Peer Effects and Debt Accumulation: Heterogeneity and Consequences for Households' Financial Vulnerability*

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Abstract

I study the effect of lottery winnings on peers' debt accumulation using Norwegian administrative data. My analysis indicates that neighbors accumulate debt to finance increased spending. On average, neighbors of lottery winners increase debt by 2.6 percent of the lottery prize. Analyzing heterogeneity, I find greater effects among households residing in single-household dwellings, with children, and with little cash on hand. Furthermore, households with characteristics associated with higher financial literacy increase debt more than others. After peer treatment, neighbors have a higher net interest rate exposure and their expenditure responses to income losses are significantly stronger than in pretreatment years.

JEL Classification: D14, D31, D91, E21, G51

Keywords: household finance, peer effects, debt accumulation, income shocks, network homophily, financial literacy

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1 Introduction

In light of recent evidence that growth in household debt is correlated with the onset and depth of financial crises, it seems important to understand household borrowing itself (Jordà, Schularick, and Taylor (2013) and Mian, Sufi, and Verner (2017)). Yet even though the timing and level of debt accumulation is always a household-level decision, the drivers of household debt at the micro level are empirically less explored and understood. A growing strand in the empirical literature points to a role for "social finance" in households' financial decisions (Kuchler and Stroebel (2021)), that is, how peers and social networks affect households' financial behavior. Not only can peer effects help understand dynamics and fluctuations in individual household behavior, but peer effects may also be important for how shocks are transmitted throughout the economy. A hypothesis is that peer effects act as a "social multiplier" (Glaeser, Sacerdote, and Scheinkman (2003)). The empirical literature indeed suggests that peer effects can influence household consumption (Agarwal, Qian, and Zou (2021), Kuhn, Kooreman, Soetevent, and Kapteyn (2011), De Giorgi, Frederiksen, and Pistaferri (2020), Bertrand and Morse (2016)), stock market participation (Kaustia and Knüpfer (2012)), housing and mortgage leverage choice (Bailey, Cao, Kuchler, and Stroebel (2018), Bailey, Dávila, Kuchler, and Stroebel (2019)) and personal bankruptcy risk (Agarwal, Mikhed, and Scholnick (2020), Roth (2020)). Survey evidence in Georgarakos, Haliassos, and Pasini (2014) shows that the perceived income rank relative to peers affects borrowing, and Kalda (2019) documents that peers' financial distress affects household leverage.

In this paper, I ask: Do income shocks that hit one household affect debt growth among neighbors? If so, which factors influence the size of these peer effects? And finally: can peer effects make households more vulnerable to fluctuations in interest rates and income? I provide causal estimates based on household-level registry data and an exogenous instrument, lottery income, that sidestep the econometric difficulties in estimating peer effects (Manski (1993)).

The contribution of my paper is threefold. First, with detailed, household-level data that allow me to control for individual, time-varying characteristics as well as household fixed effects, I add to the existing literature that links peer effects and debt. The existing literature has previously linked peer effects and debt (Agarwal et al. (2020), Georgarakos et al. (2014)). But, whereas Agarwal et al. (2020) focuses on bankruptcy filers which comprise roughly one percent of the population, my study show that peer effects and debt accumulation exist also as a wider phenomenon. I also investigate the effects on income, deposits, and expenditure. The results suggest that peers' expenditure affects household expenditure and that households are willing to finance such expenditure with more debt (Agarwal et al. (2021), Kuhn et al. (2011)). Second, I provide a novel analysis of various factors influencing peer effects. As highlighted by Kuchler and Stroebel (2021), most empirical research on peer effects has focused on broad groups of peers, such as workplace peers (e.g., De Giorgi et al. (2020) or neighborhoods (e.g., Agarwal et al. (2020)). I show that factors along several dimensions that plausibly reflect stronger social ties between the winner and a neighbor consistently produce higher (though not always statistically significant) point estimates. Third, and finally, I provide new evidence on the longer-term consequences of peer effects on households' finances. In addition to increasing the risk of defaults and personal bankruptcies, one key concern among policymakers and macroprudential authorities is that high debt makes households less resilient to economic shocks and reduces their ability to smooth consumption. I show that households become more financially vulnerable due to peer effects: In years after treatment, households are more exposed to interest rate fluctuations, and their expenditure is more sensitive to large drops in income.

My empirical strategy is to use lottery prizes won by one household and analyze if neighbors' debt responds to the shock. The advantage of using lottery prizes in this setting is that prizes are pure transitory income shocks affecting only one neighborhood household. Neighbors are therefore only indirectly affected through observing the winners' shock or behavioral response to it (Kuchler and Stroebel (2021)).[1] My data source is de-identified administrative data on balance sheets (income, wealth, and debt), individual characteristics (age, household size, number of children, and education), and addresses of all tax-paying Norwegians over the period 1994–2015. The main variable of interest, debt, consists of all household debt, including mortgages. With these data, I construct a sample of one-time lottery winners and their neighbors. I then run regressions with the lottery prize of the winner as the treatment variable and neighbors' debt as the response variable.

The key identifying assumption in this strategy is that selection into treatment is conditionally random. That is, I assume *the timing and intensity of treatment* are random for households that live on streets with only one lottery winner, after controlling for household fixed effects and time fixed effects, and time-varying covariates. The main challenge of this approach is that we do not observe the number of lottery tickets or the total amount gambled among neighbors of the winner. Therefore, I restrict attention to streets with one winner only throughout the sample period from 1994 to 2015. My analyses show no pretreatment responses, and observables do not predict treatment timing or intensity (prize size). Thus, I give the regression estimates a causal interpretation as peer effects that drive up debt.

The analyses in my paper benefit from a combination of observational household-level panel data and a credible identification scheme. This combination allows me to analyze the existence, determinants, and some broader consequences of peer effects. Data are third-party reported and rich both with respect to the time dimension and in terms of individual characteristics and household balance sheets. In addition to corroborating existing evidence on peer effect and debt accumulation with credible causal estimates (e.g., Agarwal et al. (2020), Georgarakos et al. (2014)), the contribution to the literature is in several dimensions.

The baseline regression uses a sample of lottery prizes ranging from NOK 10 000 to NOK 1 000 000 ($\approx$ USD 1100 to USD 111 000) over the period from 1994 to 2006 (hereafter "the small-prize sample"),[2] and estimate the debt effect among neighbors living up to ten houses from the winner. I later refer to this as a "sphere of influence" equal to 10. These results show a statistically significant debt response that, on average, amounts to a 2.6 percent increase in debt, measured in terms of

[1] In this paper, I think of the lottery prizes as a transitory income shock. The lottery prize may also be a wealth shock, as in Cesarini, Lindqvist, Notowidigdo, and Ostling (2017). For the purpose of this paper, this distinction is less relevant.

[2] After 2006, lottery prizes below 100 000 are no longer available. Therefore, my analysis focuses mainly on this period in order to use prizes below 100 000, which increases the number of observations and the variation in the treatment variable. However, I also show results for the entire sample period from 1994 to 2015, but in this case, only for prizes exceeding NOK 100 000. I call this sample "the big-prize sample"

the lottery prize (e.g., for a lottery prize of NOK 10 000, neighbors on average increase debt by NOK 260). A non-linear model suggests a decreasing effect with the prize size, with a 6.6 percent effect for the smallest prizes. Using a discrete treatment variable that weights all prizes equally, the average krone increase in debt is estimated as being NOK 6 100, with a 95 percent confidence interval ranging from NOK 4 500 to NOK 7 700. Estimates using the whole sample period up to 2015, and prizes exceeding NOK 100 000 (hereafter "the big-prize sample"), show smaller average linear effects, consistent with the finding that the response decreases in the prize size.

Lottery winners spend a large share of their prize within the same year as winning. In my sample, I estimate this spending response to be approximately 45 percent of the amount won.[3] Using this estimate, I can compute the neighbors' debt response as a share of the winners' spending response. This share is 6.2 percent for the small-prize sample and 6.3 for the big-prize sample. Given the existing estimates of how winners' spending responses decrease with prize size (see Fagereng et al. (2021)), my estimates imply that the neighbors' debt response is approximately linear in winners' spending response. Next, I estimate the dynamic responses with the same baseline regression but lead and lags of the debt response (treatment effect). The results show no signs of any pretreatment responses and that debt levels due to peer effects are persistent: Debt levels among neighbors stay higher than pretreatment debt levels for up to five years after the peer won a lottery prize.

Next, I estimate the effect of the lottery shock on neighbors' income, liquid assets, and imputed expenditures.[4] The estimated responses of income and liquid assets are approximately zero and not statistically significant. On the other hand, the expenditure is positive and significant for two years after the lottery win of a neighbor. The point estimates suggest an accumulated three-year expenditure response of 4.0 percent, measured in terms of the lottery prize. Thus expenditure is close to the total added debt over the same period (3.5 percent). Thus, it seems that neighbors take on debt to finance increased spending.

I extend the baseline analysis and investigate how debt responses vary with observable characteristics of the winners' neighborhoods and the neighboring households. This heterogeneity analysis serves two purposes. First and foremost, it provides a novel analysis of the determinants of peer effects within a network defined by geography. Second, my identification strategy rests on the assumption that neighbors can observe the winning households' (extra) expenditure. When I narrow down and focus on neighbors more likely to observe the winner's expenditure, I find that peer effects are stronger whenever a household is more likely to observe the winner's expenditure. Thus, this analysis supports the claim that peer effects cause debt accumulation. I find that: (1) debt re-

[3] Fagereng, Holm, and Natvik (2021) find an average expenditure response of 52 percent of the lottery prize. The reason for the discrepancy with my estimate is the sample of lottery prizes. Whereas Fagereng et al. (2021) condition only on single-winning households in the sample, I condition on single-winning *streets*. This condition considerably reduces the number and fraction of winners of small prizes in my sample, which Fagereng et al. (2021) find to have a larger marginal propensity to consume (MPC).

[4] Baker, Kueng, Meyer, and Pagel (2021) document that the economic significance of imputed consumption errors is minor for most individuals and not a concern for most research questions. Furthermore, they show that, even for wealthier individuals with extensive stock holdings, the bias can be minimized with standard methodologies.

sponses are smaller and statistically insignificant among neighbors with a relatively short tenure in the neighborhood and stronger and statistically significant among neighbors with a longer tenure; (2) neighbors with a household structure similar to that of the winner tend to increase their debt by more than neighbors with a household structure different from that of the winner; (3) there is a tendency for stronger peer effects among neighbors living in single-household dwellings than for neighbors living in multiple-household dwellings (i.e., apartment buildings). I also measure the distance from the winner and estimate larger effects for the closest neighbors, although the differences are not statistically significant. The differences are even more prominent (yet still not significant) when focusing only on neighborhoods consisting of houses. All in all, even though the estimated differences are not always statistically significant, the complete set of results suggests that stronger social ties, or structures that lay the basis for stronger social ties, induce more substantial peer effects, just as the literature on social networks predicts (McPherson, Smith-Lovin, and Cook (2001) and Sudman (1988)).[5]

At the individual level, having children living in the household, higher income, and a below-median level of bank deposits increase a household's debt response. Furthermore, indicators of a higher level of financial literacy – a higher education level and stock market participation – do not reduce the estimated debt effect. On the contrary, households with a high level of education and stock market participants increase their debt by more following a lottery win in their streets, compared with households with a low level of education and non-participants.

In a final exercise, I investigate whether households become more financially vulnerable in years after the lottery shock of their neighbor and their subsequent debt accumulation. I find that treated households become more sensitive to interest rate fluctuations as their measured net interest rate exposure, net interest expenses, and debt-to-income increase. I also estimate that their expenditures become more sensitive to income drops after treatment. That is, faced with a significant income loss (40 percent in earned income) in the year after a neighbor wins the lottery, households reduce spending by more than they would have done absent the peer effects. Moreover, this negative effect for households losing income is more persistent than the added expenditure effect of treated households that do not experience a drop in income.

My empirical strategy of using lottery prizes to study peer effects is not unique. Among the closest papers to this one are Agarwal et al. (2020) Kuhn et al. (2011), which both use lottery prizes to investigate neighborhood peer effects. Kuhn et al. (2011) use data from the Dutch Postcode Lottery and survey data on consumption to study how income shocks affect winners' and their neighbors' consumption and happiness. A key finding is that neighbors of winners increase consumption and are more likely to own a new car in the years after their neighbor wins in a lottery. Agarwal et al. (2020) find that in neighborhoods with lottery winners, households increase borrowing, visible consumption, and the risk of bankruptcy increases among the winners' neighbors.[6]

[5] The results on the variation in debt responses echo some of the empirical findings in the existing peer-effect literature on consumption, such as the effects of distance found in Kuhn et al. (2011), and of tenure found in De Giorgi et al. (2020).

[6] Using lottery prizes to study various household outcomes of the winners themselves is by now well-established in the literature, with Imbens, Rubin, and Sacerdote (2001) as an early key contribution. Cesarini

Beyond identification through lottery windfalls, both Georgarakos et al. (2014) and Kalda (2019) study how peer effects might influence households' debt decisions. Georgarakos et al. (2014) use individual survey data and find that lower perceived income relative to one's social reference group co-varies with increased borrowing and a higher debt service ratio. With individual credit data, Kalda (2019) studies peer effects after adverse health shocks that cause financial distress. The main result is that financial distress among peers leads to persistent deleveraging and lower debt levels because individuals borrow less and pay down more on existing debt. Relatedly, Agarwal et al. (2021) find that same-building neighbors of households that experience personal bankruptcy reduce consumption.

Another related literature is the one that studies consumption peer effects even if, as pointed out by Georgarakos et al. (2014), a peer effect that affects debt need not reflect a peer effect through consumption. Rayo and Becker (2006) provide a model with one simple mechanism linking conspicuous consumption and borrowing. Social status is linked to visible goods, which are also costly, durable goods. Thus, for economic agents that want to smooth consumption, status-driven consumption leads to more borrowing or less saving. A long strand of empirical literature has sought to find evidence of social image as a determinant of consumption, particularly visible consumption.[7] Bertrand and Morse (2016) find evidence of "trickle-down consumption," i.e., that poorer households spend more on visible goods if exposed to higher top-income levels, with the implication that they save less than comparable households in other regions do. Aiming at understanding mechanisms driving peer effects, Bursztyn, Ederer, Ferman, and Yuchtman (2014) conduct a field experiment and find evidence that both social learning (i.e., learning about the value of an asset through peers' purchases of the asset) and social utility (i.e., the utility from owning an asset increases with peers' possession of the same asset) affect investment decisions. Finally, with identification through "friends-of-friends" networks, De Giorgi et al. (2020) build on work by Bramoullé, Djebbari, and Fortin (2009) and De Giorgi, Pellizzari, and Redaelli (2010) to study consumption network effects. With Danish household-level data and household members' workplaces as the social network, they find small but significant network effects in consumption and show that their implied government spending multiplier depends on the policy's targeted sections (poor/rich).[8] The Danish data also allow them to look into the heterogeneity of peer effects. They find that peer effects vary with education, share of women in the workplace, economic conditions, and tenure in the workplace.[9]

Finally, this paper also speaks to a broader literature seeking to understand the rise in household

et al. (2017) investigate the effect of lottery prizes on labor supply, and Fagereng et al. (2021) investigate the marginal propensity to consume. Hankins, Hoekstra, and Skiba (2011) find that winners of small and big prizes are equally likely to file for bankruptcy, and Olafsson and Pagel (2019) look at how small windfalls increase the borrowing of winners.

[7]See Bursztyn and Jensen (2017) for a review of field experiment evidence.

[8]They show that with a policy targeted toward the rich, the aggregate multiplier effects are smaller because richer households have fewer connections.

[9]In addition to consumption, some papers study the effect of relative income on well-being. For instance, survey data in Luttmer (2005) show an inverse relationship between people's self-reported happiness levels and their neighbors' earnings. This effect is stronger when the neighbors share common characteristics and have more frequent contact. See also references therein.

debt over the past three to four decades, and its consequences for household vulnerability, financial stability and monetary policy (e.g., Dynan, Mian, and Pence (2012), Andersen, Duus, and Jensen (2016), Holm, Paul, and Tischbirek (2021), Flodén, Kilström, Sigurdsson, and Vestman (2021), Baker (2018)). Papers by Jordà et al. (2013), Mian et al. (2017) and Mian, Rao, and Sufi (2013) have highlighted the importance of understanding the drivers and determinants of household debt growth by establishing that household debt levels and debt growth have been triggers and determinants of the severity of financial crises. My paper contributes to this literature by adding empirical evidence for a behavioral dimension to debt growth that is economically significant. In addition, my analyses of the longer-term consequences of peer effects on household vulnerabilities are, to my knowledge, unique. Furthermore, the rise in inequality and private debt over the past few decades has raised the question of whether they are causally linked and, if so, what the mechanism is. One candidate mechanism is peer effects, namely that poorer households seek to "keep up" with the wealthier households' increasing level of consumption. Several papers have investigated this link. Coibion, Gorodnichenko, Kudlyak, and Mondragon (2020) find that debt is lower among low-income groups in high-inequality areas than among their counterparts in low-inequality areas, and therefore argue that inequality does not increase debt levels. However, with similar data, Bertrand and Morse (2016) reaches a starkly different conclusion, namely that non-rich households exposed to higher top incomes consume a larger share (and save less) of their income. In Drechsel-Grau and Greimel (2018), this mechanism is critical in explaining how increasing income inequality can lead to increasing household debt. In their model, rising income among the top ten percent of the income distribution fuels a spiral of house improvements, starting with the wealthy households and spreading to the non-rich households that seek to "keep up with the Joneses." Finally, with Swedish register data, Roth (2020) finds a positive relationship between higher top incomes and insolvency. My micro-level estimates lend support to the conclusion that there is indeed a causal link between inequality and debt, but that it does not necessarily rely on increasing *top income shares* to be economically significant. Instead, my findings suggest that the social distance and social similarity between peers matters for how income hikes in one group trigger debt responses in another. Furthermore, my individual-level analysis suggests that higher-income households are more prone to peer effects and that (indicators of) higher financial literacy and socioeconomic status increase the debt response.

