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Peer Effects and Debt Accumulation: Evidence from Lottery Winnings *

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Abstract

I estimate the effect of lottery winnings on peers' debt accumulation using administrative data from Norway. I identify neighbors of lottery winners, and estimate an average debt response of 2.1 percent of the lottery prize among households that live up to ten houses from the winner. Analyzing heterogeneity, I find that neighborhood characteristics and shared characteristics with the winner matter for the debt response: there is a tendency for greater effects for those (1) residing closest to the winner, (2) residing in single-household dwellings, (3) with a longer tenure, and (4) with a household structure similar to that of the winner. Finally, estimates of the (imputed) expenditure response among neighbors indicate that they accumulate debt to finance increased spending, consistent with a "keeping-up-with-the Joneses" type explanation, where neighbors react to each others expenditure.

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

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

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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, 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 (2003a). 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) and personal bankruptcy risk (Agarwal, Mikhed, and Scholnick (2019), Roth (2020)). Furthermore, Georgarakos, Haliassos, and Pasini (2014) show that the perceived income rank relative to peers affect borrowing, and Kalda (2019) document that peers' financial distress affect household leverage. But to date, no paper has estimated how observable changes in income and consumption in one household affect debt accumulation among its peers.

In this paper I aim to fill this gap, and ask: Do income shocks that hit one household affect debt growth among neighbors? I provide causal estimates based on household-level registry data and an exogenous instrument, lottery income, that side-steps the econometric difficulties in estimating peer effects (Manski (1993)). Importantly, I contribute to the exiting literature by investigating how neighborhood structures and household characteristics affect the strength of these peer effects.

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 that affect only one household in a neighborhood, and that neighbors therefore are 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. 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. Beyond this baseline approach, I take advantage of a rich set of variables in the data to investigate how my estimated peer effects vary with neighbors' financial position, neighborhood characteristics and distance to, similarity with and length of the relationship with the winner.

[1] In this paper, I think of the lottery prizes as a transitory income shocks. 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.

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 for this approach is that we do not observe the number of lottery tickets or the total amount gambled among neighbors of the winner. I therefore restrict attention to streets with one winner only, throughout the entire sample period 1994 to 2015. My analyses show no signs of pretreatment responses and observables do not predict the timing or the intensity (prize size) of treatment. Thus, I give the regression estimates a causal interpretation as peer effects that drive up debt.

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 estimates 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 2.1 percent increase in debt, measured in terms of the lottery prize (e.g. for a lottery prize of NOK 10 000, neighbors on average increase debt by NOK 210). A non-linear model suggests a decreasing effect with the prize size, with a seven 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 6400, with a 95 percent confidence interval ranging from NOK 4800 to NOK 7900. 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 4.4 percent for the small-prize sample and 4.8 for the big-prize sample. Given the existing estimates of how winners' spending responses decrease with prize size, my estimates imply that neighbor's debt response is approximately linear in winners' spending response. With the same baseline regression, but with lead and lags of the debt response (treatment effect), I estimate the dynamic responses. The results show that there are 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.

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

[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 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). Hence, the sample difference explains why the average estimates of the winners' MPC is smaller in this paper.

I extend the baseline analysis and investigate how debt responses vary with observable characteristics of the winners' neighborhoods and the neighboring households. These results align with how we would expect debt responses to vary if they are in fact driven by peer effects: (1) debt responses are smaller and statistically insignificant among neighbors with a relatively short tenure as neighbor of the winner, and stronger and statistically significant among neighbors with a longer tenure; (2) neighbors with a household structure similar to that of the winner increase 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), and finally (4) the estimated response is larger among the closest neighbors than among the more distant neighbors. Although these estimated differences are not always statistically significant, the full set of results suggests that stronger social ties, or structures that lay the basis for stronger social ties, induce stronger peer effects, just as the literature on social networks predicts (McPherson, Smith-Lovin, and Cook (2001) and Sudman (1988)).

Finally, 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. The expenditure response is 3.1 percent of the prize over the first two years after treatment. The latter estimate is the same as that for total added debt over the same period, and the time profile of the expenditure response coincides with the time profile of the debt response. It thus seems that neighbors take on debt to finance increased spending.

In total, my baseline estimates combined with the results on heterogeneity and expenditure indicate that peer effects cause debt accumulation. Moreover, the evidence is consistent with a "keeping-up-with-the Joneses" type explanation, where neighbors react to each other's expenditure.

Compared with the existing literature, this paper benefits from a combination of observational household-level panel data *and* a credible identification scheme to analyze the existence and determinants 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 solidifying previous findings on peer effect driven debt accumulation with credible causal estimates (Georgarakos et al. (2014)), the contribution is in several dimensions. First, I provide a dynamic analysis of the longer-term responses to peer effects that to my knowledge is unique. Specifically, I estimate responses up to five years after the shock, in addition to five years before the shock, to test for pretreatment responses. Second, whereas scarcity of location information often forces researchers to rely on zip codes in peer effect studies, this study identifies exactly who are neighbors and how close they are relative to the winner (see Georgarakos et al. (2014) or Kuchler and Stroebel (2021) for discussions, and Kuhn et al. (2011) for an exception to this rule). Furthermore,

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

the wide panel dimension of the data, which include individual characteristics, enables the use of a wide set of controls and a novel analysis of a wide variety of factors that the literature points to as important for peer effects, including distance, neighborhood structure and similarity among neighbors. 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). Finally, data on debt, income, deposits and stocks enable me to investigate not only the debt response, but also other outcome variables to get a broader sense of the response of households. Importantly, the results in this paper link the peer-driven debt responses of neighbors to increased spending.

