Social Circle</b>						
<b>Year</b>	<b>Perceived ability to borrow from social circle in the future (%)</b>	<b>Prevalence (%)</b>	<b>Conditional amounts outstanding</b>			
			<b>Average</b>	<b>25th perc</b>	<b>Median</b>	<b>75th perc</b>
2001	30.45%	4.87%	15,212	1,583	2,771	15,832
2002	32.24%	4.96%	13,582	2,279	5,065	12,662
2003	29.68%	4.26%	12,010	1,391	3,241	15,689
2004	25.80%	3.92%	10,207	1,058	3,704	10,783
2005	28.12%	4.47%	7,976	1,098	2,196	7,320
2006	27.55%	3.73%	7,650	1,439	3,085	7,197
2007	25.99%	3.72%	8,488	1,829	3,810	7,112
2008	28.10%	3.49%	9,422	1,500	3,000	7,900
<b>Total</b>	<b>28.31%</b>	<b>4.16%</b>	<b>10,638</b>	<b>1,519</b>	<b>3,313</b>	<b>10,282</b>

  

<b>Panel B. Collateralized Loans</b>						
	<b>Prevalence (%)</b>	<b>Conditional amounts outstanding</b>				
		<b>Average</b>	<b>25th perc</b>	<b>Median</b>	<b>75th perc</b>	
2001	37.81%	105,038	44,857	83,118	131,934	
2002	43.22%	113,177	45,760	89,288	139,512	
2003	43.12%	113,921	44,298	90,757	146,940	
2004	40.96%	110,673	46,562	92,065	145,405	
2005	41.25%	118,971	51,238	100,384	156,851	
2006	40.69%	117,246	49,353	100,763	159,370	
2007	41.02%	132,048	59,944	111,760	181,864	
2008	40.92%	132,920	61,750	120,000	180,000	
<b>Total</b>	<b>41.15%</b>	<b>117,926</b>	<b>48,620</b>	<b>98,293</b>	<b>156,664</b>	

  

<b>Panel C. Consumer Loans</b>						
	<b>Prevalence (%)</b>	<b>Conditional amounts outstanding</b>				
		<b>Average</b>	<b>25th perc</b>	<b>Median</b>	<b>75th perc</b>	
2001	22.24%	11,451	956	4,486	11,610	
2002	24.62%	9,448	843	4,659	12,344	
2003	25.86%	13,030	918	4,415	13,487	
2004	25.09%	11,315	835	4,021	11,794	
2005	19.13%	14,957	1,045	4,273	12,548	
2006	18.64%	11,267	853	4,138	12,287	
2007	20.57%	11,196	889	3,835	11,379	
2008	20.33%	12,008	680	3,750	11,206	
<b>Total</b>	<b>22.09%</b>	<b>11,793</b>	<b>875</b>	<b>4,181</b>	<b>12,155</b>	

*Note:* Weighted statistics from waves 2001-2008 of DNBHS data. Amounts refer to constant 2008 euro.