The paper is structured as follows. In Section 2 I describe my empirical strategy to identify causal peer effects on debt in further detail. Section 3 presents the data and the sample selection, and Section 4 presents the various econometric specifications. Baseline results are presented in Section 5.1, and results on the heterogeneity in peer effects are reported in Section 5.2. Finally, Section 5.3 present evidence on the consequences of peer effects for household vulnerability and Section 6 concludes.

2 Empirical strategy

Identifying peer effects is an econometric challenge, and the fundamental problem is self-selection. Because agents self-select into networks, it is not possible to separate peer effects from other sources of co-movement in behavior by regressing individuals' outcomes on their peers' outcomes. In his seminal paper, Manski (1993) pointed out three sources of co-movement among agents in a network: (1) *causal peer effects*, (2) *correlation in context and environment*, and (3) *correlated behavior*. Causal peer effects mean that the behavior of an agent's peers influences that same agent's behavior. Correlation in context refers to the notion that behavior in networks co-moves because individuals in the same network are exposed to the same shocks. Finally, correlated behavior means that agents in the same network behave similarly merely because they tend to be alike.

The ambition in this paper is to investigate whether there exists a link between changes in income and debt accumulation among neighbors via peer effects. In this context, it is important to recognize that households do not choose neighborhoods and their neighbors randomly. A simple example illustrates the problem with neighborhood peer effects. The econometrician observes a sudden increase in new cars in a neighborhood. Did households buy new cars because their neighbors bought new cars, i.e., was there a peer effect? Possibly, but not necessarily. Because neighbors tend to be similar types, they might tend to buy cars according to the same observed or unobserved rule (e.g., whenever a new model of a car make is released on the market), irrespective of what they know or think about their neighbor's car. Or, they could be working in firms related to the same industry (e.g., the oil industry) that is experiencing a boom that brightens the economic outlook for many households in the network. Or, the central bank lowers the interest rate, and neighbors have a similar interest-rate exposure through their mortgage, which in turn is a function of the house prices in the neighborhood they chose to live in. Quite likely, the observed outcome is an interplay of all three mechanisms. A naive regression trying to estimate peer effects on car purchases, with the individuals' car purchases as the outcome variable and the neighbors' car purchases as a forcing variable, would bundle all the above-listed effects into one estimate. Importantly, even if the econometrician realizes these pitfalls, in most cases it is not possible to identify each of the three effects separately.

The empirical strategy in this paper aims to rule out correlation in context and correlated behavior as potential sources of households' debt decisions, and thereby leaves pure causal peer effects as the only explanatory mechanism. This strategy is carried out by using lottery prizes as income shocks that affect only one household in a neighborhood. In contrast to most papers that use lottery prizes as income shocks, and where the lottery winners themselves are the treated (e.g., as in Cesarini et al. (2017) or Fagereng et al. (2021)), the treated households in this paper are neighbors that live on the same street as a lottery winner — i.e., the lottery winner's peers. I implement regressions with household and time fixed effects on a sample of streets that have one winner only throughout the entire period from 1994 to 2015. Under a set of identifying assumptions discussed in detail below, I can attribute the systematic changes in neighbors' debt in the treatment year to the winners' income shocks (or the winners' behavioral responses to the shocks). Thus, I argue that neighbors' estimated debt responses are due to *causal peer effects*. Note that my estimated peer effects thus include neighbors' responses to winners' own behaviors after winning.

In general, the key identifying assumption in this empirical strategy is that selection into treatment and treatment intensity is conditionally random. In my setting, the treatment is that of being a neighbor of a winner in the year the winner wins, and treatment intensity is the amount won. Hence, the identifying assumption in the empirical analysis is that the timing and size of the lottery prize in streets with only a single winner are random for neighbors of the winner, after controlling for household and time fixed effects and time-varying covariates. My tests of random selection to treatment back up the validity of the identifying assumption (see below).

Three cases would constitute breaches of the identifying assumption. First, neighbors cannot have information beyond what we observe and control for that makes them able to predict the timing and size of the winners' prizes. Such unobservable information *could* produce pretreatment responses and bias in the treatment effect. Second, I assume that winners gamble individually so that a lottery prize affects the observed income of only the reported winner. If neighbors gamble in teams and share prizes between them, it would not be picked up in the data. Finally, I do not observe how many tickets each household buys. Hence, my approach assumes that the lottery prizes observed in my sample are not driven by some general increase in gambling debt among neighbors in the years around treatment.[10]

In my analysis, I restrict my sample to households living on streets with only one winner over the full 21-year period for which data are available. The purpose of this sample restriction is precisely to reduce the plausibility of the above cases to a minimum.[11] In addition, I scrutinize the validity of the identifying assumption by testing the predictive power of time-varying covariates on treatment and pretreatment responses in debt. The details on these results are presented in Section 4.2.

If the identifying assumptions hold, the lottery prizes are exogenous shocks that affect the income of only the winner on each street. By definition, the shocks therefore exclude correlated behavior and correlated context as sources of neighbors' estimated treatment responses, and the empirical strategy identifies a causal peer effect.

3 Data and sample

3.1 Norwegian household data

In the analysis I use de-identified administrative data on Norwegian individuals over the period 1994–2015. Financial data are third-party-reported (by employers, banks, or other financial institutions), and collected by the tax authority for tax purposes. These financial data include labor income (gross and net of tax), transfers, debt, and liquid (stocks, bonds, deposits) and illiquid

[10] It is useful to think about what bias breaches to the identifying assumptions would create. Heavy, debt-financed gambling in pretreatment years would produce a negative bias, since accumulation of debt in years leading up to treatment would make the relative increase in debt lower in the treatment year. Similarly, if the prize is shared among neighbors it would introduce a negative bias since it would increase neighbors' income (possibly in the form of unobserved cash) and therefore (all else equal) reduce incentives to borrow.

[11] See Section 3.2 for details on the sample.

wealth (housing, motor vehicles). Data are reported values on December 31 every year. The main variable of interest, debt, consists of all outstanding household debt. This includes mortgages, secured and unsecured debt, and credit card loans. The data also contain household identifiers so that the individual-level tax data can be aggregated to household-level balance sheets. Crucially, the tax data include lottery prizes. These are self-reported. However, households have a strong incentive to report lottery prizes because they are not taxable, and unreported lottery prizes that show up in higher wealth or lower debt might raise questions of tax fraud.

Data on lottery prizes include the sum of prizes won from Norsk Tipping (the Norwegian gaming monopoly). Norsk Tipping offers a number of betting activities, such as scratch cards, sports betting and bingo. Playing lotteries in Norway is not uncommon. According to Norsk Tipping, 60 percent of adult Norwegians (2.4 million) played in some game at least once during 2015. A drawback of the data is that we observe the amount won, but not how many times a household wins or the sum each household spends on betting. Data on lottery prizes smaller than NOK 100 000 are not available after 2006, and I focus on prizes above this threshold (see details below, in the paragraph *Prize sample*).

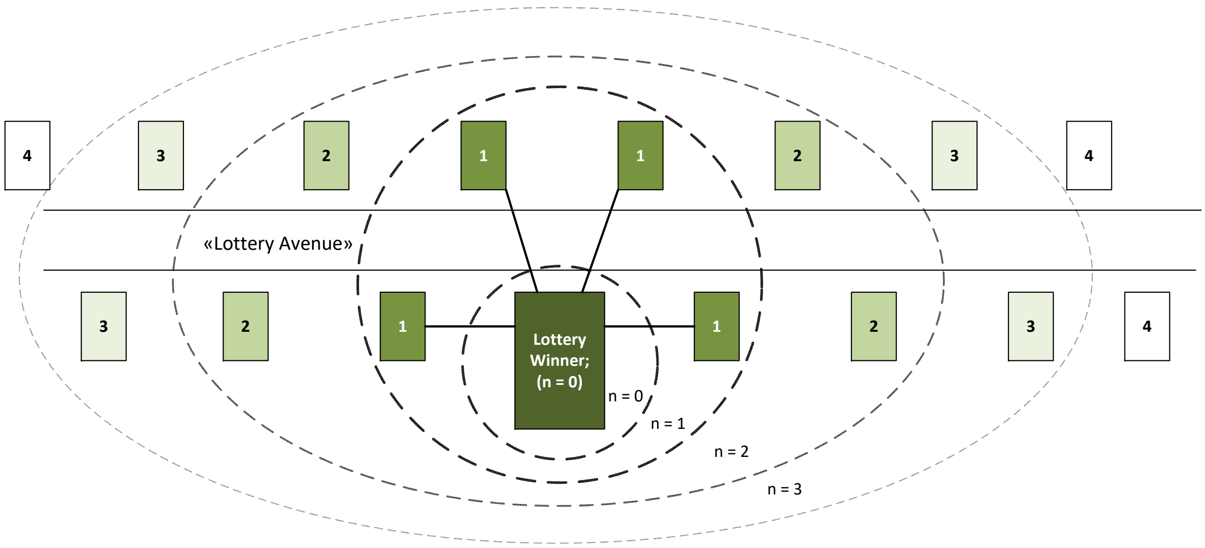
De-identified household addresses and characteristics are collected from the population register. As with individuals, streets have been given random but unique numbers. House numbers are as they appear on the map. Numbering of houses in Norway is standardized, with sequential odd numbers on one side of the street and sequential even numbers on the other. Thus, it is possible to infer which households live on the same street, and to rank the closeness between households residing on the same street by the number of houses between them. Further details on identification of neighbors by closeness is provided below (in the paragraph *Neighbor sample*).

3.2 Sample selection

Prize sample Due to changes in the Norwegian Tax Authority's reporting rules, lottery prizes below NOK 100 000 are not available after 2006. I therefore make two samples.[12] The small-prize sample includes prizes below NOK 100 000 over the period 1994–2006. Because individuals in this period were obliged to report prizes exceeding NOK 10 000, I set a lower threshold at 10 000. The big-prize sample spans the full time period 1994–2015, but only the prizes that exceed NOK 100 000 are included. In both samples, I draw an upper bound on prizes of NOK 1 million. As noted by Fagereng et al. (2021), when including very large prizes, linear estimates are mechanically pulled toward the effects at the top of the prize-size distribution. Thus, even if they are rare, big prizes will affect estimates of average responses disproportionately. For the same reasons, I prefer the small-prize sample as my main sample, although I do report results for the big-prize sample in the baseline results. Focusing on the small-prize sample increases both the number of observations and the variation in the treatment variable, which in turn allows for analyses that are more data demanding, such as an analysis of the determinants and heterogeneity in peer effects.

[12] Appendix Table A.2 and A.1 provide robustness tests for the sample selections described in this section. Also, see Section 5.1.2

Figure 1: An illustration of a street with a lottery winner and his sphere of influence



Street sample Streets have been assigned de-identified numbers, but it is possible to identify whether households live on the same streets or not. Specifically, I can identify streets with winners, and households that reside on those streets. To minimize the probability of breaches to the identifying assumption (e.g., that the street is a "gambling street" where many households play the lottery frequently and are therefore more likely to become winners), I include streets with one winner only throughout the entire time span 1994–2015 in the analysis. This approach is clearly very restrictive and possible only due to the rich data source containing the entire population of tax-paying Norwegians over 21 years of age.

Neighbor sample Figure 1 is an illustration of the empirical approach taken to define a winner's neighborhood, i.e., the network of peers. Figure 1 also illustrates how the distance between the winners and their neighbors is measured. The approach rests on the regularity of house numbering in Norway, where odd numbers are located on one side of the street and even numbers are located on the other side, without gaps. The figure illustrates a street ("Lottery Avenue") with a lottery winner (the biggest green box at the center) and his sphere of influence (drawn as dashed ellipses in the figure), meaning all neighbors *within* distance n. A sphere of influence equal to one (n = 1) refers to the four next-door buildings, i.e., one on each side of the winner's house, and two on the opposite side of the street. Widening the sphere of influence to two adds another set of four houses such that the total number of buildings expands to 8, and so on. If a box in Figure 1 is not a house, but a duplex, a townhouse or an apartment building, all households residing in that building are classified equally according to distance. The number of households within the same sphere of influence therefore varies across streets.

A distance equal to zero refers to the cases where the winning household resides in a building with more than one household. In most cases, these are apartment buildings, duplexes, townhouses or the like. However, for some households, there is uncertainty whether this is the case due to missing building codes in the data. Among the 18 130 observations in the small-prize sample living at distance equal to zero in the treatment year, 35 percent are buildings coded as duplexes,[13] townhouses or apartments. In such buildings, we can reasonably assume that households are in fact living in separate residences from the winner. For the remaining 65 percent, however, matters are unclear. Forty-three percent have an unknown building type (missing building code), and 22 percent are coded as single-household dwellings. The likelihood that a significant share of these households do live in the same residence as the winner and have a relation beyond being mere neighbors is high.[14] These observations will produce noise in the treatment variable, because a closer relationship, e.g., a family tie, implies a different treatment. Consequently, I exclude all neighbors living at distance equal to zero from the main specification.[15]

The baseline regression estimates debt responses with a sphere of influence equal to ten. The idea is to capture social interactions that are made independently of distance, without stretching the concept of a "neighborhood." If they exist (i.e., if the street is big enough), neighbors who live farther away than ten houses in either direction are not classified as treated neighbors. Beyond using the sphere of influence to distinguish treated from untreated, I use the sphere of influence variable to estimate the effect of distance (see details in Section 4.1).

Winsorizing extreme observations The final adjustment in my sample is winsorization on household income, household debt and household stocks and bonds values. The purpose is to reduce noise and spurious effects, which is particularly important in the analyses with fewer observations (such as when estimating dynamic responses (Section 5.1.1) or in estimations in subsamples (Section 5.2). Thus, I exclude households that in any one year are: (1) in the top one percent of the debt distribution, (2) in the top one percent of the stocks and bonds distribution, and/or (3) in the top or bottom one percent of the income distribution. Importantly, my baseline results are virtually unaffected by these sample restrictions.[16]

3.3 Descriptive statistics

Table 1 displays summary statistics in the small-prize sample on key household characteristics and balance-sheet variables for the treated group (i.e., the winners' neighbors) and, as a reference, a group consisting of households that live in the same postal code as the winners, but on different streets (hereafter, "the reference group"). For the neighbors, variables are measured in the year

[13] For simplicity, single-unit houses that have a letter attached to the house number are coded as duplexes.

[14] A direct family link, where the winner is either the mother or father of one of the neighboring households' members, is one specific example. In the data, this is the case for a total of only four households (30 observations) in the sample. They all live at the sphere of influence equal to zero, and are therefore excluded in my analysis.

[15] Including these neighbors in the sphere of influence equal to ten does not significantly affect the main estimates. See the Appendix, Table A.1 and Table A.2 for these results.

[16] Robustness results are presented in Section5.1.2, and results are reported in the Appendix, Table A.1 and Table A.2.

Table 1: Descriptive statistics the year before treatment: Neighbors and the reference group

	Neighbors mean	sd	median	Reference mean	sd	median
$Year_{t-1}$	2000	3.45	2000	1999	3.64	1999
Age_{t-1}	52	18.88	50	50	19.52	48
$Household\ size_{t-1}$	2	1.38	2	2	1.36	2
$Debt_{t-1}$	391 837	527 830	157 044	377 225	516 459	153 649
$Deposits_{t-1}$	185 747	332 747	64 819	169 876	323 968	53 177
$Net\ Income_{t-1}$	289 582	161 571	249 352	273 971	156 037	232 406
$Stocks\ and\ bonds_{t-1}$	37 328	127 830	0	34 116	125 225	0
Observations	186 455			1 372 039		

Notes: Descriptive statistics for households in the small-prize sample that includes prizes ranging from NOK 10 000 to NOK 1 000 000, and the years from 1994 to 2006. Neighbors are households that live on a street that has a single lottery winner over the period from 1994 to 2015. Reference are households that live in the same postal code as these winners but on different streets. Variables are measured the year before the winner on the street (or in the postal code) wins a lottery prize (i.e., $t-1$). In postal codes with more than one winner, one winner is chosen randomly to determine the treatment year. $Year$ reports the average year of the pretreatment year. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011. Age is the age of the oldest household member. $Household\ size$ is the number of household members, including adults and children. $Stocks\ and\ bonds$ is the sum of stocks, bonds, and mutual funds.

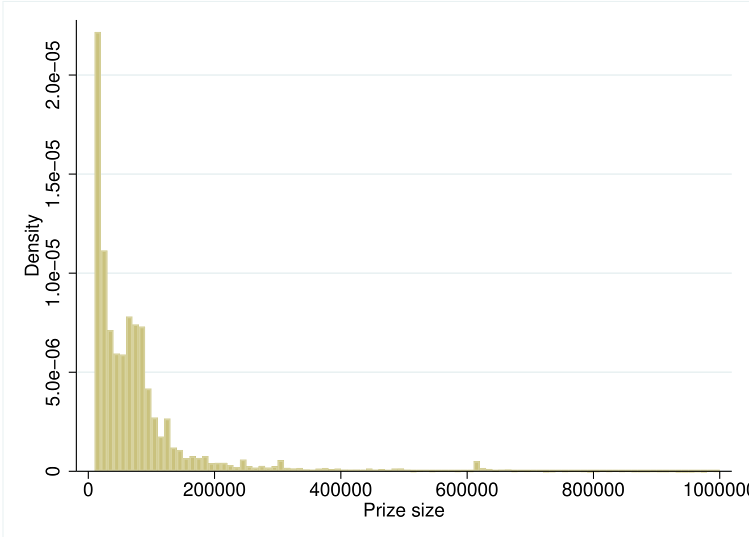
preceding the lottery win in their street. For the reference group, variables are measured in the year preceding the lottery prize in their postal code. In postal codes with multiple streets that win, one of the streets is chosen randomly to define that postal code's treatment year. The table addresses the issue of internal and external validity, and a key question is whether there are systematic differences between neighbors and the reference group. Table 1, however, shows that the neighbors and reference group are overall very similar on all key variables. The main difference between the two groups is with respect to age, measured as the age of the oldest individual in the household. Neighbors are on average two years older than the households in the reference group. Unsurprisingly, this difference translates into an overall bigger balance sheet with somewhat higher debt, liquid assets, and income. Differences are, however, small and can hardly be argued to pose any threat to the validity of the empirical analysis in the paper.