My empirical strategy og using lottery prizes to study peer effects is not unique. Among the closest papers to this one are Kuhn et al. (2011) and Agarwal et al. (2019), which both use lottery prizes to investigate peer effects in neighborhoods. 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. (2019) find that in neighborhoods with lottery winners, the risk of bankruptcy increases among the winners' neighbors.[5] 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. Kalda (2019) studies peer effects after negative health shocks that cause financial distress. Using individual credit data, the main result is that financial distress among peers leads to a persistent deleveraging and lower debt levels, both because individuals borrow less and because they 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 also tend to be 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 and, in particular, visible consumption.[6] Bertrand and Morse (2016) find evidence of "trickle-down consumption," i.e.,

[5]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 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.

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

that poorer households spend more on visible goods if they are 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 targeted sections (poor/rich) of the policy.[7] The Danish data also allow them to look into the heterogeneity of peer effects, and they find that peer effects vary with education, women's share in the workplace, economic conditions, and tenure in the workplace.[8]

Finally, this paper also speaks to a broader literature seeking to understand the rise in household debt over the past three to four decades. 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. 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 richer households' increasing level of consumption. A number of 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 it was among their counterparts in low-inequality areas, and therefore argue that inequality does not increase debt levels. With similar data, Bertrand and Morse (2016) reach 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 key 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 rich 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

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

[8]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 levels of happiness and their neighbors' earnings, and show that this effect is stronger when the neighbors share common characteristics and when they have more frequent contact. See also references therein.

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* in order to be economically significant. Rather, my findings suggest that it is the social distance and social similarity between peers that matters for how income hikes in one group triggers debt responses in another.

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. Results are presented in Section 5, 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 in 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.[9]

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.[10] In addition, I scrutinize the validity of the identifying assumption by testing the predictive power of time-varying covariates

[9]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.

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

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 wealth (housing, motor vehicles). 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

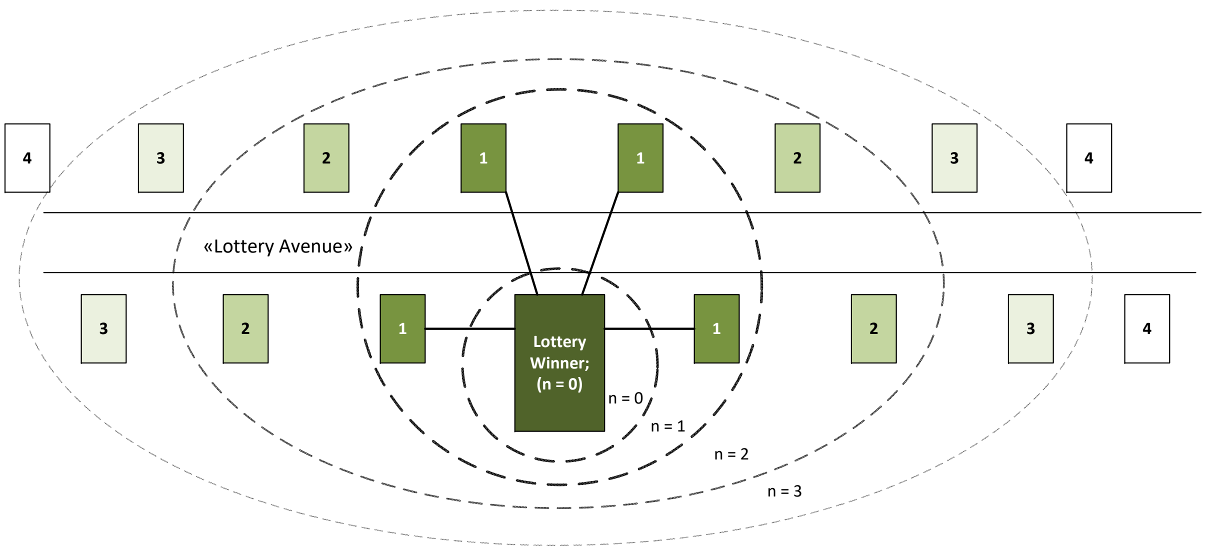
Prize sample Due to changes in the Norwegian Tax Authority's reporting rules, lottery prizes below NOK 100 000 are not available after 2006. I therefore make two samples. The small-prize sample includes prizes below NOK 100 000 over the period 1994–2006. Because individuals in this period were obliged 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. Focusing on this 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.

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, 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

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



living at distance equal to zero in the treatment year, 35 percent are buildings coded as duplexes,[11] 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-unit houses. 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.[12] 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.[13]

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.[14]

3.3 Descriptive statistics

Table 1 displays summary statistics in the small-prize sample on key household characteristics and balance-sheet variables for the treatment group (i.e., the winners' neighbors) and a constructed control group. The latter group consists of all households that live in the same zip code as the winners, but on different streets. For the neighbors, variables are measured the year preceding the lottery win in their street. For the control group, variables are measured the year preceding the

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

[12] 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.

[13] 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.

[14] 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 Control group

	Neighbors mean	sd	median	Controls 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. Controls are households that live in the same zip code as these winners, but on different streets. Variables are measured the year before the winner on the street (or in the zip code) wins a lottery prize (i.e., $t-1$). In zip codes with more than one winner, one winner is chosen randomly to determine 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.

lottery prize in their zip code. In zip codes with multiple streets that win, one of the streets is chosen randomly to define that zip code's treatment year. The table addresses the issue of internal and external validity, and the key question is whether there are systematic differences between neighbors and the control group. Table 1, however, shows that the neighbors and control 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 control 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 displays the number of lottery winners (measured on the Y-axis in 2a) and the average lottery prize per year (measured on the Y-axis in 2b) in the small-prize sample.[15] Figure 2a 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, 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.[16] Apart from this, there is clearly random variation each year, such that neither of the two observations poses any

[15] In the Appendix, Figures A.1a and A.1b are the parallel figures for the big-prize sample.

[16] The reason for the decreasing trend, more precisely, is that the distribution of prizes is leaning 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.