**Table 2. Loans from Social Circle**

	Probit				Tobit			
	Pr(perceived ability to borrow from social circle in the future>0)		Pr(Loans from social circle>0)		E(log(Loans from soc. circle)) Loans from soc. circle>0			
	Marg. Eff.	std. error	Marg. Eff.	std. error	Marg. Eff.	std. error		
IHS(avg. peer income)	0.0217	0.0076 ***	0.0079	0.0042 *	0.2541	0.1270 **		
Age	-0.0086	0.0010 ***	-0.0016	0.0005 ***	-0.0524	0.0170 ***		
Male	0.0477	0.0216 **	0.0047	0.0087	0.1585	0.2699		
Couple	0.0332	0.0221	-0.0097	0.0112	-0.2908	0.3355		
Numb of Children	-0.0040	0.0110	-0.0018	0.0056	-0.0667	0.1480		
High School Education	0.0706	0.0259 ***	0.0285	0.0130 **	0.8595	0.3478 **		
College Degree	0.0776	0.0267 ***	0.0085	0.0116	0.2969	0.3586		
Other Education	0.2444	0.1153 **	0.0000	0.0000	0.0000	0.0000		
Employed	-0.0014	0.0291	0.0149	0.0120	0.4489	0.3276		
Self employed	0.0445	0.0553	0.0493	0.0327	1.3611	0.6905 **		
Retired	-0.0562	0.0339 *	-0.0020	0.0158	-0.0817	0.4823		
Unemployed	-0.1146	0.0518 **	-0.0057	0.0210	-0.2748	0.6369		
Last year income: unusually low	-0.0852	0.0307 ***	0.0346	0.0200 *	0.9122	0.4351 **		
Health poor/ fair	-0.0531	0.0195 ***	-0.0030	0.0096	-0.1130	0.2955		
IHS(net hh income)	0.0010	0.0042	0.0006	0.0019	0.0170	0.0571		
IHS(net fin wealth)	0.0225	0.0063 ***	-0.0147	0.0033 ***	-0.6809	0.1243 ***		
IHS(net real wealth)	0.0034	0.0070	0.0061	0.0038	0.1862	0.1140		
Province dummies		yes		yes		yes		
Year dummies		yes		yes		yes		
Province x Year dummies		yes		yes		yes		
Log likelihood	-2,493.3		-786.3		-1,468.3			
Number of Observations	4,524		4,899		5,074			
	<b>IV tests</b>							
F-statistic from Auxilliary Regression ( <i>p-value</i> )	140.42	0.00 ***	169.49	0.00 ***	169.49	0.00 ***		
Exogeneity Test ( <i>p-value</i> )	2.23	0.14	0.08	0.78	0.09	0.76		
Number of Observations	4,363		4,899		4,899			

*Note:* Marginal effects from probit regressions modeling the probability of having an outstanding loan from friends and marginal effects from tobit regressions on the log amount of loan outstanding conditional on having such loan. All marginal effects have been averaged across households in the sample using survey weights. The marginal effects for peer income are based on a 12,000 euro annual increase of the underlying variable. The marginal effects for household income, financial, and real wealth are calculated assuming a one standard deviation increase of the underlying covariates. Standard errors are corrected for heteroscedasticity and clustered at the household level. \*\*\*, \*\*, \* denote significance at 1%, 5% and 10% respectively.

**Table 3. Collateralized Loans**

	Probit		Tobit	
	Pr(Collateralized Loans>0)		E(log(Colat. Loans) Colat. Loans>0)	
	Marg. Eff.	std. error	Marg. Eff.	std. error
IHS(avg. peer income)	0.0417	0.0081 ***	0.4750	0.0842 ***
Age	-0.0016	0.0011	-0.0285	0.0127 **
Male	0.0021	0.0242	0.0200	0.2996
Couple	0.1782	0.0275 ***	1.9488	0.2634 ***
Numb of Children	0.0143	0.0118	0.1214	0.1273
High School Education	0.0213	0.0298	0.2275	0.3236
College Degree	0.0685	0.0295 **	0.8108	0.3411 **
Other Education	0.0325	0.1448	0.5109	1.8565
Employed	0.0806	0.0334 **	0.8909	0.3696 **
Self employed	0.0863	0.0553	1.0572	0.6306 *
Retired	0.0287	0.0386	0.4851	0.4462
Unemployed	0.0171	0.0692	0.1932	0.8432
Last year income: unusually low	-0.0983	0.0351 ***	-1.3074	0.3927 ***
Health poor/ fair	-0.0110	0.0214	-0.2003	0.2362
IHS(net hh income)	0.0142	0.0039 ***	0.1444	0.0389 ***
IHS(net fin wealth)	0.0043	0.0069	0.0690	0.0650
IHS(net real wealth)	0.0725	0.0094 ***	0.4008	0.0551 ***
Perceived ability to borrow from soc. circle in the future	0.0054	0.0196	-0.0299	0.2213
Get advice from soc. circle	-0.0304	0.0206	-0.4084	0.2144 *
Province dummies		yes		yes
Year dummies		yes		yes
Province x Year dummies		yes		yes
Log likelihood	-2,651.1		-10,381.9	
Number of Observations	4,523		4,523	
	<b>IV tests</b>			
F-statistic from Auxilliary Regression ( <i>p-value</i> )	138.99	0.00 ***	138.99	0.00 ***
Exogeneity Test ( <i>p-value</i> )	1.72	0.19	1.37	0.24
Number of Observations	4,362		4,362	

*Note:* Marginal effects from probit regressions modeling the probability of having an outstanding collateralized loan and marginal effects from tobit regressions on the log amount of collateralized loan outstanding conditional on having such loan. See notes in Table 2.