Figure 2 is a histogram of prizes among the winners in the small-prize sample. The unit of observation in this figure is the winners' prize amount, i.e., not the number of treated neighbors. Each bar has a width of NOK 10 000. The figure displays a left-skewed distribution with most prizes clustered below NOK 100 000.[17] Next, Figure 3 displays the number of lottery winners (measured on the y-axis in 3a) and the average lottery prize per year (measured on the y-axis in 3b) in the small-prize sample.[18] Figure 3a shows significantly fewer winners in the first part of the sample, apart from the outlier in 1996. This result is likely due to an increase in the number of new games created toward the second half of the 1990s. From 1998, there is a weak trend toward fewer winners. Partly, this is an artifact of the fact that krone values are reported in 2011 kroner,

[17] Appendix figure A.1 display the same figure for the big-prize sample

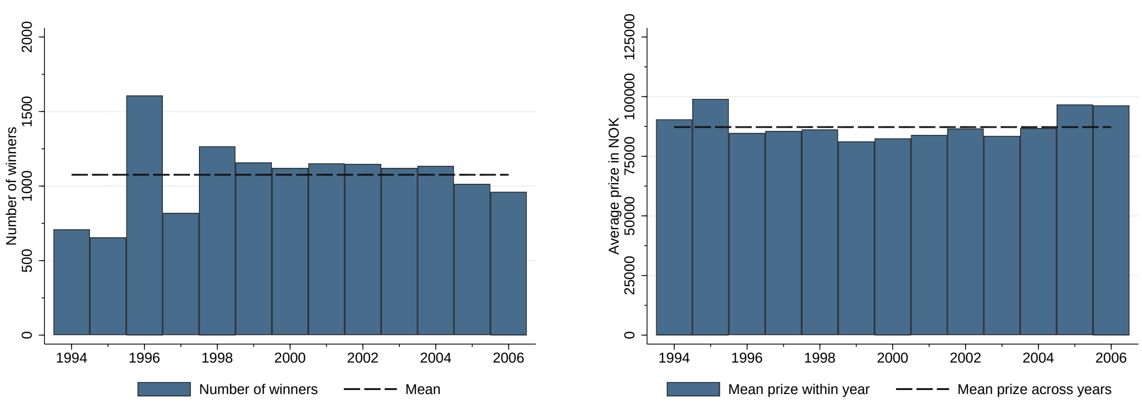
[18] In the Appendix, Figures A.2a and A.2b are the parallel figures for the big-prize sample.

Figure 2: Histogram of prizes among of winners in the small-prize sample



Notes: The figures display the density of the prize values in the small-prize sample. The width of bins is set to NOK 10 000. The small-prize sample includes prizes ranging from NOK 10 000 to NOK 1 000 000, and the years from 1994 to 2006. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

Figure 3: Number of winners and average lottery prize per year in the small-prize sample



(a) Number of winners.

(b) Average lottery prize

Notes: The figures display winners and prizes for the small-prize sample that includes prizes ranging from NOK 10 000 to NOK 1 000 000, and the years from 1994 to 2006. Bars in panel (a) display the total number of winners each year and bars in panel (b) display the average prize in NOK among these winners within each year, conditional on the prize being the only lottery prize in the lottery winner's street over the period 1994–2015. The dashed lines draw the mean value across all years. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

Table 2: Number of observations by distance to the winner in the treatment year

Distance (n)	Observations at n	Total observations within n	%	Cumulative %
Winners	.	13 866	.	.
0	18 130	18 130	7	7
1	44 630	62 760	17	24
2	34 588	97 348	13	37
3	28 293	125 641	11	48
4	23 298	148 939	9	56
5	19 213	168 152	7	64
6	15 923	184 075	6	70
7	13 397	197 472	5	75
8	11 221	208 693	4	79
9	9465	218 158	4	83
10	7924	226 082	3	86
11	6806	232 888	3	88
12	5935	238 823	2	90
13	5265	244 088	2	92
14	4515	248 603	2	94
15	3917	252 520	1	96
16	3551	256 071	1	97
17	3012	259 083	1	98
18	2766	261 849	1	99
19	2548	264 397	1	100
Total	264 397	100	100	

Notes: The table reports the number of observations (household-years) at each distance in the treatment year for the small-prize sample that includes prizes ranging from NOK 10 000 to NOK 1 000 000, and the years from 1994 to 2006. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011. Column 1 reports each distance. Distance refers to the number of houses between the winner and a neighboring household. Column 2 reports the number of observations at each distance, and Column 3 reports the number of observations within each distance (sphere of influence). Columns 4 and 5 report these numbers as the percentage of the total observations and the cumulative percentage in the treatment year, respectively. Row 1 reports the number of winners in the sample, which is equal to the number of streets in the sample. Distance equal to zero refers to households living at the same house number as the winner, typically an apartment building. Distance equal to one refers to the house next door.

such that a few prizes below 10 000 in nominal values are included in the sample, and more so the farther back in years we go.[19] Apart from this, there is clearly random variation each year, such that neither of the two observations poses any challenge to the analysis. The average number of winners each year is 1 076. Figure 3b displays the average prize of the winners in my sample. The figure shows that there is random variation in the average prize paid out. The average prize in all years is close to the cross-year average of NOK 87 232.

Lastly, Table 2 breaks down the number of neighbors by distance in the year that the street had a lottery winner. Column 2 contains the number of observations (neighbors/households) at each distance from the winner. Column 3 reports the total number of observations within each distance—what I have referred to as the sphere of influence. This is the cumulative sum of the numbers in Column 2. In addition, Column 3, Row 1 reports the number of winners in the small-prize sample. Columns 4 and 5 contain the percent of total observations in the treatment year and the cumulative percent as the sphere of influence is widened.

The total number of winners—equal to the number of treated streets—in the small-prize sample is 13 866. Next, Table 2 shows that the maximum number of households is at n equal to one (17 percent), i.e., in buildings next door to the winner. The reason for this peak at one is that many winners live in houses and consequently do not have any neighbors at n equal to zero, but all of them have at least one neighbor at n equal to one. After distance one, the number of additional neighbors added by moving one more step out from the winner drops as streets are limited in size.[20] The table further shows that 86 percent of the observations are within ten houses from the winner, underscoring that excluding neighbors beyond this point from my sample is a minor restriction.[21] Finally, we note that excluding neighbors at n equal to zero entails dropping seven percent of the observations.

4 Empirical approach

The baseline regression model is the following:

$$Debt_{ixt+h} = \beta_0 + \beta_1 \mathbf{X}_{it} + \gamma^h Lottery_{xt} + \alpha_i + \tau_t + e_{it} \tag{1}$$

where $Debt_{ixt+h}$ is the level of debt for household i, residing on street x in year $t+h$, where h is the horizon after treatment. I consider horizons from h equal to zero to h equal to five. As a first pass, I am interested in the contemporaneous response, i.e., with h equal to zero. β_0 is a time, and household-invariant constant, $\mathbf{X}$ is a vector of time-varying controls (see details below) and α_i and τ_t are household fixed effects and time fixed effects, respectively. In each of the streets, x, there is one (and only one) household that wins during the sample period from 1994 to 2015,

[19]The reason for the decreasing trend, more precisely, is that the distribution of prizes leans toward the small prizes, such that the number of prizes included due to krone adjustment is bigger than the number of prizes excluded at the top of the distribution.

[20]This number of extra neighbors added will also drop when the winner lives close to the end of the street.

[21]The robustness tests show that including households all the way to n equal to 20 as part of the treated neighbors does not significantly affect estimates. See the discussion in Section 5.1.2.

irrespective of whether the small- or big-prize sample is used. The lottery win in the street x is measured by $Lottery_{xt}$. It is equal to zero in all years preceding the lottery prize. The inclusion of a household (i.e., neighbor) in the treatment group is determined by the winners' sphere of influence as described in Section 3.2. The main regressions apply a sphere of influence equal to 10.

The main coefficient of interest is γ^h, interpreted as the debt response resulting from a *causal peer effect*: It is the neighbors' average debt response to the winners' income shock ($Lottery_{xt}$), measured as a share of the winners' prizes. Thus, a NOK 1 increase in the prize causes a NOK γ^h increase in debt at horizon h, with γ^0 yielding the contemporaneous debt response. Taken together, the set of coefficients γ^h (with h from zero to five) is the impulse response function for treated households. Because changes in the stock of debt today roll over to the stock of debt tomorrow (less the down payments), I exclude the post-treatment period in all regressions as in Fagereng et al. (2021). By doing so, I also avoid the problems of staggered treatment in two-way fixed-effects designs (i.e. using the early-treated group as control for later-treated group). Standard errors are always clustered at street level.

In addition to the continuous, linear $Lottery_t$ as treatment variable, I estimate peer effects with a discrete treatment variable equal to one in years where the winners win, and zero otherwise.[22] I also present results from a model where I add a second-order polynomial of the treatment variable, $Lottery_t^2$, to the right-hand-side variables. The former model yields the average krone amount of new debt among neighbors, independently of the prize size. A positive (negative) sign on $Lottery_t^2$ suggests that the debt response as a share of the initial prize increases (decreases) in the prize size. Fagereng et al. (2021) find that lottery winners' consumption share decreases with the amount won. A negative $Lottery_t^2$ would be consistent with this finding, assuming that the neighbors' responses monotonically increase in the winners' consumption response. On the other hand, if bigger prizes also mean expenditure that is more visible, such as status-enhancing purchases, we might expect an increasing peer effect of prize size (at least up to some point). A positive sign in the estimated coefficient on $Lottery_t^2$ would be consistent with such an effect.

Control Variables The set of time-varying controls in the vector $\mathbf{X}$ is the same for all models. These controls include the 1-year-lagged values of household size (i.e., number of adults and children), number of children under 18 in the household, a second-order polynomial on age and the education level of the oldest individual in the household. In order to capture large movements in debt associated with house purchases that would create noise in the estimates I add the contemporaneous and 2-year lags of a dummy variable equal to one in the year a household moves, and zero otherwise. Lastly, I control for 1-year-lagged values of household net income, bank deposits and cash, the sum of mutual funds, stocks and bonds, (estimated) housing wealth, total wealth and taxable gifts and inheritance received over the course of a year. The full set of control variables is included in all regressions, unless otherwise explicitly stated. In addition, household fixed effects and time fixed effects are always included.

[22]With household fixed effects, this approach amounts to a difference-in-difference design.

4.1 Investigating determinants of peer effects: Individual characteristics, financial position and homophily.

I will extend and back up the baseline analysis by exploring whether the size of peer effects vary with observable characteristics of the neighbors and neighborhoods, and, crucially, if this variation is in line with what to expect if the baseline estimates are in fact true peer effects. I consider variables that can be broadly classified into three categories: (1) *individual determinants*, meaning individual/household characteristics (age and number of children), financial position (income, liquid wealth, total wealth) as well as indicators of financial literacy; (2) *neighborhood characteristics*, meaning the neighbors' residence and the mode of living (apartments or houses) and the distance between the winners' residence, and finally; (3) *common characteristics* of winner-neighbor pairs that capture the degree of similarity between them, known in the network literature as "homophily." It is well-established in the literature that homophily matters for social interaction and the creation of friendships (see e.g., Currarini, Jackson, and Pin (2009) and McPherson et al. (2001)).

The first analysis of heterogeneity in peer effects entails adding interaction terms of the control variables in X_{t-1} to the model. That is, I run separate regressions for each of the interaction terms, leaving the model otherwise unaltered:

$$Debt_{ixt+h} = \beta_0 + \beta_1 \mathbf{X}_{it-1} + \gamma^h Lottery_{xt} + \delta Lottery_{xt} \# z_{it-1} + \alpha_i + \tau_t + e_{it} \tag{2}$$

Here, z_{it} is always one of the elements in the vector $\mathbf{X}_{it}$ (as described in the paragraph *Control variables*) and δ is the interaction coefficient. Interaction variables, z_{it}, are mean centered to ease interpretation. Thus, the main effect, γ, is the treatment effect at the mean value of $z_{t.-1}$. For instance, the mean household head age is 54. Thus, we interpret γ as the average debt response among households at age 54, whereas the interaction term, δ, is the added effect of increasing age by one year.

The second category looks into how distance and type of neighborhood matter for peer effects. With respect to the former of these two, the underlying idea is that the probability of having close social ties with the winner decreases with distance, and that closer social ties (homophily) pave the way for stronger peer effects (Sudman (1988)). Neighbors at closer distances are also more likely to observe the winner's income shock, regardless of the social relationship with the winner. I use the sphere-of-influence variable that is constructed based on house numbers to measure the distance from the winner. Admittedly, this is merely a rank distance, and no perfect measure of metric distance, nor of social closeness. Nonetheless, all else equal, neighbors that rank closer are more likely to interact, and winners' income shocks are more likely to be observed. Hence, the hypothesis is that peer effects are stronger at narrower spheres of influence.[23] In the baseline regression, I apply a sphere of influence equal to 10. In this analysis of distance, I vary the sphere of influence step-wise from one to ten. That is, I run separate regressions for each sphere of influence.[24]

[23] This approach to estimating social proximity is close to the framework suggested by Glaeser et al. (2003).

[24] Recall that, to avoid noise, households living at a sphere of influence equal to 0 (same house number) are excluded in the main regressions. That is also the case here.

Next, I investigate how the type of neighborhood, or the households' modes of living, affects peer effects. In survey data, Sudman (1988) shows that individuals living in single-household dwellings are much more likely to consider their neighbor a friend, and have more knowledge about their neighbor, than do individuals residing in apartments. Based on data from Statistics Norway, I distinguish between single-household dwellings, duplexes, and townhouses (hereafter "houses"), on the one hand, and apartment buildings (hereafter "apartments") on the other. As previously noted, a large share of buildings are without a building code, and the original two categories have a large overweight of houses. Therefore, I lump the missing values together with apartments, such that the samples are approximately equal in size.[25] With this rough classification, I run separate regressions for each type, and run pooled regressions with a dummy interaction term equal to one if the household lives in an apartment building, and zero otherwise. I label this dummy variable variable $Apartments(0/1)$.

Finally, I look into the differential effects across neighbors, based on their overlapping characteristics with the winner on their street. In the social network literature, it is a well-established finding that homophily among individuals is an important factor in determining both social interactions and friendships, and the strength of peer pressure (see, e.g., McPherson et al. (2001) and Currarini et al. (2009)). I focus on two indicators.

The first indicator is based on the neighbors' household structure vis-a-vis the winner's household structure. Winner-neighbor pairs, where either both have, or both do not have, children under 18 living in the household, are identified and the neighbors are classified as having an aligned household structure with the winner (hereafter "aligned household structure"). Conversely, winner-neighbor pairs, where the winner has children and the neighbor does not have children (or, the neighbor has children and the winner does not), are classified as having a not having an aligned household structure with the winner (hereafter "unaligned household structure"). As with the neighborhood structure regression, I run separate regressions for each of the two samples (aligned and unaligned), and pooled-sample regression with a dummy interaction term. The dummy variable is labelled $Aligned(0/1)$

The second indicator captures how many years a neighbor have been living on the same street, hereafter referred to as their "tenure". The hypothesis is that building friendships takes time, and therefore that peer effects should be stronger for longer-tenured neighbors than for shorter-tenured neighbors. Based on each household's date of moving into their current residence, I calculate how long each household have been living on the street at the time of treatment. I use this variable to split the data into quartiles of tenure in the year of treatment, and run separate regressions for each quartile.[26]

As with the rank distance made from house numbers, these indicators increase only the probability of stronger social ties, but they need not capture the real-life strength of social ties. Results are therefore prone to noise and should be interpreted with care.

[25] The two categories might therefore more precisely be termed "buildings known to be houses" and "buildings excluding known houses."

[26] The quartiles are < 8 years, 8–15 years, 16–26 years, and > 27 years.

4.2 Can we predict the timing and size of treatment for neighbors?

The key identifying assumption in the paper is that treatment is random, conditional on fixed effects, where treatment is either continuous or dichotomous. As such, it should not be possible to predict the timing of treatment (in the dichotomous case) or the treatment intensity (in the continuous case). In the spirit of Cesarini et al. (2017), I run two regressions with the lagged time-varying controls as predictors and the dichotomous and continuous treatment variables as outcome variables, respectively.[27] Time and household fixed effects are included and standard errors are clustered at street level. The test is performed on the small-prize sample and the big-prize sample.

The results, reported in Table 3, are reassuring. All coefficients are essentially zero and, with the exception of *Debt* in column one and *Deposits* in column two, not statistically significant. The explained variation (R^2) is close to 0 in all cases. A joint F-test, with the null hypothesis that all time-varying variables are 0, fails to reject the null. In sum, Table 3 shows that the variables in the model have no predictive power with respect to when and how much households in my sample will be treated.

Signs of pretreatment responses are indications of potential breaches of the identifying assumption. Results with pretreatment responses among the neighbors are presented in Section 5.1.1 and Figure 4. But it is worth noting the main take-away from Figure 4, namely that the neighbors of *future winners* do not increase debt in the years leading up to treatment. Thus, I conclude that my identifying assumption is in all likelihood fulfilled.

5 Results

5.1 Baseline results

Table 4 reports results from the baseline regression (Equation 1) with the continuous treatment, $Lottery_t$ (i.e., the lottery prize itself) and a sphere of influence equal to 10. I report coefficients on the contemporaneous debt response of neighbors from two models: the small-prize sample (Column 1), and the big-prize sample (Column 4). Recall that the small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 in the period from 1994 to 2006 and that the big-prize sample includes prizes from 100 000 to 1 000 000 and all years from 1994 to 2015.