Table 2: Number of observations (neighboring households) by distance to the winner in the treatment year

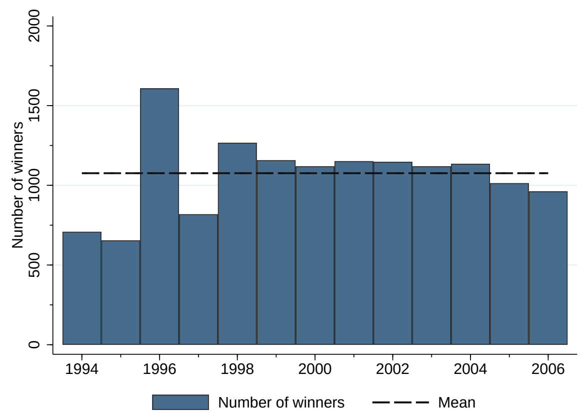
Distance (n)	Observations at n	Total observations within n	%	Cumulative %
Winners	.	13 866	.	.
0	18 130	18 130	7	7
1	44 630	62 760	17	24
2	34 588	97 348	13	37
3	28 293	125641	11	48
4	23 298	148 939	9	56
5	19 213	168 152	7	64
6	15 923	184 075	6	70
7	13 397	197 472	5	75
8	11 221	208 693	4	79
9	9465	218 158	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.

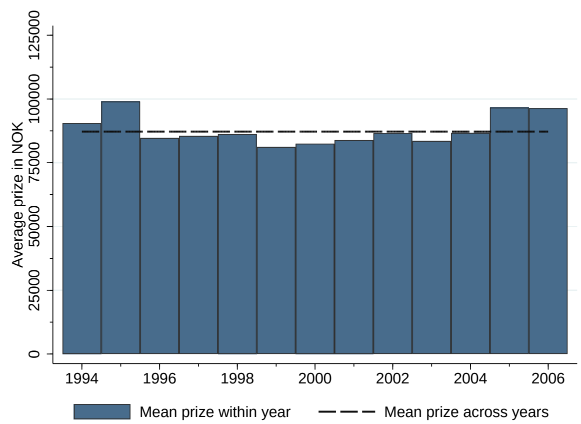
challenge to the analysis. The average number of winners each year is 1076. Figure 2b displays the average prize of the winners in my sample. The figure shows that there is random variation in the average prize paid out. The average prize in all years is close to the cross-year average of NOK 87 232.

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

Figure 2: Number of winners and average lottery prize per year in the small-prize sample (1994–2006)



(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.

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. 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.[17] 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 follows the local projection setup pioneered by Jordà (2005):

$$Debt_{ixt+h} = \beta_0 + \beta_1 Debt_{ixt-1|j_x - k_x < n} + \beta_2 \mathbf{X}_{it} + \gamma^h Lottery_{xt} + \alpha_i + \tau_t + e_{it} \tag{1}$$

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. The model always includes the lagged $(t-1)$ value of the dependent variable, $Debt_{ixt-1}$.[18] 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, irrespective of whether the small- or big-prize sample is used. 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 one krone increase in the prize causes a NOK γ^h increase in debt at horizon h, with γ^0 yielding the contemporaneous debt response. Taken

[17] 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.

[18] The most obvious competeing model to the one presented in Equation 1 is one without the lagged dependent variable. In the robustness section, Section 5.1.2, I scrutinize how the lagged dependent variable on the right-hand side in my regression model affects my results, keeping in mind the "Nickell bias" that may arise when including a lagged dependent variable as part of the controls in a fixed-effects model (Nickell (1981)). Results in Table A.3 show that my results are robust to this change in model, and that coefficients are, if anything, larger when excluding the lagged dependent variable. Furthermore, in the typical local projection regression, several lags of the dependent variable and shock variable are included. Since there is only one shock/treatment per household, lags of the shock is obviously superfluous. Adding 2- and 3-year lags of the dependent variable to the model does not affect either the coefficient estimates or the statistical significance, and is therefore not included. These results are reported in the Appendix Table A.4.

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). 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.[19] 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 is increasing (decreasing) in the prize size. Fagereng et al. (2021) find that lottery winners' consumption share is decreasing in the amount won. A negative $Lottery_t^2$ would be consistent with this finding, assuming that the neighbors' responses are monotonically increasing 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 in 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 household size (i.e., number of adults and children) ($Household\,size$), a second-order polynomial on age of the oldest individual in the household (Age^2), and the contemporaneous and 2-year lags of a dummy variable equal to one in the year a household moves, and zero otherwise.[20] The latter controls are included to capture large movements in debt associated with house purchases that induce noise in the estimates. Lastly, I control for 1-year-lagged values of household net income ($Income$), bank deposits and cash ($Deposits$), mutual funds, stocks and bonds ($Stocks\ and\ bonds$), and taxable gifts and inheritance received over the course of a year ($Inheritance$).[21] 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.

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

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

[20]Recall that, with household fixed effects and time fixed effects, the age is perfectly collinear in time fixed effects

[21]Education level and the size of the street, meaning the number of buildings and/or houses on the street, might seem relevant to include in the controls. These variables are not included because there is either no or very little within-household variation in them, and they are therefore captured by the household fixed effects. Their effect on peer effects, however, are estimated and commented on in the Section 5.2.1, where I analyze interaction effects, and in Section 5.2.2, where I analyze the effect of neighborhood characteristics.

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 characteristics and financial position (income, debt, wealth); (2) *neighborhood characteristics*, meaning the distance between the winners' residence and the neighbors' residence and the mode of living (apartments or houses), 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:

$$Debt_{ixt+h} = \beta_0 + \beta_1 Debt_{ixt-1} + \beta_2 \mathbf{X}_{it-1} + \gamma^h Lottery_{xt} + \delta Lottery_{xt} \# z_{it-1} + \alpha_i + \tau_t + e_{it} \quad (2)$$

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 size is 2. Thus, we interpret γ as the average debt response among families consisting of two people, whereas the interaction term, δ, is the added effect of increasing household size by one member.