**Table 4. Consumer Loans**

	Probit		Tobit	
	Pr(Consumer Loans>0)		E(log(Cons. Loans) Cons. Loans>0)	
	Marg. Eff.	std. error	Marg. Eff.	std. error
IHS(avg. peer income)	0.0157	0.0055 ***	0.2422	0.0796 ***
Age	-0.0020	0.0007 ***	-0.0273	0.0088 ***
Male	0.0090	0.0139	0.2316	0.2033
Couple	0.0549	0.0154 ***	0.6820	0.2216 ***
Numb of Children	0.0013	0.0073	0.0352	0.0989
High School Education	0.0220	0.0184	0.2427	0.2661
College Degree	0.0169	0.0180	0.1149	0.2662
Other Education	0.1151	0.0923	0.9607	1.0431
Employed	0.0190	0.0208	0.1022	0.2515
Self employed	0.0337	0.0335	0.1990	0.4650
Retired	0.0016	0.0259	-0.2109	0.4032
Unemployed	0.0592	0.0529	0.6828	0.6059
Last year income: unusually low	0.0706	0.0294 **	0.8688	0.3181 ***
Health poor/ fair	0.0221	0.0140	0.2659	0.1884
IHS(net hh income)	0.0037	0.0029	0.0808	0.0485 *
IHS(net fin wealth)	-0.1969	0.0041 ***	-3.5256	0.0534 ***
IHS(net real wealth)	0.0008	0.0054	-0.0006	0.0933
Perceived ability to borrow from soc. circle in the future	-0.0103	0.0125	-0.1614	0.1800
Get advice from soc. circle	-0.0260	0.0130 **	-0.3613	0.1786 **
Province dummies		yes		yes
Year dummies		yes		yes
Province x Year dummies		yes		yes
Log likelihood	-1,652.3		-5,405.3	
Number of Observations	4,513		4,523	
	<b>IV tests</b>			
F-statistic from Auxilliary Regression ( <i>p-value</i> )	135.67	0.00 ***	139.26	0.00 ***
Exogeneity Test ( <i>p-value</i> )	0.04	0.85	0.12	0.73
Number of Observations	4,273		4,346	

*Note:* Marginal effects from probit regressions modeling the probability of having a consumer loan and marginal effects from tobit regressions on the log amount of consumer loan outstanding conditional on having such loan. See notes in Table 2.

**Table 5. Placebo Regressions**

<b>Panel A. Collateralized Loans</b>				
	<b>Pr(Collateralized Loans&gt;0)</b>		<b>E(log(Colat. Loans)) Colat. Loans&gt;0</b>	
	<b>Marg. Eff. std. error</b>		<b>Marg. Eff. std. error</b>	
IHS(avg. peer income)	0.0051	0.0063	0.0556	0.0787
<b>Panel B. Consumer Loans</b>				
	<b>Pr(Consumer Loans&gt;0)</b>		<b>E(log(Cons. Loans)) Cons. Loans&gt;0</b>	
	<b>Marg. Eff. std. error</b>		<b>Marg. Eff. std. error</b>	
IHS(avg. peer income)	0.0007	0.0046	0.0055	0.0501

*Note:* Selected marginal effects from probit regressions modeling the probability of having a formal loan and marginal effects from tobit regressions on the log amount of the loan outstanding conditional on having such loan. Peer income is that of a randomly assigned household belonging to the same year, age, education, gender cell as the respondent's social circle. The marginal effects for peer income are based on a 12,000 euro annual increase of the underlying variable. Specifications in panel A (panel B) condition on the same set of covariates used in the baseline specifications in Table 3 (Table 4). Standard errors are corrected for heteroscedasticity and clustered at the household level. \*\*\*,\*\*,\* denote significance at 1%, 5% and 10% respectively.