Column 1 reports a coefficient of 0.026, meaning that in the year that the winner wins, neighbors on average increase debt by 2.6 percent of the prize won (e.g. for a lottery prize of NOK 10 000, neighbors on average increase debt by NOK 260). This estimate is statistically significant at significance levels below 0.1 percent. Standard errors at 0.5 percent imply that with 95 percent probability, the true debt response lies between 1.6 and 3.6 percent. Column 2 shows results for the big-prize sample. The estimated debt response drops to 1.9 percent of the winner's prize, and the point estimates is statistically significant. Thus, we see that the average debt response drops

[27] For the purpose of this exercise, models are estimated as linear probability models and OLS, since the goal here is not to model the relationship per se but rather to detect whether there is any predictive power in the observables.

Table 3: The effect of predetermined observable characteristics on probability of treatment and intensity of treatment

	Small-prize sample		Big-prize sample	
Treatment	Timing(0/1)	Intensity	Timing(0/1)	Intensity
Age_{t-1}	0.000	-0.154	0.000	-1.597
	(0.90)	(-0.26)	(0.24)	(-1.27)
$Householdsize_{t-1}$	0.001	123.7	0.000	-156.4
	(1.08)	(1.12)	(0.21)	(-0.54)
$Moved_{t-1}$	-0.001	-12.45	0.005	326.0
	(-0.39)	(-0.03)	(0.92)	(0.18)
$Income_{t-1}$	0.000	0.000	0.000	0.000
	(-1.21)	(0.12)	(0.26)	(0.57)
$Deposits_{t-1}$	-0.000	-0.000	-0.000*	-0.000
	(-1.13)	(-1.35)	(-2.02)	(-1.86)
$Stocks\ and\ bonds_{t-1}$	0.000	-0.000	0.000	0.000
	(0.25)	(-0.17)	(0.97)	(0.28)
$Inheritance_{t-1}$	-0.000	-0.000	-0.000	10.001
	(-1.67)	(-1.03)	(-0.88)	(-1.10)
$Debt_{t-1}$	-0.000*	-0.000	-0.000	-0.000
	(-2.08)	(-0.10)	(-0.31)	(-0.25)
$Constant$	0.050***	5231.8***	0.058***	18 380
	(4.80)	(3.53)	(5.97)	(6.18)
N	1 816 326	1 816 326	788 919	788 919
adj. R^2	0.006	0.002	0.005	0.002
F (prob > F)	1.94 (0.07)	0.52 (0.84)	0.96 (0.45)	0.89 (0.52)

Notes: t-statistics in parentheses. Standard errors are clustered at variable street ID. All regressions include household fixed effects and time fixed effects. The final row reports results from F-tests where the null hypothesis is that coefficients on all time-varying variables is equal to zero. Predictor variables are measured at $t-1$. The two samples include only households that live on a street where there is a single winner throughout the sample period from 1994 to 2015. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. The big-prize sample includes prizes from NOK 100 000 to NOK 1 000 000 and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011. Headers indicate the dependent variable: "Timing of treatment" is one if a household lives within ten houses of a lottery winner in the year that the winner wins, and 0 otherwise; "Intensity of treatment" is equal to the lottery prize in the street of the household. Models are estimated with linear OLS.

Table 4: Debt response among neighbors at sphere of influence equal to ten. Small-prize and big-prize samples

	Small-prize sample	Big-prize sample
$Lottery_t$	0.026***	0.019***
	(0.005)	(0.005)
N	612 259	237 678
adj. R^2	0.224	0.337

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. The treatment variable $Lottery_t$ is the prize of the unique winner on a household's street. All regressions use a sphere of influence equal to ten, and include household fixed effects and time fixed effects. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 in 2011 NOK and years from 1994 to 2006. The big-prize sample includes prizes from NOK 100 000 to NOK 1 000 000 in 2011 NOK and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011. Time-varying controls include a second-order polynomial of age, education level, household size, number of children under 18, contemporaneous and two lags of dummy capturing year of moving, and lagged values of net income, housing wealth, deposits, stocks and bonds, total wealth and inheritance, see Section 4.7

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

by more than half a percentage point when we focus solely on big prizes above NOK 100 000.

In sum, the results in Table 4 show that debt is accumulated in response to the income shock of the winner, and I interpret this as a causal peer effect.[28] For the average lottery prize in the small-prize sample (≈ NOK 90 000) a coefficient of 2.6 percent amounts to an average increase in debt of NOK 2 340 for each neighbor within the sphere of influence of ten (on average 22 households). Or, in terms of average debt for the treated in the year before treatment (≈ NOK 390 000), it is a 0.6 percent increase in debt. Similar back-of-the-envelope calculations for the big-prize sample suggest an increase in debt of NOK 4 940 for average prizes (≈ NOK 260 000), or a 1.2 percent increase in debt relative to debt before treatment.

Table 5 provides further insight into the main result with coefficient estimates from regressions with a discrete treatment variable, and a model that includes a second-order polynomial of the lottery prize ($Lottery_t^2$). The former variable, $Lottery(0/1)_t$ is one in treatment years, and zero in all other years. The coefficient $Lottery_t^2$, is negative if the debt response in terms of the lottery prize decreases with the prize size, as the results in Table 4 indicated. For both models, I report the results for the small-prize sample (Columns 1 and 3) and the big-prize sample (Columns 2 and 4).

The discrete model estimates that within the small-prize sample, the average effect on debt

[28] One interpretation of the debt response among neighbors is that they reflect a gambling peer effect, i.e., that neighbors increase gambling as a response to the winners' lottery prize. Since I cannot observe gambling, only prizes, I cannot exclude this possibility. However, my sample selection, i.e., focusing on streets with one winner only, is intended to reduce the likelihood of this being the case. If there is still a gambling peer effect in my sample, my sample selection will place an upward bias on my estimates because, by construction, my sample excludes the cases were households win (and thereby presumably *reduce debt*) because of the increased gambling. It is worth underscoring that the possibility of a gambling peer effect does not pose a threat to my identification strategy, nor the interpretation that *a* peer effect causes neighbors to increase debt.

Table 5: Debt response among neighbors at sphere of influence equal to ten. Discrete and non-linear models/treatment

	Discrete treatment		Non-linear treatment	
	Small-prize sample	Big-prize sample	Small-prize sample	Big-prize sample
$Lottery(0/1)_t$	6083.2***	6342.4***		
	(808.0)	(1584..1)		
$Lottery_t$			0.066***	0.036**
			(0.0107)	(0.0116)
$Lottery_t^2$			-7.75e-08***	-2.76e-08
			(1.68e-08)	(1.77-08)
N	612 259	237 678	612 259	237 678
adj. R^2	0.224	0.337	0.224	0.337

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Treatment variable $Lottery(0/1)_t$ is a dummy variable equal to one the year the street's unique winner wins, and zero otherwise. $Lottery_t$ is a second-order polynomial of the continuous lottery prize variable ($Lottery_t$). All regressions use a sphere of influence equal to ten, and include household fixed effects and time fixed effects. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 in 2011 NOK and years from 1994 to 2006. The big-prize sample includes prizes from NOK 100 000 to NOK 1 000 000 in 2011 NOK and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011. Time-varying controls include a second-order polynomial of age, education level, household size, number of children under 18, contemporaneous and two lags of dummy capturing year of moving, and lagged values of net income, housing wealth, deposits, stocks and bonds, total wealth and inheritance, see Section 4.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

is NOK 6 100. The corresponding estimate is somewhat larger in the big-prize sample (NOK 6 300). Both estimates are statistically significant. Three features of these model results are worth noting. First, the discrete-response estimates imply a more than three times higher response to the average prize than do the linear estimates.[29] The discrete model weights all lottery prizes equally, whereas in the linear model, OLS regressions assign a higher weight to the large prizes. Hence, the discrepancy between the two estimates suggests that, for the average prize, the response is higher than 2.6 percent. Second, the fact that the average krone response is virtually unchanged for the two samples also suggests a non-linear effect, and that the relatively higher average response to small prizes balances out the fact that the same percentage response to a high and low prize is higher for the high prize in absolute NOK terms. Third, the 95 percent confidence interval is quite wide for the two estimates: from NOK 4 500 to NOK 7 700 in the small-prize sample, and from NOK 3 200 to NOK 9 400 in the big-prize sample. This wide interval is not surprising, given that all prizes are weighted equally. Furthermore, the distribution of prizes in the two samples partly explains the much wider confidence band of the big-prize-sample estimate: Prizes are distributed more evenly in the big-prize sample, whereas they are left-skewed toward the smallest prizes in the small-prize sample. Therefore, the variance in the debt responses in absolute NOK terms is higher in the big-prize sample than in the small-prize sample, where the responses to the smallest prizes dominate.

The non-linear effect is confirmed in Columns 3 and 4, where coefficients on both $Lottery_t$ and $Lottery_t^2$ are reported. $Lottery_t^2$ is negative and statistically significant which implies that the debt response as a share of the winner's prize is falling in the amount won. The effect on the main coefficient is that it increases to 0.066, or a 6.6 percent debt response relative to the winner's prize. Since the coefficient on $Lottery_t^2$ is very small, this is approximately the estimate for the smallest prizes in the sample.[30] Unsurprisingly, both the estimates of the main coefficient (0.036) and the concavity of the effect are smaller for the big-prize sample because these exclude the smaller prizes below NOK 100 000. The coefficient on $Lottery_t^2$ is not statistically significant at conventional acceptance levels.[31]

A note on the interpretation of the results The results presented thus far are average effects across all neighbors within a sphere of influence of 10. Whether we interpret the peer effect straightforwardly as a response to the income shock (as above) or as a response to the winners' behavioral response to that shock, including their expenditure, is relevant when considering the magnitude of the estimated peer effect. Equally important is the share of neighbors who

[29] That is, $6\,100/90\,000 \approx 7$ percent in the discrete model, compared to 2.6 percent in the baseline (linear) model.

[30] For the smallest prizes in the sample, 10 000, the added krone value from $Lottery_t^2$ is -7.75, such that the estimated krone effect for these prizes is (10 000*0.066–7.75 =) NOK 652.

[31] Figure A.3 in the Appendix plots the implied NOK values for all prizes, for both the small- and the big-prize samples. They show that for the two prize samples, the krone response to the treatment in the non-linear model exceeds that of the linear model up to prize values roughly around NOK 650 000 for the small-prize sample and NOK 800 000 for the big-prize sample. At most, the non-linear model suggests an average increase in debt of between NOK 14 000 and NOK 15 000.

actually make the discrete choice to take up debt (i.e., compliers to treatment). Unfortunately, both factors are unobservable. Nevertheless, it is worth noting that both considerations suggest that the estimated peer effect of 2.6 percent is a lower bound on the peer effect.

After observing a lottery prize on their streets, the winners' neighbors face both an extensive and intensive margin choice with respect to their debt response. That is, they may decide to increase debt, or not, *and* if they increase debt, they must decide by how much. The estimates in Table 4 reflect a combination of both the extensive and intensive margins. In reality, different neighbors are likely to end up on both sides of the extensive margin choice, where some households do not take on debt at all. If one half or a quarter of the neighbors respond, the peer effect is accordingly doubled or quadrupled. Suffice it to say, statements about the size of the peer effect based on assumptions about the share of neighbors responding will be entirely speculative. Still, we should keep in mind that the average debt response among the neighbors who do take on debt is quite likely higher than what the point estimates thus far indicates. The analysis of heterogeneity in Section 5.2 confirms this hypothesis.

A plausible interpretation of my results is that neighbors increase debt as a response to the winners' expenditure responses to the lottery prize, rather than as a response to the income shocks per se. That is, that the underlying mechanism driving neighbors' debt responses is a consumption peer effect. However, we cannot observe whether neighbors respond to the winners' expenditure hike after the income shock or to the income shock itself. Without observational evidence that can distinguish between the two, I instead make a simple back-of-the-envelope calculation to get some sense of the magnitude of these expenditure-induced debt responses. The approach is simply to scale the neighbors' identified debt responses by the winners' identified expenditure responses to the lottery prize.[32]

To estimate the winners' expenditure response, I run a regression as in Equation 1 but on the winners in my sample, and with their imputed expenditure as the dependent variable.[33] I estimate that the winners in my sample spend 42 percent of the winning prize the year they win, with a standard error of 3.0[34] The expenditure response for winners in the big-prize sample is 34 percent, with a standard error of 2.9.

I use these estimates to make a rough back-of-the-envelope calculation of how much debt increases in response to and in terms of the winners' expenditure hike. Scaling the baseline estimated peer effect by the winners' contemporaneous expenditure estimates implies a debt response of (0.026/0.42 =) 6.2 percent in the small-prize sample, and (0.019/.3 =) 6.3 percent in the big-prize

[32]It is important to note that this is a matter of interpretation and not identification. A second approach is an instrumental variables (IV) regression. The reason for not focusing on IV in this paper has to do with the validity of the exclusion restriction. That is, I cannot observe, or test, whether neighbors respond to the endogenous variable (expenditure) or the instrument (income), or both. Instead, I leave that issue to a matter of interpretation, as I do in this section.

[33]Since expenditure is not directly observed in the data, spending is imputed by using the budget constraint and the observed income and wealth (changes) of each household (see e.g. Fagereng and Halvorsen (2017)).

[34]In Fagereng et al. (2021), they estimate that winners spend 52 percent of the prize within the year of winning, with a standard error of 1.4. My estimated expenditure response is somewhat smaller, but the confidence bands of the two estimates overlap. The difference is due to a smaller fraction of small-prize winners in my sample.

sample. These calculations therefore suggest that the neighbors' debt responses are approximately linear in the *winners' expenditure* responses. Combined with the non-linear debt response among neighbors reported in Section 5.1, this is consistent with the finding in Fagereng et al. (2021) that winners' expenditure decreases with the prize size.[35]

5.1.1 Dynamics and pretreatment trends

To this point, effects beyond the treatment year have not been discussed. From a macro perspective, not only the magnitude but also the persistence of the debt levels induced by peer effects is important. If peer effects simply affect the timing of purchases (expenditure shifting), and not the sum of purchases and debt, they are less important for macro analyses. If, on the other hand, peer effects cause persistent effects, they can contribute to explaining phenomena such as the parallel rise in inequality and debt or whether peer effects might be a concern for financial stability. I analyze this issue further in Section 5.3.

Figure 4 plots the dynamic responses of neighbors with a sphere of influence equal to ten. The x-axis plots the horizon relative to the treatment year. That is, values represent years to or since treatment, with zero as the treatment year. The point estimate at horizons zero to five is from the regression Equation 1 with h = {0, 5}. In addition it plots estimates of the lead effect of the treatment, meaning the effect of a future lottery prize in the street on current debt of neighbors. To be precise:

$$Debt_{ixt} = \beta_0 + \beta_1 \mathbf{X}_{it-1} + \gamma^j Lottery_{xt+j} + \alpha_i + \tau_t + e_{it} \tag{3}$$

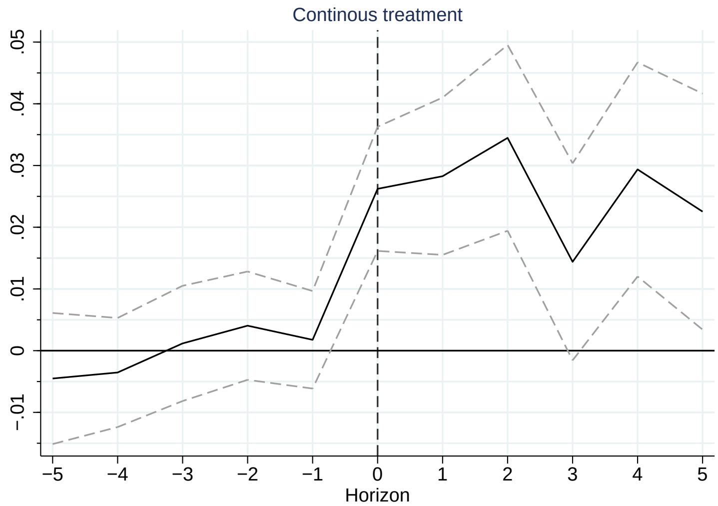
where γ^j is the estimated responses to the winners' future prizes, j periods ahead ($Lottery_{xt+j}$). I estimate Equation 3 for $j = 1$ to $j = 5$. All other variables are as in Equation 1.

Plotting these 11 γ coefficients together produces the impulse response function of debt to the lottery shock. Because my dependent variable is debt in levels, the solid lines read as the cumulative response of debt at different horizons, or how the stock of debt evolves in years before and after treatment (after taking controlling for household and time fixed effects as well as time-varying covariates). The plotted results are from the specification with the linear treatment variable and the small-prize sample. The y-axis thus measure the debt response as a fraction of the prize size.[36] Together with the point estimates (solid lines), I plot the 95 percent confidence intervals (dashed lines). A parallel figure plotting the (placebo) treatment effect for the reference group (described in section 3.3), is reported in the Appendix, Figure A.5. That figure shows no sign of a "treatment response" for this untreated group.

[35] An alternative explanation for the non-linear effect is that lottery winners in fact share some of their winnings with their neighbors either in the form of informal lending or charity. If this is the case, it seems plausible that this effect is stronger for higher prizes, which in turn may explain a relatively weaker effect on observed debt from big prizes. Unfortunately, however, with the current data at hand I cannot investigate this mechanism.

[36] In the Appendix, Figure A.4 report the dynamic response when replacing the linear with the discrete treatment variable. The results are very similar. I do not plot a similar figure for the non-linear model for two main reasons: 1: Response depends on prize size, and what level to plot is not straightforward to choose. 2: The dynamic effect and prize size together are likely not independent, making both results and the interpretation of them untransparent.

Figure 4: Dynamic debt responses before and after treatment.



Notes: The figure plots the point estimates of the debt response of neighbors at different horizons relative to the treatment year. Treatment is the continuous treatment variable Lottery$_t$, and the y-axis reports the debt response as a fraction of the winner's prize. The solid lines read as the evolution of the stock of debt before and after treatment, after controlling for household fixed effects and time fixed effects and time-varying controls. Dashed lines display the 95% confidence bands around the point estimates. Each horizon is estimated separately, with Equation 1 and Equation 3. Point estimates at the negative horizon are the debt effect of a future lottery prize in the street (pretreatment response). Estimates are for the small-prize sample, with a sphere of influence equal to ten.