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 is decreasing in 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.[22] 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.[23]

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,

[22]This approach to estimating social proximity is close to the framework suggested by Glaeser, Sacerdote, and Scheinkman (2003b).

[23]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.

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.[24] 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 winner and a neighbor have been living on the same street, hereafter referred to as their "common 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 the winner and each neighbor have been neighbors at the time of treatment.[25] I use this variable to split the data into quartiles of common 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.

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

[25]That is, common tenure = min(winner's tenure, neighbor's tenure).

[26]The quartiles are < 4 years, 4–8 years, 9–17 years, and > 17 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. With the exception of *Household size*, all coefficients are essentially 0 and not statistically significant.[28] 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 with p-values above 0.35. 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 to the identifying assumption. Results with pretreatment responses among the neighbors are presented in Section 5.1.1 and Figure 3. But it is worth noting the main take-away from Figure 3, 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 Main 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 four models: the small-prize sample with and without time-varying controls (Columns 1 and 2), and the big-prize sample with and without time-varying controls (Columns 3 and 4). Since the estimated coefficients on the time-varying controls are not of any interest by themselves, they are not listed in the tables. 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.

[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 our observables.

[28]Household size apparently affects the intensity of treatment with a significance level of 10 percent. The coefficient, however, suggests that one extra household member increases the treatment by NOK 183, which is a very small effect. It is not a far reach to assign the size and significance of this covariate to random chance.

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	0.519	0.000	-0.604
	(1.28)	(0.90)	(0.09)	(-0.48)
$Householdsize_{t-1}$	0.001	182.097+	0.001	48.790
	(1.40)	(1.68)	(0.73)	(0.17)
$Moved_{t-1}$	-0.003	-65.722	0.007	895.806
	(-0.99)	(-0.14)	(1.24)	(0.51)
$Income_{t-1}$	0.000	0.000	0.000	0.000
	(0.51)	(0.49)	(0.28)	(0.67)
$Deposits_{t-1}$	-0.000	-0.000	-0.000+	-0.000
	(-0.67)	(-0.55)	(-1.90)	(-1.50)
$Stocks\ and\ bonds_{t-1}$	-0.000	-0.000	0.000	0.000
	(-1.41)	(-0.91)	(0.52)	(0.51)
$Inheritance_{t-1}$	-0.000	-0.000	-0.000	0.000
	(-0.95)	(-0.14)	(-0.01)	(0.10)
$Debt_{t-1}$	0.000	-0.000	-0.000	-0.000
	(0.46)	(-0.81)	(-0.82)	(-1.23)
$Constant$	0.046***	4007.767**	0.068***	18600.751***
	(4.57)	(2.71)	(6.79)	(6.03)
N	1 936 287	1 936 287	840 977	840 977
adj. R^2	0.006	0.002	0.006	0.003
F (prob > F)	1.12 (.35)	0.58 (.80)	1.03 (.40)	0.58 (.80)

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.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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	
$Lottery_t$	0.020***	0.021***	0.015***	0.017***
	(0.004)	(0.005)	(0.004)	(0.004)
Time-varying controls	No	Yes	No	Yes
N	843 341	659 976	312 967	253 401
adj. R^2	0.260	0.254	0.386	0.401

*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 a lagged dependent variable ($Debt_{t-1}$), as well as 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, household size, contemporaneous and two lags of dummy capturing year of moving, and lagged values of income, deposits, stocks and bonds and inheritance, see Section 4. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Column 1 reports a coefficient of 0.020, meaning that in the year that the winner wins, neighbors on average increase debt by two percent of the prize won (e.g. for a lottery prize of NOK 10 000, neighbors on average increase debt by NOK 200). This estimate is statistically significant at significance levels below 0.1 percent. Adding time-varying controls to the regression does not change statistical significance, and the estimated response is minimally increased to 2.1 percent. From the perspective of identification, it is reassuring that including predetermined controls does not affect the treatment response.[29] Standard errors at 0.5 percent imply that with 95 percent probability, the true debt response lies between 1.1 and 3.2 percent. Column 3 is the parallel of Column 1, but for the big-prize sample. The estimated debt response drops to 1.5 percent of the winner's prize. As with the small-prize sample, adding controls (Column 4) affects the point estimate marginally, as it increases to 1.7 percent. Both estimates are statistically significant. Thus, we see that the average debt response drops by approximately 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.[30] For the average lottery prize in the small-

[29] That estimates are unaltered when including time-varying controls in the model suggests that if any other unobservable variables are driving the estimates — i.e., affecting both the winners' likelihood to win *and* neighbors' debt — they must be doing so to a much larger degree than the observables do, which seems unlikely.

[30] 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,

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
$Lottery(0/1)_t$	6403.4***	6229.8***		
	(768.9)	(1498.9)		
$Lottery_t$			0.0709***	0.0341**
			(0.0102)	(0.0108)
$Lottery_t^2$			-9.55e-08***	-2.86e-08^{+}
			(1.58e-08)	(1.60e-08)
N	659 976	253 401	659 976	253 401
adj. R^2	0.254	0.401	0.254	0.401

*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 a lagged dependent variable ($Debt_{t-1}$), as well as 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, household size, contemporaneous and two lags of a dummy capturing year of moving, and lagged values of income, deposits, stocks and bonds and inheritance, see Section 4. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

prize sample (≈ NOK 90 000) a coefficient of two percent amounts to an average increase in debt of NOK 1800 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 0.5 percent increase in debt. Similar back-of-the-envelope calculations for the big-prize sample suggest an increase in debt of NOK 3900 for average prizes (≈ NOK 260 000), or a one 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 is decreasing in 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).

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.