**Table 6. Asymmetric Effects of Peer Income**

<b>Panel A. Loans from Social Circle</b>							
	<b>Pr(perceived ability to borrow from social circle in the future&gt;0)</b>				<b>E(log(Loans from social circle)) Loans from social circle&gt;0</b>		
	<b>Marg. Eff.</b>	<b>std. error</b>		<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>
IHS(own income)-IHS(avg. peer income)>0	0.0117	0.0051 **		0.0006	0.0024	0.0133	0.0700
IHS(own income)-IHS(avg. peer income)<0	0.0104	0.0042 **		0.0060	0.0025 **	0.2009	0.0746 ***
<b>Panel B. Collateralized Loans</b>							
	<b>Pr(Collateralized Loans&gt;0)</b>				<b>E(log(Colat. Loans)) Colat. Loans&gt;0</b>		
	<b>Marg. Eff.</b>	<b>std. error</b>		<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>
IHS(own income)-IHS(avg. peer income)>0				-0.0059	0.0060	-0.0261	0.0657
IHS(own income)-IHS(avg. peer income)<0				0.0354	0.0051 ***	0.3116	0.0424 ***
<b>Panel C. Consumer Loans</b>							
	<b>Pr(Consumer Loans&gt;0)</b>				<b>E(log(Cons. Loans)) Cons. Loans&gt;0</b>		
	<b>Marg. Eff.</b>	<b>std. error</b>		<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>
IHS(own income)-IHS(avg. peer income)>0				0.0034	0.0037	0.0483	0.0498
IHS(own income)-IHS(avg. peer income)<0				0.0104	0.0031 ***	0.1696	0.0490 ***

*Note:* Selected marginal effects from probit regressions modeling the probability of having a loan and marginal effects from tobit regressions on the log amount of the loan outstanding conditional on having such loan. Presented marginal effects are based on a 12,000 euro annual increase of peer income. Specifications in panels A, B, and C condition on the same set of covariates used in the baseline specifications in Tables 2, 3, and 4, respectively. Standard errors are corrected for heteroscedasticity and clustered at the household level. \*\*\*, \*\*, \* denote significance at 1%, 5% and 10% respectively.

**Table 7. Living Standards, Perceived Ability of the Social Circle to Spend, and Future Own Income**

<b>Panel A. Collateralized Loans</b>									
	<b>Probit</b>				<b>Tobit</b>				
	<b>Prob(Collateralized Loans&gt;0)</b>				<b>E(log(Colat. Loans) Colat. Loans&gt;0)</b>				
	<b>Marg. Eff.</b>	<b>std. error</b>		<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>
<b>IHS(avg. peer income)</b>	0.0328	0.0086 ***		0.0225	0.0092 **	0.3920	0.0887 ***	0.2877	0.0944 ***
<b>Avg sq. meters of friends</b>	0.0207	0.0105 **		0.0279	0.0108 ***	0.2764	0.1245 **	0.3691	0.1314 ***
<b>Own sq. meters</b>	0.0421	0.0163 ***		0.0324	0.0149 **	0.3750	0.1372 ***	0.2926	0.1226 **
<b>IHS(perceived lower bound on next period's income)</b>	0.0434	0.0124 ***		-	-	0.4707	0.1334 ***	-	-
<b>IHS(estimated permanent income)</b>	-	-		0.0384	0.0185 **	-	-	0.6096	0.2982 **
Log likelihood	-2,407.9			-2,228.3		-9,851.7		-9,182.5	
Number of Observations	4,204			3,964		4,206		3,964	
<b>Panel B. Consumer Loans</b>									
	<b>Probit</b>				<b>Tobit</b>				
	<b>Prob(Consumer Loans&gt;0)</b>				<b>E(log(Cons. Loans) Cons. Loans&gt;0)</b>				
	<b>Marg. Eff.</b>	<b>std. error</b>		<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>	<b>Marg. Eff.</b>	<b>std. error</b>
<b>IHS(avg. peer income)</b>	0.0128	0.0058 **		0.0127	0.0064 **	0.1920	0.0859 **	0.1956	0.0833 **
<b>Soc. circle has more money to spend than I</b>	0.0125	0.0048 ***		0.0135	0.0049 ***	0.1873	0.0618 ***	0.1958	0.0675 ***
<b>IHS(perceived lower bound on next period's income)</b>	-0.0007	0.0086		-	-	0.0218	0.1308	-	-
<b>IHS(estimated permanent income)</b>	-	-		0.0086	0.0210	-	-	0.0926	0.2417
Log likelihood	-1,329.8			-1,248.2		-4,413.0		-4,128.5	
Number of Observations	3,662			3,440		3,669		3,457	