Three features of Figure 4 stand out. First, pretreatment responses could bias results, and in the worst case be signs of breaches of the assumption that treatment is random. We note that there are no signs of a pretreatment response in debt in the five years leading up to treatment in the figure. Second, Figure 4 visualizes the significant and sharp debt response in the treatment year (i.e., at x-axis = 0). This is the same point estimate as that reported in Table 4. The third and final feature is that the figure shows a persistent debt effect: the linear model estimates a positive debt effect up to five years after the treatment year (although not significant in year three). In the two years after treatment (x-axis = 1 and x-axis = 2), this model estimates an even higher level of debt than in the treatment year. This could reflect further accumulation of debt (and a lag in repayment of debt from period zero) among the neighbors responding contemporaneously. But it could also be that some neighbors respond with a lag, and thereby add to the average debt-level response in these periods. Although not always statistically significant, point estimates suggest that neighbors' debt stays above pretreatment levels up to five years after the winner wins a lottery prize. Thus, the increase in debt in the treatment year is not simply a one-off effect that is repaid in full in the immediate following year (suggesting that the peer effect merely results in expenditure shifting), but rather a higher debt that households carry for several years.

5.1.2 Robustness

Before turning to the analysis of determinants of the debt responses, I scrutinize the sensitivity of my baseline estimates with a series of robustness tests. These tests show that the estimate is robust to various changes in the sample and model specifications: My point estimates are at most marginally different from the baseline of 0.026, and are always statistically significant at the one percent significance level. I categorize my robustness tests in two main categories: *sample robustness* and *model robustness*. Results are reported in the Appendix, Tables A.1–A.3. Since inference is not affected by the alternations, I will not make specific comments on standard errors in the detailed description below.

Sample robustness Table A.1 reports results from the baseline model (Equation 1) albeit with a series of different changes in the small-prize sample. My baseline estimate of the coefficient on $Lottery_t$ is reported for convenience in Column 2. Table A.2 reports the same results with the big-prize sample. These results are overall the same as in the small-prize sample, and are not discussed here.

I first investigate the sensitivity of my results to the max-limit on prizes set at NOK 1 million. Columns 3 and 4 report the debt response when reducing or increasing the maximum prize in the sample by NOK 50 000. The results echo the non-linear debt response in the prize size, as the point estimate is increased (to 0.029) and reduced (to 0.022), respectively.

Next, I report estimates when altering the sample trimming on debt, income and stock value. Recall that in my main samples, households with debt, income or stock value in the top one percent of the distribution, in addition to households with income in the bottom one percent in any of the years in my sample period, are excluded. Columns 5 to 8 report point estimates when each one of these conditions is reversed. Including households with debt in the top one percent in the sample (Column 5, labeled "Debt") increases the point estimate marginally (to 0.027). If I instead include households in the bottom and top one percent of the income distribution (Column 6, "Income") or the households with the top one percent stocks and bonds values (Column 7, "Stocks"), the estimates are also marginally affected, if at all (0.028 and 0.027, respectively). I conclude that, if anything, my baseline estimate is pulled down by my relatively moderate trimming on these variables.

The final column provide the result where I include households living in the same building as the winner (Column 9) in my estimated model with a sphere of influence of 10. This adjustment changes the estimated debt response minimally, to 0.025.

Model robustness In the main part of the paper, the empirical specification is a regression model with debt in levels on the left-hand side without controlling for the lagged value of the debt level on the right-hand side. This is the preferred specification because adding lagged debt as a control might cause a Nickell bias (Nickell (1981)). This is not the only candidate specification for estimating the debt response to the lottery shock. Thus, I scrutinize the sensitivity of my point estimate by altering the model specification. Results are reported in Table A.3. The table reports results for the small-prize sample (labeled SPS in the table) and big-prize sample (labeled BPS in the table).

Since the treatment is a one-time shock, and arguably strictly exogenous, the Nickell bias resulting from adding the lagged dependent variable on the right-hand side should be small. Columns 1 and 2 in Table A.3 show that when I add $Debt_{t-1}$ to the regression as part of the controls, the estimates increase marginally, to 2.7 and 2.0 percent for the small- and big-prize samples, respectively.

In the final two columns of Table A.3, I shift the dependent variable to annual change in debt ($\Delta Debt_t$). With household fixed effects and time fixed effects, the interpretation of the treatment effect is therefore the effect on *acceleration in debt growth*, an admittedly difficult variable to compare with the response in debt levels. The estimated coefficients are somewhat larger (0.028 and 0.021), and still significant at the 0.1 percent significance level.

5.1.3 Effects on income, deposits and imputed expenditure

Why do neighbors take on debt in response to the winner's income shocks? A natural extension of my analysis of debt is to investigate the effect on other balance-sheet items, as we ask what the increased debt is financing. The first explanation that comes to mind is that households increase debt to finance higher consumption. However, as pointed out by Georgarakos et al. (2014), consumption hikes caused by peer effects on consumption may be financed by increased labor income, reduced savings or increased debt. But the converse need not be true, meaning that increased debt need not imply increased consumption. Thus, there are competing candidates in the consumption story. First, increased debt could be caused by lower income, leaving spending levels unchanged. Second, increased debt could be deposited in a bank account or invested in the stock market, again leaving spending levels unaltered.

In this section, I therefore briefly investigate how the lottery shocks affect neighbors' observed income and deposits, in addition to households' imputed expenditure.[37] Since the imputed expenditure variable contains noise (at least compared to the noise in the other observable variables), I present results where the small-prize sample is additionally trimmed to exclude outliers. Households with expenditure levels in the top or bottom 1 percent of the distribution in any year during the sample period are excluded.[38] The regression model is identical to that of Equation 1, except that the dependent variable is replaced by $Income_t$, $Deposits_t$ and $Expenditure_t$.[39]

Table 6 presents results for effects in the treatment year, and the two years that follow. For the reader's convenience, Row 1 contains the debt responses already plotted in Figure 4a. The first take-away from the table is that neighbors do not change income (Row 2) or deposits (Row

[37] Expenditure is imputed as in Fagereng et al. (2021).

[38] Results with an untrimmed sample yield similar, but somewhat higher expenditure responses. These results are reported in the Appendix, see Table A.4.

[39] In a separate set of regressions I use neighbors' tax reported value of cars and boats as the dependent variable. I run regressions both for the small-prize sample and the big-prize sample. The results do not provide any evidence that neighbors buy cars or boats as a response to their neighbor winning the lottery. Results are reported in Appendix Tables A.5 and A.6.

Table 6: Contemporaneous and lagged responses of neighbors' balance-sheet items after a lottery prize

Horizon:	Treatment year	Treatment year + 1	Treatment year + 2
Dependent variable:			
Debt	0.026***	0.028***	0.035***
	(0.005)	(0.007)	(0.008)
Income	0.002	0.001	0.001
	(0.002)	(0.003)	(0.002)
Deposits	0.002	-0.005	-0.005
	(0.004)	(0.004)	(0.005)
Expenditure	0.026***	0.013*	0.005
	(0.005)	(0.005)	0.006
N	612 259	595 127	572 924

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Each row represents a separate regression. The regression specification is as in Equation 1, except for the dependent variable. The dependent variable in each regression is listed in Column 1. Cells report the coefficient estimate on $Lottery_t$ *in treatment year, t (Column 2), the year after treatment,* $t+1$ *(Column 3), and two years after treatment,* $t+2$ *(Column 4). The procedure for imputing expenditure is as in Fagereng et al. (2021), except for the sampling therein. The samples in the expenditure regressions are trimmed, such that households with expenditure above the top one percent or below the bottom 1 percent are excluded from the sample. The resulting number of observations in the regressions in the third row are 571 378, 555 128 and 533 883 for* t, $t+1$ *and* $t+2$, *respectively. All regressions include time-varying controls as described in Section 4, excluding the relevant left-hand-side variable in each individual regression, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.*
$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

3) in the years following the winners' prize. Point estimates are small and insignificant. Thus, the households' budget constraint implies that the debt neighbors acquire finances higher spending.[40]

Row 3 reports the estimated effects on the imputed expenditure of neighbors. It shows a statistically significant effect (p-value below 0.1) in the treatment year: Neighbors increase spending by 2.6 percent of the winners' prize. This flow of increased spending is upheld in the year thereafter, with an additional 1.4 percent (p-value 0.014). At $t+2$ there is no significant effect on expenditure, suggesting that spending levels are back to pretreatment levels. In terms of total spending over the two years, it amounts to four percent of the winners' prizes. Note that the sum of added spending due to the lottery prize of the winner is in the ballpark of the estimated debt response. Contemporaneously, the expenditure and debt response is the same. Two years after treatment debt has increased by almost the same amount as the cumulative expenditure response.

The results suggest that households increase expenditure by increasing debt, but not reducing

[40] Results with the effect on liquid wealth (meaning sum of deposits, stocks and bonds) are similar to that on deposits only. That is, the regressions produce the same signs on the coefficients, but small and statistically insignificant point estimates, see Table A.4 in the Appendix.

deposits. This result might seem puzzling, and in the next section I therefore explore whether this result depend on the households' existing stock of deposits available. However, it should also be noted that the seemingly suboptimal behavior that many households simultaneously take on expensive credit card debt and hold liquid assets (e.g. deposits) is considered an empirical regularity.[41] I scrutinize this finding further in Section 5.2.1.

5.2 Determinants of peer effects

I turn next to the analysis of determinants, or heterogeneity in peer effects. As highlighted by Kuchler and Stroebel (2021), most empirical research on peer effects have focused on broad groups of peers, such as workplace peers (e.g. De Giorgi et al. (2020) or neighborhoods (e.g. Agarwal et al. (2020)). But what type of neighbors are more likely to affect a household's financial decisions? In this section I provide a novel analysis of a wide range of factors potentially influencing peer effects. I show that factors that plausibly reflect stronger social ties between the winner and a neighbor consistently produce higher point estimates, even though differences are not always statistically significant.[42]

I present the results in three main sections: (1) differences in households' individual characteristics and finances including financial-literacy indicators, (2) neighborhood characteristics, including the type of building households reside in and the distance between neighbors and winners, and (3) measures of homophily, which include the household structures of neighbors and winners, and the neighbors' tenure on the street. The list of factors potentially affecting the strength of the peer effect is obviously not exhaustive, but one that is possible to investigate reasonably well with the available data.

5.2.1 Household characteristics, financial position and financial literacy

Table 7 reports results from six individual regressions as presented in Equation 2. I investigate the interaction effect of age (Age_{t-1}), number of children in the household ($\#Children_{t-1}$) , net household income ($Income_{t-1}$), bank deposits ($Deposits_{t-1}$), the sum of stocks, bonds and mutual funds ($Stocks and bonds_{t-1}$) and wealth ($Wealth_{t-1}$). I run seperate regressions for each interaction variable of interest, keeping other control variables as before. Table 7 shows the results with each regression is presented in a row. The interaction variable in question is indicated in the first column, the main coefficient on $Lottery_t$ in the second column, and the interaction term (marked by "$\#z_t$") in the third column. To ease interpretation, interaction variables, except for the variable $\#Children_{t-1}$, are centered at their mean. The estimated coefficients on the financial variables are multiplied by 100 000 in Table 7. All variables are one-year lagged values, to avoid endogenous effects.

[41] See e.g. Guiso and Sodini (2013) for a discussion of this puzzle

[42] The results also strengthens my claim that the estimated debt responses presented thus far are indeed peer effects, since we would expect peer effects to vary with the degree of homophily, or the likelihood of friendship among winners and neighbors.

Table 7: Interaction of debt response with household characteristics and household financial variables

	Main coefficient $Lottery_t$	Interaction term z_{t-1}
Interaction variable:		
$\#Age_t$	0.028***	-0.0008**
	(0.005)	(0.0003)
$\#Children_t$	0.017**	0.018**
	(0.005)	(0.006)
$\#Income_{t-1}$	0.031***	0.015***
	(0.006)	(0.005)
$\#Deposits_{t-1}$	0.026***	-0.000
	(0.005)	(0.001)
$\#Stocks\ and\ bonds_{t-1}$	0.027***	0.006
	(0.005)	(0.008)
$\#Wealth_{t-1}$	0.025***	-0.000
	(0.006)	(0.000)

Notes: Coefficients on financial variables are multiplied by 100 000. Clustered standard errors in parentheses. Cluster variable is street ID. All regressions are for the small-prize sample, and with the continuous treatment, $Lottery_t$. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011. Interaction variables, except for $\#Children_{t-1}$, are centered at the mean within the sample. Financial variables are lagged values. All regressions include time-varying variables as described in Section 4, and household fixed effects and time fixed effects. Number of observations is 612 259 in all regressions.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Three models with interaction terms show a statistically significant effect of their respective interaction term: Age_{t-1}, $\#Children_{t-1}$, and $Income_{t-1}$. The first row suggests that the debt effect decreases with age. The average age in the sample is 54 years, such that above this age level, debt responses decrease by 0.08 percent for each year. The average effect among households without children is 1.7 percent. For households with children, the response for each additional child is estimated to add 1.8 percent.[43] Finally, the regressions with an interaction term on income the year before treatment show a positive effect of higher income before treatment. First, we note that the main coefficient in Column 2 is 0.032. This means that at mean income levels debt levels (approximately NOK 290 000), the debt response is 3.2 percent of the winner's prize. The interaction term is significant: NOK 100 000 extra in income before treatment increase the peer effect by 1.5 percentage point. Together, the higher average estimate in this model compared to the baseline, and the positive sign on the interaction term, tell us that the estimated treatment effects come from the right part of the income distribution. The interaction terms in the final three rows, $Wealth_{t-1}$, $Deposits_{t-1}$ and $Stocks\,and\,bonds_{t-1}$ are small and insignificant.[44] Note also that the main coefficient reported in Column 2 is virtually unaffected by including these terms.

Liquid assets Results in the previous section suggest that amount of liquid assets (deposits or stocks) does not affect how much debt neighbors accumulate. Furthermore, the results in Section 5.1.3 suggested that, on average, households do not run down deposits to finance increased expenditure. Even though it is common in the literature to find that households simultaneously hold both costly credit and bank deposits, these two findings seem somewhat puzzling (Guiso and Sodini (2013). Noting that both regression models above test a linear relationship, I scrutinize this issue further by analyzing the responses of two population subgroups, depending on their stock of deposits. First, I classify household-years with bank deposits below and above the median ("Low deposits" and "High deposits"). Then, I run regressions for each subgroup based on classification status in the year before treatment (i.e., $t-1$) with debt, deposits, and expenditure as outcome variables. The results are reported in Table 8, where the rows represent the outcome variables (as listed in the first column) and the columns the different horizons.

Households with low levels of bank deposits increase their debt significantly more than households with high levels of bank deposits. While the former group raises debt by an accumulated 4.0 percent of the lottery prize over the same three years, the coefficient estimate for the latter group is 1.7 percent and not statistically significant. Instead, this group seems to run down existing deposits, although we note that point estimates are only significant in year $t+1$. Households in the low-deposits group increase their deposits by 1.2 percent of the lottery prize over the period. Turning attention to expenditure, households with little deposits initially increase spending more than households with more deposits (2.8 versus 2.0 percent). Over the next two years, expenditure is not significantly different from zero for the two groups. The point estimates, however, indicate

[43] Table A.7 in the Appendix reports results when splitting the sample into singles and non-singles. The results show that singles tend to respond less than non-single households.

[44] The same is true if I sum the two variables $Deposits_{t-1}$ and $Stocks\,and\,bonds_{t-1}$to measure total $Liquid\,wealth_{t-1}$.

Table 8: Contemporaneous and lagged responses of neighbors' debt, deposits and expenditure by high and low deposits in $t-1$

Horizon:	t		$t+1$		$t+2$	
Deposits in $t-1$:	High	Low	High	Low	High	Low
Dependent variable:						
Debt	0.008	0.035***	0.015	0.030***	0.017	0.040***
	(0.009)	(0.007)	(0.010)	(0.008)	(0.010)	(0.011)
Deposits	-0.007	0.009***	-0.017*	0.007*	-0.016	0.012*
	(0.008)	(0.003)	(0.009)	(0.003)	(0.011)	(0.005)
Expenditure	0.020**	0.028***	0.016	0.006	0.015	-0.007
	(0.008)	(0.006)	(0.008)	(0.007)	(0.010)	(0.008)
N	266 623	345 636	260 861	334 266	252 315	320 609
N (Expenditure)	245 828	325 550	240 419	314 709	232 296	301 587

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Each row represents a separate regression. The regression specification is as in Equation 1, except for the dependent variable. The dependent variable in each regression is listed in Column 1. Regressions are run for subsamples based on amount of deposits in $t-1$. A household is classified as "Low deposits" if it has below the median amount of bank deposits in $t-1$. A household is classified as "High deposits" if it has above the median amount of bank deposits in $t-1$. Cells report the coefficient estimate on Lottery$_t$ in treatment year, t (Column 2–3), the year after treatment, $t+1$ (Column 3–4), and two years after treatment, $t+2$ (Column 4–5). The procedure for imputing expenditure is as in Fagereng et al. (2021), except for the sampling therein. The samples in the expenditure regressions are trimmed, such that households with expenditure above the top one percent or below the bottom 1 percent are excluded from the sample. All regressions include time-varying controls as described in Section 4, excluding the relevant left-hand-side variable in each individual regression, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.
$* p < 0.05$, $** p < 0.01$, $*** p < 0.001$

that households with more deposits increase their total three-year spending more than households with little deposits. Overall, Table 8 suggests that households with more deposits use these to finance increased spending, while households with fewer deposits take on debt to finance spending.

Financial literacy The economic importance of financial literacy has been readily documented in the literature (Lusardi and Mitchell (2014)). One interpretation of the estimated peer effect in debt is that it is distortive, pushing treated households away from their planned path of saving (debt) and expenditure (Kuchler and Stroebel (2021)). From this perspective, financially literate households might increase their debt by less than less financially literate households. On the other hand, financial literacy and socioeconomic status are correlated features of individuals. Thus, if peer effects reflect pressure for status-seeking (conspicuous) consumption that is more prominent (or costly) among households with higher socioeconomic status, this will be a counteracting force on debt accumulation (Veblen (1899), Frank (1985)).