The discrete model estimates that within the small-prize sample, the average effect on debt is NOK 6400. The corresponding estimate is somewhat smaller in the big-prize sample (NOK 6200). 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.[31] 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 2.1 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 4860 to NOK 7900 in the small-prize sample, and from NOK 3200 to NOK 9200 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 0.07, or a seven 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.[32] Unsurprisingly, both the estimates of the main coefficient (0.034) 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.[33]

[31] That is, $6400/390000 \approx 7$ percent in the discrete model, compared to 2.1 percent in the baseline (linear) model.

[32] For the smallest prizes in the sample, 10 000, the added krone value from $Lottery_t^2$ is -9.55, such that the estimated krone effect for these prizes is (10 000*0.07–9.55 =) NOK 690.

[33] Figure A.2 in the Appendix plots the implied krone values for all prizes, for both the small- and the big-prize samples. They show that for both 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 500 000. At most, the non-linear model suggests an average increase in debt of about NOK 13 000 at prizes close to NOK 400 000, after which the NOK response is declining as prizes increase. In an earlier version of this paper, using the big-prize sample but with no upper bound on prizes and years from 1994 to 2011, the non-linear model was not discussed. Instead the paper presented results with a "kink" in the effect around NOK 1 million, where there was essentially a zero effect. The non-linear graph in Figure A.2b is consistent with this finding. It shows that as the prizes go toward 1 million, the implied krone value decreases toward zero, implying a coefficient close to zero around NOK 1 million.

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 affect 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 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 two 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 up 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 of course be entirely speculative. Still, we should keep in mind that the average debt response among the neighbors who do take up debt is quite likely higher than two percent of the prize.

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.[34]

[34]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. A second reason is that we do not observe consumption directly in the data, and need to impute expenditure from the observed balance sheet.

If, however, one accepts the assumption that the income shock of the winner affects the debt behavior of the neighbors *through the observation of the winner's expenditure response only*, I can do IV regressions with the winner's expenditure instrumented with the lottery prize (i.e., $Lottery_t$). Appendix Table A.9 reports estimates from such IV regressions, where control variables and sample selection are otherwise identical to those of the reduced-form estimates in the rest of the paper. Columns 1 and 2 report the first- and second-stage estimates for the small-prize sample. Columns 3 and 4 similarly report the first- and second-stage estimates for the big-prize sample. First-stage coefficients are as before 0.45 and 0.38, with F-statistics at ten or above. Coefficients in the second stage are largely in line and consistent with the back-of-the envelope calculation in the upcoming paragraph. According to the small-prize estimates (Column 2), the winners' expenditure hike increases neighbors' debt by 4.3 percent, in terms of the winners' (predicted) expenditure

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.[35] I estimate that the winners in my sample spend 45 percent of the winning prize the year they win, with a standard error of 3.[36] The expenditure response for winners in the big-prize sample is 0.35, with a standard error of 0.03.

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 ($0.021/0.45$ =) 4.7 percent in the small-prize sample, and (0.017×0.35 =) 4.8 percent in the big-prize 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 reported in Section 5.1, this is consistent with the finding in Fagereng et al. (2021) that winners' expenditure is decreasing in the prize size.[37]

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.

Figure 3 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.

increase. The IV coefficient is significant at the one percent significance level (p-value = 0.003). For the big-prize sample, the IV-coefficient is 3.8 percent (p-value = 0.004) of the winners' expenditure response.

[35] In short, 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)).

[36] 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 are overlapping. The difference is due to a smaller fraction of small-prize winners in my sample.

[37] An alternative explanation for the non-linear effect is that lottery winners are in fact sharing 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.

To be precise:

$$Debt_{ixt} = \beta_0 + \beta_1 Debt_{ixt-1} + \beta_2 \mathbf{X}_{it-1} + \gamma^j Lottery_{xt+j} + \alpha_i + \tau_t + e_{it} \tag{3}$$

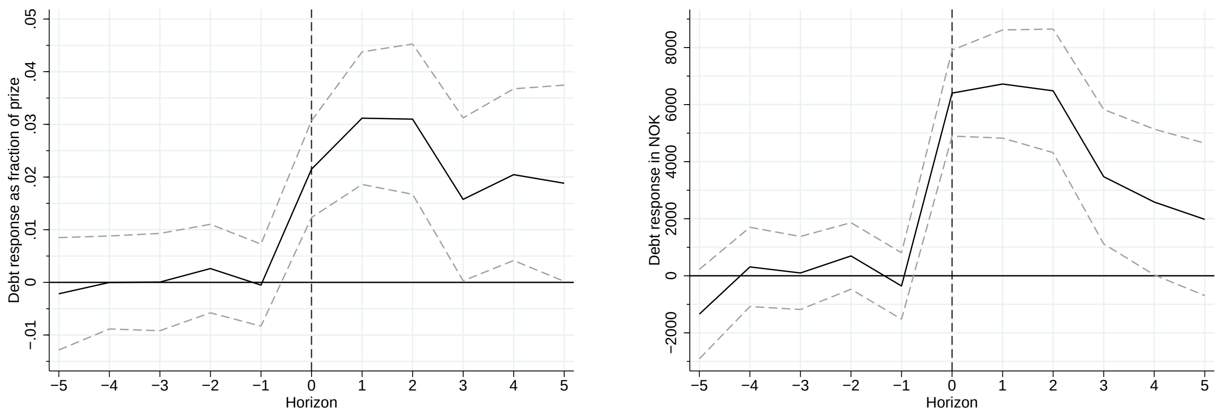
where γ^j is the estimated responses to the winners' future prizes, j periods ahead ($Lottery_{xt+j}$). I estimate Equation 3 for $j = 1$ to $j = 5$. All other variables are as in Equation 1.