*Note:* Selected marginal effects from probit regressions modeling the probability of having a formal loan and marginal effects from tobit regressions on the log amount of the loan outstanding conditional on having such loan. The marginal effects for peer income are based on a 12,000 euro annual increase of the underlying variable. The marginal effects for average sq. meters of friends, own square meters, expected lower bound on next period's income, and estimated permanent income are calculated assuming a one SD increase of the underlying covariates. Specifications in panel A (panel B) also condition on the set of covariates used in the baseline specifications in Table 3 (Table 4). Standard errors are corrected for heteroscedasticity and clustered at the household level. \*\*\*, \*\*, \* denote significance at 1%, 5% and 10% respectively.

**Table 8. Peer Income and Over-indebtedness**

	Tobit		Tobit	
	E(Loan to Value Ratio)		E(Debt Servicing Ratio)	
	Marg. Eff.	std. error	Marg. Eff.	std. error
IHS(avg. peer income)	0.0353	0.0058 ***	0.0093	0.0032 ***
Age	-0.0040	0.0007 ***	-0.0006	0.0005
Male	-0.0047	0.0163	0.0118	0.0105
Couple	0.1149	0.0177 ***	0.0295	0.0113 ***
Numb of Children	0.0101	0.0080	0.0005	0.0046
High School Education	0.0003	0.0191	0.0198	0.0109 *
College Degree	0.0408	0.0192 **	0.0314	0.0116 ***
Other Education	0.0729	0.1201	0.0505	0.0786
Employed	0.0712	0.0206 ***	0.0346	0.0153 **
Self employed	0.0879	0.0382 **	0.0200	0.0232
Retired	0.0464	0.0256 *	0.0185	0.0166
Unemployed	0.0176	0.0477	0.0286	0.0308
Last year income: unusually low	-0.0943	0.0222 ***	-0.0493	0.0123 ***
Health poor/ fair	-0.0175	0.0147	-0.0095	0.0084
IHS(net hh income)	0.0107	0.0030 ***	0.0184	0.0017 ***
IHS(net fin wealth)	0.0118	0.0052 **	0.0017	0.0026
IHS(net real wealth)	-0.0031	0.0070	0.0242	0.0041 ***
Perceived ability to borrow from soc. circle in the future	-0.0123	0.0131	-0.0045	0.0080
Get advice from soc. circle	-0.0318	0.0129 **	-0.0090	0.0083
Province dummies		yes		yes
Year dummies		yes		yes
Province x Year dummies		yes		yes
Log likelihood	-3,115.0		-1,759.4	
Number of Observations	4,504		3,722	
	<b>IV tests</b>			
F-statistic from Auxilliary Regression ( <i>p-value</i> )	139.05	0.00 ***	126.52	0.00 ***
Exogeneity Test ( <i>p-value</i> )	0.43	0.51	0.62	0.43
Number of Observations	4,331		3,453	

*Note:* Marginal effects from tobit regressions on measures of financial distress. All marginal effects have been averaged across households in the sample using survey weights. The marginal effects for peer income are based on a 12,000 euro annual increase of the underlying variable. The marginal effects for household income, financial, and real wealth are calculated assuming a one standard deviation increase of the underlying covariates. Standard errors are corrected for heteroscedasticity and clustered at the household level. \*\*\*, \*\*, \* denote significance at 1%, 5% and 10% respectively.