My data source does not provide any direct measure of financial literacy. Instead, I utilize two proxies of financial literacy, namely level of education and stock market participation. The correlation between financial literacy and education is well documented (Lusardi and Mitchell (2014)). Furthermore, both Calvet, Campbell, and Sodini (2007) and Van Rooij, Lusardi, and Alessie (2011) show that more financially literate individuals are more likely to participate in the stock market. Hence, I use these two measures as proxies for financial literacy while at the same time noting that they also might reflect socioeconomic status. For education, I split the population into households where the household head has Lower (10 years or less of schooling), Intermediate (between 11 and 13 years of schooling), and Higher education (more than 13 years of schooling). For stock market participation, I divide households into stock market participants and non-stock market participants. I define the former as households that own a positive amount of stocks or bonds in (at least) the two years leading up to treatment (i.e., in $t-1$ and $t-2$). All other households are classified as non-participants. For each indicator, I estimate four regression models. First, I run separate regressions for the subgroups and then two interaction models. In the first interaction model, I run regression on the pooled sample with an added interaction term (i.e., $Lottery_t * Education(Low/Intermediate/High)$ or $Lottery_t * Stocks(0/1)$) as formalized in Equation 2. In the second interaction model, I run a fully interacted model, i.e., a model where the same categorical variables are interacted with all time-varying covariates. The results are reported in Table 9 and Table 10.

Tables 9 and 10 show that both higher education and stock-market participants increase debt more than low-educated and non-participants. When comparing the subsample estimates in Table 9 (columns 1–3), the point estimate of households with a high education level (3.3 percent relative to the neighbor's lottery prize) is more than double the size of the point estimates of the households with a low education level (1.5 percent). Both estimates are statistically significant, but confidence bands overlap. The interaction models, however, estimate that the difference between high- and low-educated households is statistically significant at a five percent significance level and an even larger difference.

In Table 10, the difference between the subsample estimates (columns 1 and 2) is less pronounced than in comparing low- and high-educated households. Nevertheless, stock market participants increase their debt by more (3.1 percent) than non-participants (2.4 percent). On the other hand, the interaction models estimate a large and statistically significant difference between the two groups (about 2.8 percent in both models). Thus, a higher score on my two (imperfect) financial-literacy indicators does not seem like a bulwark against peer effects. Instead, the results suggest that households with higher education and resources (financial and/or intellectual) to participate in the stock market are more willing to take on debt in response to their neighbors' income shocks. Finally, note that these findings, pointing to socioeconomic status as an important determinant of peer effects, are also consistent with the result (presented in Section 5.2.1) that households with higher income tend to increase debt more than others.

Table 9: Debt response by level of education

	Subsamples: Education level			Interaction (pooled sample)	
	Lower	Intermediate	Higher	w/$Lottery_t$	Fully interacted
$Lottery_t$	0.015**	0.027***	0.033*	0.014*	0.007
	(0.005)	(0.007)	(0.013)	(0.006)	(0.006)
$Lottery_t * Education(0)$				.	.
				.	.
$Lottery_t * Education(1)$				0.010	0.022*
				(0.009)	(0.009)
$Lottery_t * Education(2)$				0.030*	0.032*
				(0.014)	(0.015)
N	155 305	306 272	150 682	612 259	612 259
adj. R^2	0.206	0.231	0.223	0.224	0.224

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Fully interacted model includes interaction terms between Education(0/1/2) and all time-varying controls.All regressions are for the sphere of influence equal to ten, and excluding households living in the same building. All regressions are for the small-prize sample, with a linear continuous treatment and include time-varying variables as described in Section 4, and household fixed effects and time fixed effects. Lower education is 10 years of schooling (primary education). Intermediate is years of schooling between 11 and 13 years. Higher education is defined as more than 13 years of schooling. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. The big-prize sample includes prizes from NOK 100 000 to NOK 1 000 000 and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Debt response by stock market participation status

	Subsample: stock participation status		Interaction (pooled sample)	
	Non-participant	Participant	w/$Lottery_t$	Fully interacted
$Lottery_t$	0.024***	0.031**	0.015**	0.015**
	(0.006)	(0.011)	(0.005)	(0.005)
$Lottery_t * Stocks(0/1)$			0.028*	0.029**
			(0.011)	(0.011)
N	387 378	224 881	612 259	612 259
adj. R^2	0.201	0.187	0.224	0.224

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Fully interacted model includes interaction terms between Stock participation(0/1) and all time-varying controls. All regressions are for the small-prize sample, with a linear continuous treatment and include time-varying variables as described in Section 4 and household fixed effects and time fixed effects. Stock market participants are defined as households with a positive value of stocks or bonds in year $t-1$ and year $t-2$. Non-participants are all other households. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.2.2 Neighborhood characteristics

In the social network literature, space is viewed as one of the most basic sources of homophily, or similarity, between individuals (McPherson et al. (2001)). Shorter geographic distance breeds closer social relations. Here, I analyze how peer effects in debt are affected by distance, measured as the number of houses between neighbor and winner, and the types of residential units that the households live in, measured roughly as the difference between houses (e.g., single-household dwellings) and apartments. Both factors affect the degree of homophily on the street and, crucially, the ability of neighbors to observe the winners' income shocks and expenditure. The hypothesis is that peer effects are greater at shorter distances, and in neighborhoods that do not consist of large apartment buildings. Compared to variables that measure different dimensions of homophily directly, based on individual characteristics of the winner and his or her neighbors (i.e., that reflect the probability of friendship at the individual level) (see Section 5.2.3), these variables affect the probability of friendship in general, independently of individual-level characteristics.

Houses versus apartments Here, I investigate is how peer effects differ for households living in houses and apartments. The tendency for stronger social ties among neighbors living in houses than among neighbors living in apartments was clear in survey data in Sudman (1988). In addition to affecting the formation of social ties, the urbanization level of a street might reflect the degree of homophily in other dimensions: Apartments in urban areas typically attract a more heterogeneous mix of households than do areas with single-household dwellings (McPherson et al. (2001)). Finally, housing structure affects the ability to observe the income shock of the neighbor and/or his spending behavior. For instance, a new car parked in the driveway of a single-unit house is easier to observe and ascribe to the owner of that driveway than is a new car parked in a basement garage or on the street lined by apartment buildings.

The procedure for distinguishing between houses (single-household dwellings) and apartments (i.e., buildings classified as apartments, plus buildings with missing values) was described in Section 4.1. Table 11 reports regression results for each subsample, with the houses subsample in Column 2 and the apartments subsample in Column 3. In Column 4 I report results from the pooled sample with an interaction term equal to one if the household lives in an apartment, and zero if the household lives in a house. Finally, in Column 5 we find the coefficients from a fully interacted model, i.e., a model where the same dummy variable is interacted with all time-varying covariates. This model accounts for differences between subsamples on all dimensions. Only the coefficient for the variable of interest, $Lottery_t * Apartments(0/1)$, is reported.

Point estimates show a larger peer effect for households living in houses in the treatment year. These households increase their debt by 3.5 percent of the prize, compared to 1.6 percent for those living in apartments. Both estimates are significantly different from zero at a 1 percent significance level. The interaction-term model in Column 4 uses information from the full sample, and estimates a larger difference between households living in houses and those living in apartments. The former is estimated to be 4.4 percent of the lottery prize, and the latter is estimated to be 0.8 percent of the lottery prize. Both point estimates are significant at a 0.1 percent significance level. The fully

Table 11: Debt response by mode of living: Single- versus multiple-household dwellings (houses versus apartments)

	Subsamples of neighborhoods		Interaction (pooled sample)	
	Houses	Apartments	w/$Lottery_t$	Fully interacted
$Lottery_t$	0.035***	0.016**	0.044***	0.040***
	(0.008)	(0.006)	(0.008)	0.008
$Lottery_t * Apartments(0/1)$			-0.036***	-0.028**
			(0.010)	(0.010)
N	310 518	301 741	612 259	612 259
adj. R^2	0.248	0.250	0.254	0.254

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Fully interacted model includes interaction terms between Apartments(0/1) and all time-varying controls (not reported in the table). All regressions are for the small-prize sample, and with a linear continuous treatment. Houses are defined as buildings classified as single-unit houses, duplexes or townhouses by Statistics Norway. Apartments are defined as buildings classified as apartment buildings, and buildings not classified (missing code). All regressions include a lagged dependent variable ($Debt_{t-1}$), time-varying variables as described in Section 4, and household fixed effects and time fixed effects. Estimates are for the small-prize sample, with a linear, continuous treatment, and with a sphere of influence equal to ten.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

interacted model that takes into account systematic differences between households living in houses and those living in apartments produces point estimates of similar magnitudes (p-value 0.003). Thus, these results provide quite strong evidence that peer effects are stronger in neighborhoods with single-household dwellings compared with neighborhoods with apartments.

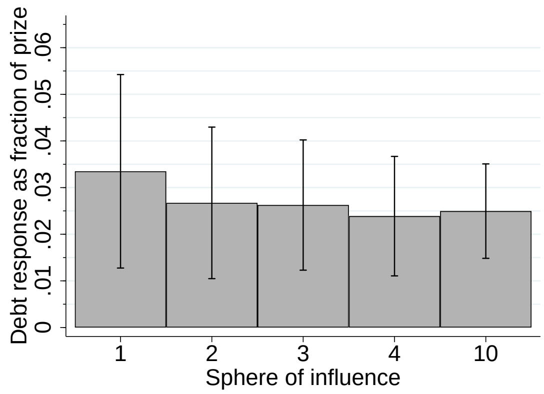
Distance All estimates presented thus far have been with a sphere of influence equal to ten. I now present results with a varying sphere of influence, as described in Section 4.1 to investigate whether debt responses differ depending on the distance to the winner.

Figure 5 reports the point estimates for linear regression models where the sphere of influence is set to one, two, three, four, and ten, respectively.[45] The sphere of influence can be read from the x-axis in each subfigure. The point estimates are depicted as bars, and the vertical capped lines illustrate the 95% confidence bands for these estimates.

The linear model estimates a response of 3.3 percent for the closest neighbors. Compared with the estimates at the sphere of influence of 10, depicted in the right-most bars in each figure, these estimates are 25 percent higher. Estimates including neighbors at distances two, three, and four are also lower than those including only the closest neighbors (i.e., at a distance equal to one) and indistinguishable from those at ten. Overlapping confidence bands imply that these differences are not statistically significant. Still, Figure 5 points to debt responses consistent with the expectation that peer effects are stronger at the closest distances and that the average estimated responses reported in Section 5.1 are pulled up by the closest neighbors. Kuhn et al. (2011) found similar

[45] Neighbor distances from five to nine are not reported because these estimates essentially overlap with the estimates from three to ten. Results are available on request.

Figure 5: Debt responses by sphere of influence.



Notes: Each bar represents the point estimate for separate regressions (as in Equation 1) with a specific sphere of influence. The sphere of influence in the regression is on the x-axis. A sphere of influence equal to "#" includes neighbors at distance # (i.e., # houses away from the winner) and neighbors at distances closer than #. Capped vertical lines display the 95% confidence bands around the point estimates. The y-axis reports the debt response as a fraction of the lottery prize. The model uses a continuous lottery variable as the treatment variable. Results from the spheres of influence from five to nine are not reported but are available on request.

stronger consumption peer effects for the next-door neighbors.[46]

The point estimates at different distances reported in Figure 5 are not significantly different from each other. The difference between neighbors living in apartments and those living in houses is perhaps most pronounced for the next-door neighbors. In urban areas, social interaction with neighbors living in the apartment building next door is more likely to be limited. In contrast, contact with the next-door neighbor living in a single-unit house is likely to be relatively frequent. In Table 12 I therefore report estimates with a sphere of influence equal to one, but separately for the two neighborhood types. Columns 1 and 2 report the estimates for the two subsamples, while columns 3 and 4 report the pooled and interacted models. Finally, I compare these results with those in Table 11 where the distance was set to ten.

Table 12 shows that for the closest neighbors living in single-household dwellings, the estimated debt response is multiple times larger than the debt response among the closest neighbors living in apartments. In the subsamples, the debt response among next-door neighbors is 4.4 percent (and statistically significant) in areas with houses, while only 1.1 percent in apartment buildings (and not statistically significant). In the fully interacted model (column 4), the estimated effect on debt is 4.6 percent of the winning prize for the former. The interaction term (-3.8) is statistically significant (p-value = 0.035) and indicates that the effect for close-living neighbors in apartments is only 0.8 percent. Compared with the estimates for the sphere of influence of ten reported in Table 11 the effects are larger, as expected (4.5 versus 4.0 for houses, and an apartment interaction term

[46]As pointed out in Section 5.1, the number of households that respond to treatment is important but unobserved. One reason for a stronger effect at the closest distance could be that a higher share of neighbors at that distance respond (the extensive margin).

Table 12: Debt response by mode of living for the closest neighbors: Single- versus multiple-household dwellings (houses versus apartments)

	Subsamples of neighborhoods		Interaction (pooled sample)	
	Houses	Apartments	w/$Lottery_t$	Fully interacted
$Lottery_t$	0.044**	0.011	0.047**	0.045**
	(0.015)	(0.011)	(0.015)	(0.015)
$Lottery_t * Apartments(0/1)$			-0.041*	-0.038*
			(0.018)	(0.018)
N	73 909	76 887	150 796	150 796
adj. R^2	0.242	0.213	0.239	0.239

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Fully interacted model includes interaction terms between $Apartments(0/1)$ and all time-varying controls. All regressions are for the sphere of influence equal to one, and excluding households living in the same building. All regressions are for the small-prize sample, and with a linear continuous treatment. Neighbors outside the sphere of influence equal to one are excluded from the sample. Houses are defined as buildings classified as single-unit houses, duplexes or townhouses by Statistics Norway. Apartments are defined as buildings classified as apartment buildings, and buildings not classified (missing code). All regressions include, time-varying variables as described in Section 4, and household fixed effects and time fixed effects. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. The big-prize sample includes prizes from NOK 100 000 to NOK 1 000 000 and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

of -3.6 versus -2.8). However, confidence intervals around these point estimates are overlapping. Although indicating that distance might matter, I cannot reject the null that there is no difference between neighbors living next door and living ten doors down.

5.2.3 Direct measures of homophily: Family and tenure

This section investigates how two (out of many) dimensions of winner-neighbor pair characteristics affect peer effects, namely how long they have been neighbors (tenure) and whether they have similar household structures (aligned or unaligned household structure).

Tenure A social relationship takes time to develop and, except in rare cases, households do not know their neighbors when they move to a new street. Furthermore, with time, structural factors such as neighborhood size, density, or distance become less important, and human factors (similarity or homophily) become more important. We should therefore expect to see a stronger peer effect among neighbors with a longer tenure in the neighborhood. To shed light on this issue, I run regressions on each quartile of the neighbors' tenure on the street, i.e. how many years since they moved to the address of current residence. The quartiles have thresholds of tenure at <8 years, 8–15 years, 16–26 years, and >27 years.

Results are reported in Table 13. Estimates of the debt response in the first quartile show a

Table 13: Debt response by quartiles of neighbors' tenure on the street.

	(1)	(2)	(3)	(4)
	1st quartile	2nd quartile	3rd quartile	4th quartile
$Lottery_t$	0.004	0.034**	0.034***	0.018**
	(0.015)	(0.010)	(0.010)	(0.007)
N	79 922	141 618	174 343	216 376
adj. R^2	0.081	0.214	0.267	0.262

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions are for the small-prize sample, with a linear continuous treatment, and a sphere of influence equal to ten. Tenure is defined as the number of years since moving into current residence at time of treatment. Quartiles of tenure have the following thresholds: < 8 years, 8–15 years, 16–26 years, and > 26 years. All regressions include a lagged dependent variable ($Debt_{t-1}$), time-varying variables as described in Section 4, and household fixed effects and time fixed effects. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

small (0.004) and statistically insignificant effect for "new neighbors" with a tenure of less than eight years. Hence, we cannot reject that the effect for these neighbors is zero. This result is not surprising, and parallel to the finding of De Giorgi et al. (2020) that consumption network effects in workplaces are small and insignificant for new employees. In contrast, for the remaining three quartiles, the null hypothesis of zero-debt responses is rejected at significance levels below 5 percent. Point estimates are higher for the second and third quartile (point estimates of 0.34) compared with the the baseline model (i.e., 2.6 percent). The fourth quartiles have point estimates of 1.8 percent. For the second quartile, i.e., neighbors that have been living on the same street for four to eight years, the effect is estimated to be 2.6 percent. All four estimates have quite large standard errors. Point estimates for the shortest tenure group, however lies outside the 95 percent confidence bands of the second and third groups. That the neighbors with the shortest tenure have smaller estimated debt responses is exactly as one would expect from a peer effect because time is of the essence when building friendships.

Household structure The results in Section 5.2.1 revealed that having children within the household added to the debt response of treated households. Families with children have a number of arenas to meet each other and socialize, such as the local school or playground. Two households that live on the same street and have children are therefore more likely to be in the same social network than if one of them does not have children. Households without children are different types (typically either older or younger) and do not attend these arenas. They might, however, share interests and common meeting grounds with other childless families. In addition, older neighbors might know each other from the time when they did have children at the same age. Thus, in this section, I test whether an *aligned household structure* of the neighbors and the winners creates stronger peer effects.

To study these effects, I split the sample into two subsamples based on whether neighbors have a similar household structure to that of the winner, with respect to children living in the household. I identify whether a winner has children, or not, and similarly for each neighbor. I then create a dummy variable that is equal to one if both households in a winner-neighbor pair have children under age 18 living in the household, or if neither of them has children. Thus, this group includes childless households and households with adult children who have moved out of the house. These winner-neighbor pairs have what I term "aligned household structures." The dummy variable is zero if one of the parties has children and the other one does not, or if the winner-neighbor pair consists of single-member households. This group is labeled "unaligned household structure."[47] As in the *House versus apartments* paragraph, I run separate regressions for each subsample, and run two regressions on the pooled sample: one with the dummy variable, $Aligned(0/1)$, interacted with only the treatment variable ($Lottery_t$), and one fully interacted model where I interact the $Aligned(0/1)$ variable with the full set of time-varying variables.