Plotting these 11 γ coefficients together produces the impulse response function (IRF) 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). Panels (a) and (b) show results for the linear and the discrete treatment variables, with the debt response as a fraction of the prize and the debt response in NOK on the Y-axis, respectively.[38] Together with the point estimates (solid lines), I plot the 95 percent confidence intervals (dashed lines). Both IRFs in Figure 3 are for the small-prize sample. A parallel figure plotting the (placebo) treatment effect for the control group (described in section 3.3), is reported in the Appendix, Figure A.3. That figure shows no sign of a "treatment response" for this untreated group.

Three features of Figure 3a and 3b 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 either of the figures. Second, Figures 3a and 3b both visualize the significant and sharp debt response in the treatment year (i.e., at X-axis = 0). These are the same point estimates as those reported in Table 4 and Table 5. The third and final feature is that they both show a persistent debt effect: Both the discrete and the linear model estimate positive and significant debt effects up to three or four years after the treatment year. In the year after treatment (X-axis = 1), the linear model (Figure 3a) 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 period $t + 1$. The discrete model shows estimates that are marginally different at horizons zero, one and two. Both models show that three periods after the shock, debt levels start falling. Although estimates are less clearly 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 on for several years.

[38] 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 3: Dynamic debt responses before and after treatment.



(a) Continuous treatment

(b) Discrete 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. Panel (a) reports point estimates of the continuous treatment variable $Lottery_t$*, and the Y-axis reports the debt response as a fraction of the winner's prize. Panel (b) 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 panel (b) 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 linear, continuous treatment, and with a sphere of influence equal to ten.*

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 0.021, 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.4.[39]

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. Results are overall the same, 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 0.024) and reduced (to 0.016), 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 somewhat (to 0.025). 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 marginally affected, if at all (0.023 and 0.021, respectively). Finally, in Column 8 (labeled "Top 1%") the sample includes all households, except those that are simultaneously in the top one percent of all three distributions discussed. The estimate is heightened to 0.03. I conclude that, if anything, my baseline estimate is pulled down by my relatively moderate trimming on these variables.

The final two columns provide results where I first include households living in the same building as the winner (Column 9) in my estimated model with a sphere of influence of 10, and then widen the sphere of influence from ten to 20 houses away from the winner (Column 10). The former adjustment changes the estimated debt response minimally, to 0.020. The point estimate for the sphere of influence of 20 is 0.018. It should be noted that this estimate is probably misleading in suggesting that neighbors at the farthest distance increase debt by 1.8 percent of the prize. The reason is that the additional observations added to the sample beyond a distance of ten are few

[39]Since inference is not affected by the alternations, I will not make specific comments on standard errors in the detailed description below.

(roughly 15 percent of the total), and that this estimate is therefore predominantly driven by the ten closest neighbors.

Model robustness In the main part of the paper, I follow the local projections approach suggested by Jordà (2005), i.e., with debt in levels on the left-hand side and the lagged value of the debt level on the right-hand side in Equation 1, in addition to other controls. 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 and Table A.4. Both tables report results for the small-prize sample (labeled SPS in the tables) and big-prize sample (labeled BPS in the tables).

Columns 1 and 2 in Table A.3 show that when I drop $Debt_{t-1}$ from the regression as part of the controls, the estimates increase to 2.9 and 2.7 percent for the small- and big-prize samples, respectively. An interaction effect between debt response and initial debt levels, discussed in the next section, where higher initial debt decreases the debt response, explain this result (see the discussion in Section 5.2.1 for details). Importantly, the next section shows that the debt response is smaller at the average debt levels than in the baseline estimates. This difference implies that the average debt-response estimate is pushed up by the responses in the lower part of the debt distribution. Hence, by not controlling for the households' debt levels before treatment, the point estimate for the average response will also be pushed up.

With a fixed effects model and a lagged dependent variable on the right-hand side, as I have in my main specification, estimates may be biased (Nickell (1981)). Notably, since the treatment is a one-time shock, and arguably strictly exogenous, the Nickell bias should be small. Arellano and Bond (1991) suggest using an IV approach with longer lags of the dependent variable as instruments to bypass this problem. However, since my errors are serially correlated, this IV estimator also suffers from the same endogeneity problem, if there exists one to begin with. Thus, this estimator does not really add any information. Angrist and Pischke (2008) offer a pragmatic solution when this is the case, and when both fixed effects model and a lagged-dependent-variable model seem relevant. They suggest comparing estimates from a pooled OLS model that includes the lagged dependent variable in the controls (but no household fixed effects), with a fixed effects model that does not include the lagged dependent variable. The key observation in Angrist and Pischke (2008) is that if the former model is wrong, it will produce too small estimates, and if the latter model is wrong, it will produce too high estimates, and that they therefore provide the upper and lower bounds for the treatment effect. As already commented, the fixed-effect model without $Debt_{t-1}$ yields estimates of 2.9 and 2.4 percent for the small- and big-prize samples, respectively. The pooled OLS regressions with a lagged dependent variable are reported in Columns 6 and 7. Point estimates are lower, at 1.8 and 1.4 percent for the two samples. Thus, my main estimates of 2.1 and 1.7 conform well to these suggested lower and upper bounds of 1.8 to 2.9 for the small-prize sample, and 1.4 to 2.4 for the big-prize sample.

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 smaller (0.015 and 0.012), but still significant at the 1 percent significance level, indicating a significant debt response, nonetheless.

Finally, in the local projections approach, it is common procedure to include several lags of the dependent variable and the shock. In this paper, each household experiences one shock only, rendering lags of the treatment superfluous. In the final robustness test, I include additional lags of the dependent variable, $Debt_{t-j}$, to the control variables. Results, reported in A.4, show that the main estimates for both the small- and big-prize samples are virtually unaffected by including either two or three lags of debt in the regression model.

5.2 Determinants of peer effects

I turn next to the analysis of determinants, or heterogeneity in peer effects. If the estimated debt responses presented thus far are indeed peer effects, we would expect them to vary with the degree of homophily, or the likelihood of friendship among winners and neighbors. In addition to providing a novel analysis of various factors influencing peer effects, the main contribution from this section is therefore to investigate whether my estimates are consistent with a peer effect. The results presented in this section support this interpretation: All factors that plausibly reflect stronger social ties between the winner and a neighbor produce higher point estimates.

I present the results in three main sections: (1) interactions with households' time-varying controls, (2) neighborhood characteristics, including the distance between neighbors and winners and the type of building they reside in, and (3) measures of homophily, which include the household structures of neighbors and winners, and their common 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 and financial position before treatment

Table 6 reports results from eight individual regressions as presented in Equation 2. Each regression is presented in a row, with the interaction variable in question 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. The interaction variables are the control variables in the vector $\mathbf{X}$, in addition to a categorical variable capturing $Education\, level$. This variable takes on three values depending on years of schooling of whoever has the most years of education in the household: 1 for 1–10 years of education, 2 for 10–12, and 3 for more than 12 years of education. To ease interpretation, interaction variables are centered at their mean, and the estimated coefficients on the financial variables are multiplied by 100 000 in Table 6. Financial variables are one-year lagged values, to avoid endogenous effects.

Table 6: Interaction of debt response with household characteristics and household financial variables

	Main coefficient $Lottery_t$	Interaction term z_{t-1}
Interaction variable:		
$\#Age_t$	0.023***	-0.0006**
	-0.005	-0.0002
$\#Household\ size_t$	0.017***	0.0106**
	-0.004	-0.00372
$\#Debt_{t-1}$	0.015**	-0.006***
	-0.006	-0.002
$\#Income_{t-1}$	0.024***	0.007
	-0.006	-0.004
$\#Deposits_{t-1}$	0.021***	-0.000
	-0.005	-0.000
$\#Stocks\ and\ bonds_{t-1}$	0.023***	0.009
	-0.005	-0.008
$\#Liquid\ wealth_{t-1}$	0.022***	0.000
	-0.005	-0.001
$\#Education\ level_{t-1}$	0.021***	0.010
	0.005	0.007
N	659 976	659 976
adj. R^2	0.254	0.254

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 are centered at the mean within the sample. Financial variables are lagged values.* $Household\,size$ *is centered at 2. Age is centered at 54.* $Education\,level$ *is a categorical variable that takes on three values: 1 for 1–10 years of education, 2 for 10–12 years, and 3 for* $>$ *12 years of education.* $Education\,level$ *is centered at 2. 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.*

$+\ p < 0.10$, $*\ p < 0.05$, $**\ p < 0.01$, $***\ p < 0.001$.

Three models with interaction terms show a statistically significant effect of their respective interaction term: Age_t, $Household\ size_t$, and $Debt_{t-1}$. The first row suggests that the debt effect decreases by age. The average age in the sample is 52 years, such that above this age level, debt responses decrease by 0.06 percent for each year. The household-size variable is centered at 2. Compared with the baseline estimate (2.1), the main coefficient is smaller, 1.7 percent. The interaction term suggests that families consisting of three (in most cases indicating at least one child) increase debt by one percentage point to 2.7 percent, or about 65 percent more, than do 2-person families. Finally, the regressions with an interaction term on debt the year before treatment show a clear negative effect of higher debt before treatment. First, we note that the main coefficient in Column 2 is 0.015. This means that at mean debt levels (approximately NOK 390 000), the debt response is 1.5 percent of the winner's prize. The interaction term is small, but significant: NOK 100 000 extra in debt before treatment reduces the peer effect by 0.6 percentage point. Together, the lower average estimate in this model compared to the baseline, and the negative sign on the interaction term, tell us that the estimated treatment effects come from the lower part of the debt distribution. One possible reason is that households farther away from the debt limit (less credit constrained) are more able to respond to peer effects. The four interaction terms ($Income_{t-1}$, $Deposits_{t-1}$, $Stocks\ and\ bonds_{t-1}$, and the sum of the latter two, $Liquid\ wealth_{t-1}$), are insignificant. Note also that the main coefficient reported in Column 2 is virtually unaffected by including these terms. Finally, the estimated coefficient on *Education level* is 0.01, which indicates a larger debt response among the higher educated. The point estimate is, however, not statistically significant.[40]

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-unit houses) and apartments. Both factors affect the degree of homophily on the street and the ability of neighbors to observe the winners' income shocks. 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.

[40] Changing the specification of interaction, by using *Education level* as an indicator variable, does not change this conclusion.

Distance All estimates to this point 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 4 reports the point estimates for separate regression models where the sphere of influence is set to one, two, three, four, and ten, respectively.[41] 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. In panels (a), (b) and (c) I report estimates for the linear model, the non-linear model and the discrete model, respectively. The Y-axes of (a) and (b) report the debt response as a fraction of the winners' prize, and the Y-axis of panel (c) reads as the average NOK response from a discrete treatment. The point estimates reported in panel (b), the non-linear model, are those on $Lottery_t$, ignoring $Lottery_t^2$, such that the point estimates reported are close to that for the smallest prizes in the sample.