Table 14 reports the results. The regressions with the subsamples estimate debt responses to 2.1 percent among households with an unaligned household structure, and to 3.4 percent among households with an aligned household structure. Estimates are significant at the one percent significance level. The subsample estimates therefore suggest a substantially larger effect for aligned families than for unaligned families, although we should note that the 95 percent confidence bands of these two estimates overlap. Column 3 reports a smaller effect, with a coefficient on the interaction term that implies that aligned neighbors respond 0.8 percentage points more than do unaligned neighbors. The interaction term is, however, not statistically significant.

The difference between subsample estimates and this single-interaction-term model suggests that the two subsamples also vary on other dimensions than simply children or no children, and that these dimensions also affect the debt response. The fully interacted model takes these differences into account. The results, reported in Column 4, show that the aligned interaction effect is substantially higher (0.19) than in the simple interaction model, and close to conventional significance levels with p-value of 0.06. The inclusion of the other interaction terms (or looking at subsamples) therefore indicates that the two groups constructed in this paper are also different on other dimensions than simply their household structure.

5.3 Peer effects and households' financial vulnerability

Generally, households can interpret peers' saving and consumption decisions as conveying useful information (e.g., about future income) but may also create pressure to "keep up", independently of the information effect (D'Acunto, Rossi, and Weber (2022) Herskovic and Ramos (2020) Moretti (2011),Gomes, Haliassos, and Ramadorai (2021)). In the case of this study, households increase both expenditure and debt due to an isolated income shock that affects only one household in the neighborhood but not themselves. If the debt and expenditure responses of neighbors reflect deviations from otherwise (more) optimal behavior, a natural question emerges whether regulation

[47]Winner-neighbor pairs where both are single-member households are included in this group rather than in the aligned group exactly because they consist of very heterogeneous individuals, ranging from students, divorced households, career-minded adult individuals to widows.

Table 14: Debt response by household structure: Households with aligned or unaligned household structures vis-a-vis the winner

	Subsamples of household structure		Interaction (pooled sample)	
	Unaligned	Aligned	w/$Lottery_t$	Fully interacted
$Lottery_t$	0.021***	0.034***	0.023***	0.018***
	(0.006)	(0.008)	(0.006)	0.006
$Lottery_t * Aligned(0/1)$			0.008	0.019
			(0.008)	(0.010)
N	359 424	252 835	612 259	612 259
adj. R^2	0.218	0.230	0.223	0.224

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. The fully interacted model includes interaction terms between Aligned(0/1) and all time-varying controls (not reported in the table). Aligned household structure includes winner-neighbor pairs where both have children under 18 living in the household, or where both sides do not have children, at the time of treatment. Unaligned refers to winner-neighbor pairs where one of the households has children, but the other does not (or vice versa), and single-member households. All regressions include a lagged dependent variable ($Debt_{t-1}$), time-varying variables as described in Section 4, and household fixed effects and time fixed effects. Estimates are for the small-prize sample, with a linear, continuous treatment, and with a sphere of influence equal to ten.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

should attempt to counteract such distortions (Gomes et al. (2021), Kuchler and Stroebel (2021)). The answer depends on the longer-term and broader consequences for household finances after treatment. Furthermore, high levels and rapid accumulation of household debt concern policymakers because it presumably makes households more vulnerable. One prominent view is that high household debt makes households more sensitive to interest rate fluctuations. Another is that high levels of debt reduce households' ability to smooth consumption when faced with transitory shocks to income.[48] Therefore, in this section, I investigate whether the identified peer effect on debt also has consequences for household vulnerabilities.

I begin by examining whether the identified peer effects on debt make households more exposed to interest rate fluctuations. I measure households' net interest rate exposure, interest rate expenses, and overall debt-to-income. As in Holm et al. (2021) I define $Net\ interest\ exposure_{t+h}$ as total debt net of bank deposits, and $Net interest expenses_{t+h}$ as interest expenses net of interest income. DTI_{t+h} is calculated as total debt over disposable income (i.e., earned income plus transfers net of net interest expenses and taxes). I regress these outcome variables on the same controls and the $Lottery_t$ variable as in Equation 1. A positive sign on the coefficient on $Lottery_t$ (i.e., γ) indicates that households become more sensitive to interest rate fluctuations. Results are reported in Table 15 with the dependent variables in rows and the horizons in the columns. I multiply the

[48] Recent literature find that the cash-flow channel of monetary policy is indeed stronger among households with higher debt (Flodén et al. (2021), Holm et al. (2021), Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru, and Yao (2017)). The role of debt in determining consumption response to income shocks is, however, less clearcut and seems to depend crucially on access to liquid funds (see, e.g., Baker (2018) and Fagereng et al. (2021)). Debt-financed overconsumption, however, seems to reduce households' ability to smooth consumption (Dynan et al. (2012), Andersen et al. (2016)) .

Table 15: Neighbors' net interest rate exposure, debt-to-income and (net) interest expenses

Horizon:	t	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$
$Net\ interest\ exposure_{t+h}$	0.025***	0.033***	0.039***	0.023*	0.025*	0.0351**
	(0.006)	(0.007)	(0.009)	(0.010)	(0.011)	(0.012)
$Net\ interest\ expenses_{t+h}{}^{\dagger}$	19.5	35.8	140*	190**	102	104
	(49.2)	(53.3)	(61.3)	(68.7)	(70.1)	(70.9)
$DTI_{t+h}{}^{\dagger}$	1.05***	0.92***	0.84**	0.18	0.85**	0.65*
	(0.21)	(0.24)	(0.28)	(0.31)	(0.30)	(0.32)
N	612 259	595 127	572 924	547 330	519 670	490 755

Notes: †: the coefficient γ from the regression is multiplied by 100 000. Clustered standard errors in parentheses. Cluster variable is street ID. Each column represents the expenditure response at h years after a neighbor wins in a lottery prize. Each column is the result from separate regressions. The regression specification is as in Equation 4. Net interest exposure is defined as total debt net of bank deposits. Debt-to-income (DTI) is defined as total debt divided by disposable income (i.e. earned income plus transfers net of net interest expenses and taxes). All regressions include time-varying controls as described in Section 4, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

γ-coefficients from the regressions with interest expenses and DTI as dependent variables (rows 2 and 3) by 100 000, such that the interpretation is the effect at approximately the average lottery prize.

Given the result that, on average, households increase debt and do not change deposits, it is not surprising that net interest rate exposure increases (row 1). In magnitude and significance, effects are even more prominent than in the baseline result, where I only studied the debt response. Similarly, given rising debt, net interest payments should increase. Point estimates suggest they rise, but the effects are only significant after two years. In $t+2$ and $t+3$, net interest expenses increase by NOK 140 – 190 for a lottery prize of NOK 100 000. Finally, the third row shows that households have a higher debt-to-income ratio throughout the five years: a lottery prize of NOK 100 000 increases neighbors' DTI by roughly one percentage point contemporaneously. After five years, DTI is still 0.6 percentage points higher than before treatment.

To investigate whether neighbors' expenditure is more sensitive to income changes after treatment, I construct a simple dummy variable that captures a significant drop in income, $Incomeloss_t$. This variable is equal to one in years where earned income decreases by 40 percent or more and zero in other years. I then regress household expenditure on the neighbor lottery shock and include an interaction term $Lottery_t * Income\,loss(0/1)_{t+1}$. The regression is therefore similar to Equation 2, with the modification that z_{t-1} is replaced with the dummy variable $Income\,loss_{t+h}$:

$$Expenditure_{ixt+1+h} = \beta_0 + \beta_1 \mathbf{X}_{it-1} + \gamma^h Lottery_{xt} + \chi^h Income\,loss_{it+h}$$
$$+\delta Lottery_{xt} \# Income\,loss_{it+h} + \alpha_i + \tau_t + e_{it} \quad (4)$$

Where all elements in X_{it-1} and fixed effects (α_i and τ_t) are unaltered from previous specifications. γ^h, as before, reflect the average expenditure response due to the lottery win of a neighbor. χ^h estimates the average expenditure response due to the income shock. The main coefficient of interest is $delta^h$ which is the estimate of the additional expenditure response to an income loss due to peer effects. Note that I estimate the expenditure response to an income shock realized at time $t+1$ (i.e., the year after peer treatment), depending on the lottery shock at time t.[49] If peer effects make households more sensitive to income fluctuations, then we would expect the sign of δ^h to be negative. Table 16 reports the results for income losses in year $t+1$, that is, the year after the initial lottery treatment, and the expenditure response to this shock in years $t+1$ to $t+3$.[50] Rows one to three report the γ^h's, the chi^hs, and the δ^hs, respectively.

The first row provides estimates of the average consumption response in years after a neighbor wins the lottery. They are roughly similar to the estimates reported in Section 5.1.3. In the year after the lottery shock, the extra expenditure of treated households is 1.9 percent of the neighbor's lottery prize (compared with 1.4 percent from Table 6). After that, there is no significant effect on expenditure. Row 2 shows that independently of peer effects (i.e., both before and after treatment), households on average decrease expenditure by NOK 13 000 contemporaneously to an income shock, and NOK 15 000 and NOK 7000 in the two subsequent years. Relative to the average income shock ($\approx$ NOK - 57 000 in disposable income), the contemporaneous average expenditure response is 23 percent. In the third and final row, point estimates have a negative sign on all horizons and are statistically significant at (at least) a five percent significance level for years t and $t+1$. These estimates, therefore, suggest that households that experience a large drop in income reduce spending more due to peer effects. The point estimates indicate that, in the year of a large drop in income, households reduce spending by an additional (0.019 - 0.08=) 6 percent of the lottery prize of their neighbor. That amounts to NOK 5400 for the average lottery prize in the small-prize sample (i.e., $\approx$ NOK 90 000). A back-of-the-envelope calculation suggests that for the average income drop and the average lottery prize, the expenditure response increases from 23 percent to 32 percent.[51] In year $t+2$ the point estimate is -4.7 percent and statistically significant at conventional levels (p-value = 0.028). The baseline response is, however, small (0.6 percent) and not statistically significant.[52] This suggests that the negative expenditure effect of households losing income is more persistent than the expenditure response of households not experiencing a drop in income. After two years, the point estimate on $Lottery_t * Income\,shock(0/1)_{t+1}$ is still negative but no longer statistically

[49] I do not estimate the effect of an income loss arriving at time t because the timing of the lottery shock and income loss within a year t is unobservable and therefore.

[50] In the years $t+4$ and $t+5$ point estimates are small and statistically insignificant, and I do not report these in the table. These results are available on request.

[51] That is: (13 000 + 5400)/57 000 = 0.32

[52] Recall that this was also the case in the analysis in Section 5.1.3.

Table 16: Neighbors' expenditure response to a negative income shock one year after peer treatment

	Expenditure response		
Horizon:	$t+1$	$t+2$	$t+3$
$Lottery_t$	0.019**	0.007	0.006
	(0.006)	(0.006)	(0.006)
$Income\ loss(0/1)_{t+1}$	-12 880***	-15 164***	-7338***
	(1182)	(1159)	(1176)
$Lottery_t * Income\ shock(0/1)_{t+1}$	-0.080***	-0.047*	-0.025
	(0.017)	(0.022)	(0.020)
N	555 128	532 522	507 802

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Each column represents the expenditure response at h years after a neighbor wins in a lottery prize. Each column is the result from separate regressions. The regression specification is as in Equation 4. An income loss is defined as a dummy variable equal to one if earned income falls by 40 percent or more between year t and t+1, and zero otherwise. The procedure for imputing expenditure is as in Fagereng et al. (2021), except for the sampling therein. The samples in the expenditure regressions are trimmed, such that households with expenditure above the top one percent or below the bottom 1 percent are excluded from the sample. All regressions include time-varying controls as described in Section 4, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

significant.

6 Conclusion

I have conducted an empirical micro-level investigation of the causal link between one household's income and its neighbors' debt accumulation. With Norwegian administrative data on household characteristics, balance sheets, and addresses, I constructed a data set consisting of one-time lottery-prize winners and their neighbors. Because lottery prizes are shocks that affect the income of only one household in a neighborhood, my research design bypasses the main econometric challenges in identifying causal peer effects in debt. The crucial identifying assumption is that the timing of becoming a neighbor, selection to treatment, is conditionally random. My analysis shows no pretreatment responses, and observables do not predict treatment timing and intensity. These two results support the identifying assumption.

The baseline results are for a sample of lottery prizes ranging from NOK 10 000 to NOK 1 000 000, and the debt effect for neighbors living up to ten houses from the winner (that is, a sphere of influence equal to 10). These results show a statistically significant debt response that, on average, amounts to a 2.6 percent increase in debt, measured in terms of the lottery prize. A non-linear model suggests that the effect decreases with prize size, with a 6.6 percent debt response for the smallest prizes. Dynamic responses show that debt levels acquired in the year of treatment are persistent because debt remains higher than in the pretreatment period for up to five years after the initial shock. Finally, I show that estimated expenditure responses support the interpretation that neighbors take on debt to finance increased spending.

I back up my baseline findings and provide new evidence on the heterogeneity of peer effects. First, I show that the debt responses vary with observable characteristics likely influencing the degree of social closeness and interaction between a neighbor and a winner. Specifically, the data have allowed me to explore some key characteristics of households, neighborhoods, and similarity (homophily) of winner-neighbor pairs that the social network literature has suggested affect the likelihood of developing friendships and peer effects. These characteristics might affect peer effects either directly (e.g., through the financial ability to acquire new debt or through social connectedness with the winner) or indirectly (e.g., through the neighbors' ability to observe the income shock of the winner). Furthermore, I have provided some novel evidence indicating that households with presumably higher levels of financial literacy (measured by education level and stock-market participation) increase debt more, not less, than other households.

As the importance of debt has gained attention due to its possible role in triggering and exacerbating recessions, understanding the drivers of debt growth becomes increasingly important. This paper has focused on one micro-level behavioral driver of debt accumulation – peer effects. A question that naturally arises from the analysis of peer effects is: are the peer effects beneficial or harmful to households? (Gomes et al. (2021)). Therefore, I have scrutinized the consequences of peer effects on households' financial vulnerability. The results suggest that households become more vulnerable as their interest rate exposure increases and their debt-to-income ratio rises. Furthermore, the expenditure response to a large income loss is stronger than it would have been absent treatment. Although the average household-level responses are relatively moderate, they are non-negligible from a macro perspective. First, the size of the neighborhoods is 22 households at its mean. Therefore, in aggregate, debt accumulation is economically significant. Furthermore, as suggested by the analysis of heterogeneity and, in particular, of access to liquid assets, not all neighbors will increase their debt. This implies that debt accumulation is substantially higher among those who respond to the winner's income shock, making these households particularly vulnerable. Second, to make identification credible, I have focused on a transitory income shock that affects one household only. Thus, peer pressure is arguably relatively weak. The macro-level trend, on the other hand, is a substantial upsurge in income inequality, entailing a significantly stronger and broader peer signal. My results indicate that this trend in income inequality could be one important driver behind increasing aggregate debt levels. Third, and finally, the results show a persistent effect on debt. Hence, the estimated peer effect is a matter not only for the timing of debt accumulation (like expenditure shifting) but also seems relevant for longer-term debt levels.

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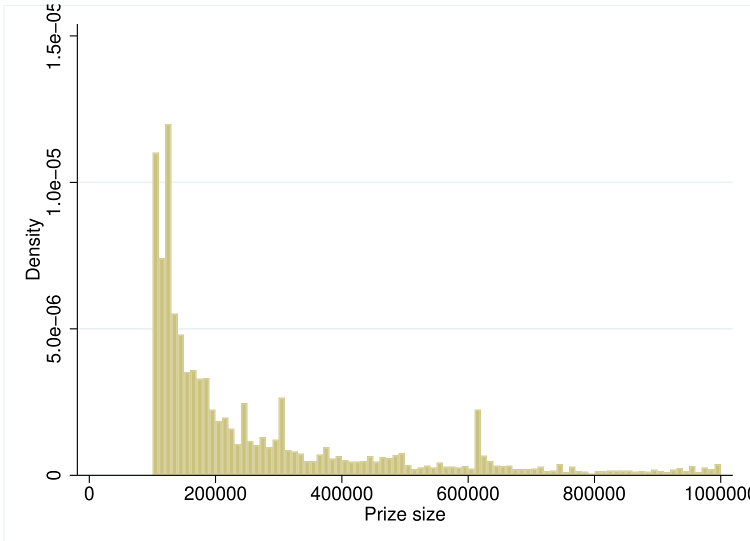
A Appendix

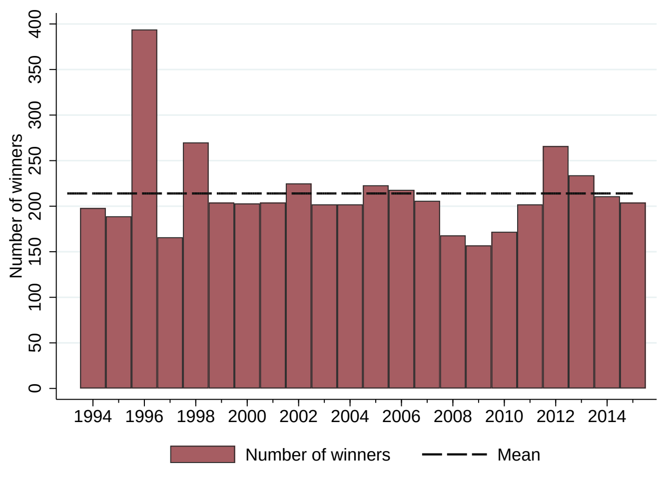
A.1 Extra figures and tables, robustness and IV estimates

Figure A.1: Histogram of prizes among of winners in the big-prize sample

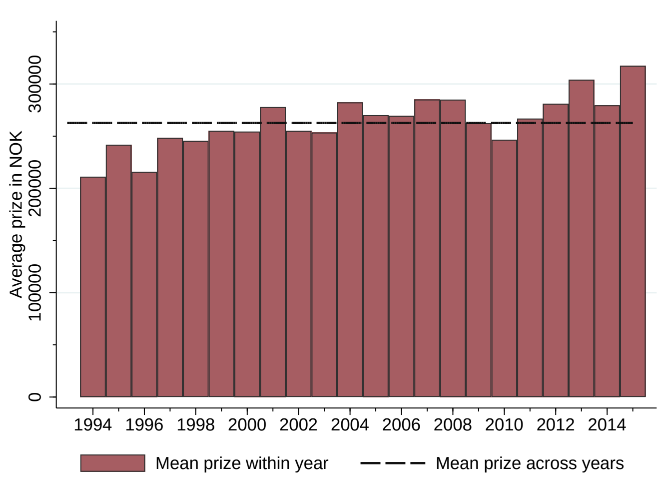


Notes: The figures display the density of the prize values in the big-prize sample. The width of bins is set to NOK 10 000. The big-prize sample includes prizes ranging from NOK 100 000 to NOK 1 000 000, and the years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

Figure A.2: Number of winners and average lottery prize per year in the big-prize sample.



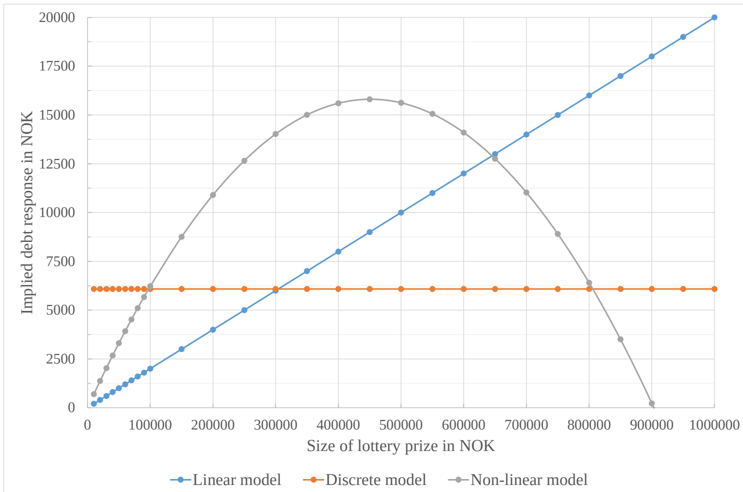
(a) Number of winners.



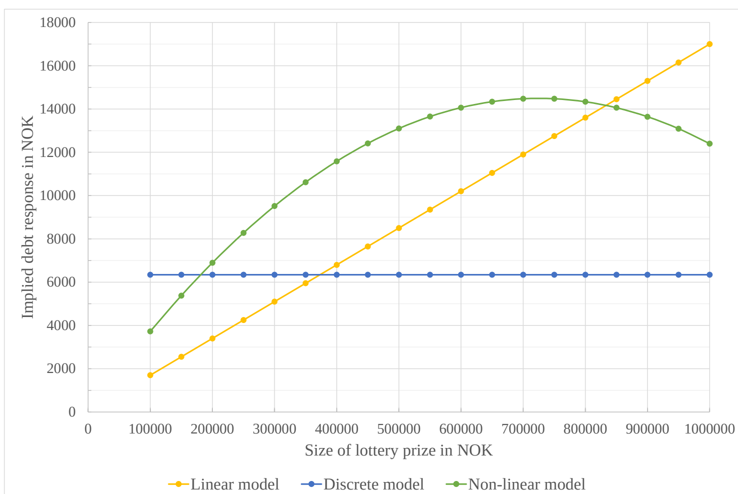
(b) Average lottery prize

Notes: The figures display winners and prizes for the big-prize sample. Bars in panel (a) display the total number of winners each year and bars in panel (b) the average prize in NOK among these winners within each year, conditional on the prize being the only lottery prize on the lottery winner's street over the period 1994–2015. The dashed lines draw the mean value across all years. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011. The big-prize sample includes prizes ranging from NOK 100 000 to NOK 1 000 000, and the years from 1994 to 2015.

Figure A.3: Krone responses implied by linear, discrete and non-linear coefficient estimates



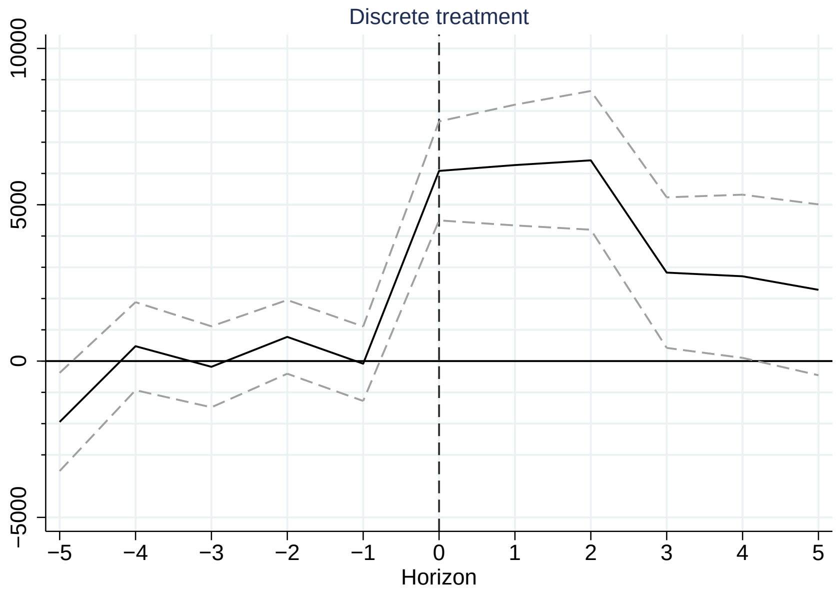
(a) Small-prize sample



(b) Big-prize sample

Notes: Figures display the absolute, average NOK debt responses for various prize magnitudes, as implied by the coefficient estimates from the linear, non-linear and discrete models. These coefficient estimates are found in Table 4 and Table 5. The X-axes measure the prize size in NOK, and the Y-axes measure the implied debt response in NOK. Panel (a) reports the responses for the small-prize sample, and panel (b) reports the responses for the big-prize sample. In panel (a) the blue line plots the implied responses for the linear model, the grey line plots the implied responses for the non-linear model, and the orange line plots the implied responses for the discrete model. In panel (b) the dark-blue line plots the implied responses for the linear model, the yellow line plots the implied responses for the non-linear model, and the green line plots the implied responses for the discrete model. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

Figure A.4: Dynamic debt responses before and after treatment.



Notes: The figures plot the point estimates of the debt response of neighbors at different horizons relative to the treatment year. The solid lines read as the evolvement of the stock of debt before and after treatment, after controlling for household fixed effects and time fixed effects and time-varying controls. Dashed lines display the 95% confidence bands around the point estimates. The figure reports point estimates of the discrete treatment variable $Lottery(0/1)_t$*, which is one in the treatment year and zero otherwise. The y-axis in reports the average debt response in NOK, independently of prize size. Each horizon is estimated separately, with Equation 1 and Equation 3. Point estimates at the negative horizon are the debt effect of a future lottery prize in the street (pretreatment response). Estimates are for the small-prize sample, with a sphere of influence equal to ten.*

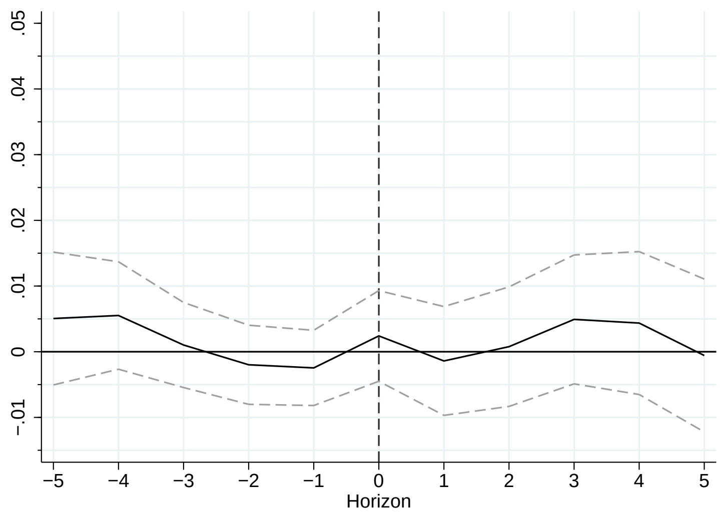
Table A.1: Sample robustness: Sensitivity of baseline estimates to changes in the prize sample, winsorization, and including neighbors at distance 0 or 20 in the sample. Small-prize sample

		Max prize			Trimming		Incl. neighbors at
	Baseline	950K	1.05 mill.	Debt	Income	Stocks	distance 0
$Lottery_t$	0.026***	0.029***	0.022***	0.027***	0.028***	0.027***	0.025***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
N	612 259	610 021	613 712	623 318	670 327	618 694	646 218

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions use a sphere of influence equal to ten, except in Column 9, which uses a sphere of influence equal to 20. All regressions use the linear, continuous treatment variable. All regressions include a lagged dependent variable ($Debt_{t-1}$)*,* age^2*, household size, a dummy variable capturing that the household moves, and lagged income, deposits, stocks and bonds, and inheritance, in addition to household fixed effects and time fixed effects. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.*

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A.5: Dynamic treatment effect in the reference group (placebo treatment).



Notes: The figure plot the point estimates of the debt response of households living in different streets but on the same postal code as a lottery winner (i.e. the reference group described in section 3.3) at different horizons relative to the year a lottery winner wins. Placebo treatment is a continuous variable, equal to the lottery winner's prize. Regressions are run on a ten percent random sample from the reference group to ensure similar sample size as in the main regressions. The solid lines read as the evolvement of the stock of debt before and after the placebo treatment, after controlling for household fixed effects and time fixed effects and time-varying controls. Dashed lines display the 95% confidence bands around the point estimates. The y-axis reports the average debt response in NOK, as share of the prize size. Each horizon is estimated separately, as in Equation 1 and Equation 3. Point estimates at the negative horizon are the debt effect of a future placebo treatment.

Table A.2: Sample robustness: Sensitivity of baseline estimates to changes in the prize sample, winsorization, and including neighbors at distance 0 or 20 in the sample. Big-prize sample

		Max prize			Trimming		Incl. neighbors at:
	Baseline	950K	1.05 mill.	Debt	Income	Stocks	distance 0
$Lottery_t$	0.019***	0.021***	0.016***	0.021***	0.022***	0.020***	0.019***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.05)	(0.005)	(0.005)
N	237 678	233 255	239 622	243 773	260 721	240 129	250 745

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions use a sphere of influence equal to ten, except in Column 9, which uses a sphere of influence equal to 20. All regressions use the linear, continuous treatment variable. All regressions include, age², household size, a dummy variable capturing that the household moves, and lagged income, deposits, stocks and bonds, and inheritance, in addition to household fixed effects and time fixed effects. The big-prize sample includes prizes from NOK 100 000 to NOK 1 000 000 and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3: Model robustness: Fixed effects model without lagged dependent variable and a model with annual change in debt as dependent variable

	Model incl. $Debt_{t-1}$		DV: $\Delta Debt_t$	
	SPS	BPS	SPS	BPS
$Lottery_t$	0.027***	0.020***	0.028***	0.021***
	(0.005)	(0.004)	(0.005)	(0.004)
N	612 259 216	237 678	612 259	237 678
adj. R^2	0.258	0.393	0.165	0.132

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions include time-varying controls and fixed effects as described in Section 4. Column 2–3 include $Debt_{t-1}$ as a control variable. Columns 4–5 report estimates with annual change in debt ($Debt_{t-1}$) as the dependent variable. This model does not include the lagged dependent variable as a control. The small-prize sample (SPS) includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. The big-prize sample (BPS) includes prizes from NOK 100 000 to NOK 1 000 000 and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4: Contemporaneous and lagged responses of neighbors' liquid wealth and expenditure after a lottery prize. Trimmed and untrimmed expenditure sample.

Horizon:	Treatment year	Treatment year + 1	Treatment year + 2
Dependent variable:			
Liquid wealth	0.001	- 0.004	-0.008
	(0.005)	(0.005)	(0.006)
Expenditure†	0.031***	0.018*	-0.004
(*untrimmed sample*)	(0.009)	(0.007)	(0.011)
Expenditure‡	0.026***	0.013*	0.005
(*trimmed sample*)	(0.005)	(0.005)	0.006
N *untrimmed sample*	612 259	595 127	572 924
N *trimmed sample*	571 378	555 128	533 883

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Each row represents a separate regression. The regression specification is as in Equation 1, except for the dependent variable. The dependent variable in each regression is listed in Column 1. Cells report the coefficient estimate on $Lottery_t$ in treatment year, t (Column 2), the year after treatment, $t+1$ (Column 3), and two years after treatment, $t+2$ (Column 4). The procedure for imputing expenditure is as in Fagereng et al. (2021), except for the sampling therein. †: Expenditure is untrimmed, i.e., the sample is the small-prize sample. ‡: Expenditure is trimmed, such that households with expenditure above the top one percent or below the bottom 1 percent are excluded from the sample. All regressions include time-varying controls as described in Section 4, excluding the relevant left-hand-side variable in each individual regression, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5: Contemporaneous and lagged responses of neighbors' car and boat values after a lottery prize. Small-prize sample

Horizon:	Treatment year	Treatment year + 1	Treatment year + 2
Dependent variable:			
Cars	-0.003*	0.001	0.001
	(0.0015)	(0.002)	(0.002)

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Each row represents a separate regression. The regression specification is as in Equation 1, except for the dependent variable. The dependent variable is the NOK tax value of cars and boats. Cells report the coefficient estimate on $Lottery_t$ *in treatment year, t (Column 2), the year after treatment,* $t+1$ *(Column 3), and two years after treatment,* $t+2$ *(Column 4). All regressions include time-varying controls as described in Section 4, excluding the relevant left-hand-side variable in each individual regression, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.*

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: Contemporaneous and lagged responses of neighbors' car and boat values after a lottery prize. Big-prize sample

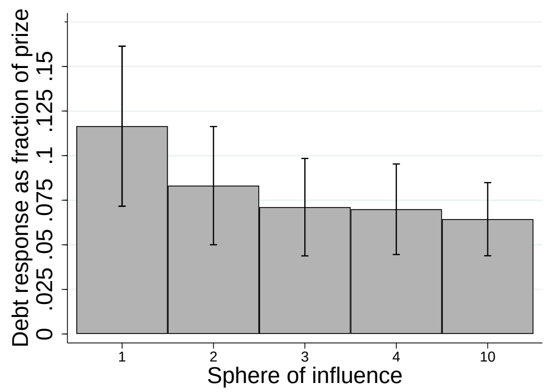
Horizon:	Treatment year	Treatment year + 1	Treatment year + 2
Dependent variable:			
Cars	-0.003*	0.000	0.002
	(0.0013)	(0.002)	(0.002)

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Each row represents a separate regression. The regression specification is as in Equation 1, except for the dependent variable. The dependent variable is the NOK tax value of cars and boats. Cells report the coefficient estimate on $Lottery_t$ *in treatment year, t (Column 2), the year after treatment,* $t+1$ *(Column 3), and two years after treatment,* $t+2$ *(Column 4). All regressions include time-varying controls as described in Section 4, excluding the relevant left-hand-side variable in each individual regression, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.*

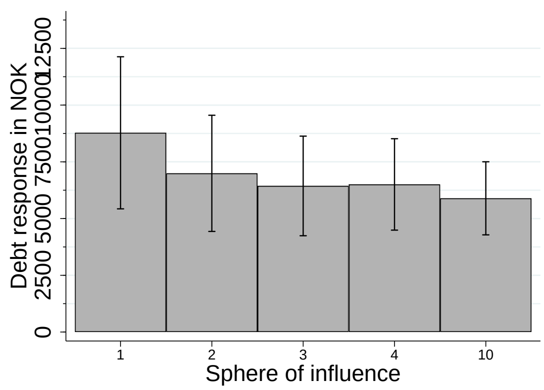
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A.6: Debt responses by sphere of influence. Linear, non-linear and discrete models.

(a) Linear model



(b) Non-linear model



(c) Discrete model

Notes: Each bar represents the point estimate for separate regressions (as in Equation 1) with a specific sphere of influence. The sphere of influence in the regression is on the x-axis in all panels. A sphere of influence equal to "#" includes neighbors at distance # (i.e., # houses away from winner) and neighbors at distances closer than #. Capped vertical lines display the 95% confidence bands around the point estimates. The y-axis in panels (a) and (b) reports the debt response as a fraction of the lottery prize, and in panel (c) it reports the average NOK response from a lottery win in a street, regardless of prize size. The linear model uses a continuous lottery variable as the treatment variable. The non-linear model adds a second-order polynomial to the linear model. Point estimates reported in the non-linear model are those on $Lottery_t$*, i.e., at prizes of approximately NOK 10 000. The discrete model uses a dummy variable as the treatment variable, equal to one the year a street has a winner, and zero otherwise. Results from the spheres of influence from five to nine are not reported, but are available on request.*

Table A.7: Debt response by single and non-single households

	Household members		Interaction (pooled sample)	
	Single	Non-single	w/$Lottery_t$	Fully interacted
$Lottery_t$	0.018**	0.029***	0.033***	0.035***
	(0.007)	(0.007)	(0.006)	(0.007)
$Lottery_t * Single(0/1)$			-0.020*	-0.027**
			(0.008)	(010)
N	189 499	422 760	612 259	612 259
adj. R^2	0.191	0.231	0.224	0.224

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions are for the small-prize sample, with a linear continuous treatment, and a sphere of influence equal to ten. Single households refers to neighbors households with only one member, irrespective of the household members in the winning household. All regressions include a lagged dependent variable ($Debt_{t-1}$), time-varying variables as described in Section 4, and household fixed effects and time fixed effects. The small-prize sample includes prizes from NOK 10 000 to NOK 1 000 000 and years from 1994 to 2006. The big-prize sample includes prizes from NOK 100 000 to NOK 1 000 000 and years from 1994 to 2015. Monetary amounts are measured in NOK that are CPI-adjusted to the year 2011.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$