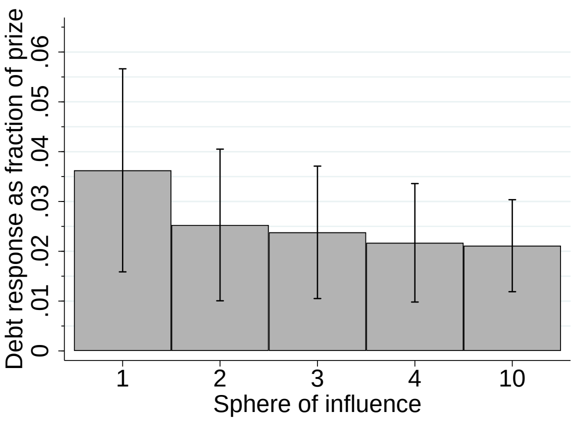
Figure 4 shows that all three models produce the same qualitative results. In all three models, the neighbors at a sphere of influence equal to one, i.e., those living next-door to the winner, have a higher estimated debt response than do neighbors farther from the winner, although we note that the confidence bands are wide and overlapping across all distances. The linear model estimates a response of 3.8 percent, the non-linear model estimates a response of about 11 percent (at approximately the smallest prizes), and the discrete model estimates an average increase in debt of NOK 9000. Compared with the estimates at the sphere of influence of 10, depicted in the right-most bars in each figure, these estimates are 40 to 80 percent higher. Estimates including neighbors at distances two, three, and four are also consistently lower than those including only the closest neighbors (i.e, at distance equal to one), and are higher than those at ten in all models (≈ 10 percent higher). The models show an almost monotonically decreasing effect as the sphere of influence is widened, but overlapping confidence bands imply that these differences are not statistically significant. Still, Figure 4 points to debt responses that are consistent with the expectation that peer effects are stronger at the closer distances. It is also worth noting that in all three models, the point estimate for the closest neighbors (at X-axis = 1) lies above the upper limit on the 95 percent confidence bands on the estimate for the model including neighbors farthest away (at X-axis = 10). Kuhn et al. (2011) found similar stronger peer effects for the next-door neighbors.

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 simply be that a higher share of neighbors at that distance respond (the extensive margin).

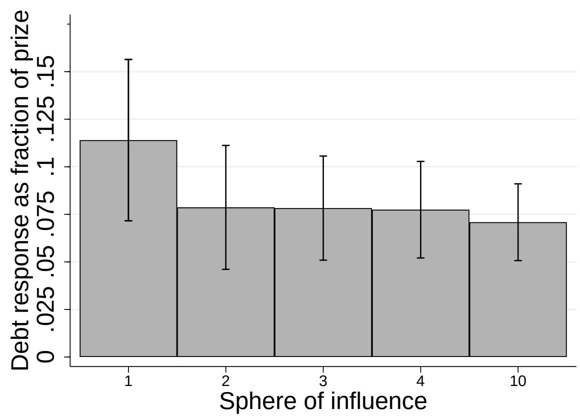
Houses versus Apartments Next, I investigate is how peer effects differ for households living in Houses and Apartments. The tendency for stronger social ties among neighbors living in houses than among neighbors living in apartments was clear in survey data in Sudman (1988). In addition to affecting the formation of social ties, the urbanization level of a street might be reflecting the

[41] Neighbor distances from five to nine are not reported because they are essentially overlapping with the estimates from three to ten. Results are available on request.

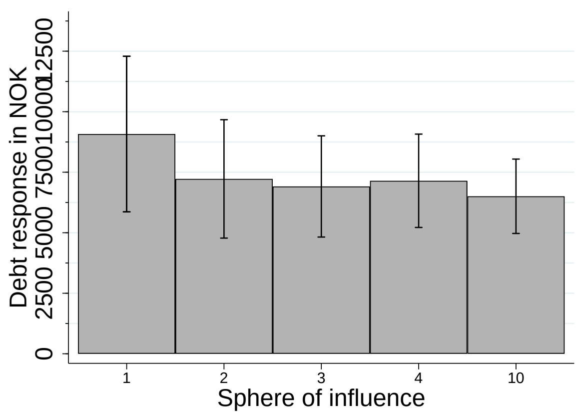
Figure 4: Debt responses by sphere of influence. Linear, non-linear and discrete models.



(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 7: 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
$Lottery_t$	0.024**	0.018***	0.029***	0.028***
	(0.007)	(0.005)	(0.007)	0.007
$Lottery_t * Apartments(0/1)$			-0.016+	-0.013
			(0.009)	(0.009)
N	337 303	322 673	659 976	659 976
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.
$+ \, p < 0.10$, $* \, p < 0.05$, $** \, p < 0.01$, $*** \, p < 0.001$

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, just as with distance, 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 7 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 2.4 percent of the prize, compared to 1.8 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 2.9 percent of the lottery prize, and the latter is estimated to be 1.3 percent of the lottery prize. However, the interaction term is significant only at a 7 percent significance level. The fully interacted model that takes into account systematic differences between households living in houses and those living in apartments produces very similar point estimates, but the estimated interaction coefficient is now some way apart from conventional levels of statistical significance (p-value 0.13). One reason for the insignificant difference between these samples could be the noisy separation between them due to the many missing building codes. The sample of apartments almost certainly includes some residences that are in fact single-unit dwellings.[42,43]

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 (common 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 as neighbors. To shed light on this issue, I run regressions on each quartile of the measured common tenure, i.e., how many years the winner and neighbor have been neighbors. The quartiles have thresholds of common tenure at < 4 years, 4–8 years, 9–17 years, and > 17 years.

[42]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 likely to be limited, whereas contact with the next-door neighbor living in a single-unit house is likely to be relatively frequent. Table A.6 in the Appendix explores this hypothesis by the same approach as in the main text, but with a sphere of influence equal to 1. The results are even stronger effects for the closest neighbor if they live in a House, compared with the average effect for households at this distance presented in Figure 4. The point estimate is 0.047 in the Houses subsample, compared with 0.036 for the model that do not distinguish between building types (see Figure 4), and 0.024 for the Apartments subsample. Interaction terms are not significant, but overall the results support the idea that mode of living matters for peer effects.

[43]A third and related structural factor is the size of the streets. I measure network size as the number of households living within a sphere of influence of ten in the treatment year, and estimate the effect of network size by including this variable as an interaction term, as in Equation 2. I center the size-variable to its mean, which is 22. The interaction term is positive, and borderline significant with a p-value of 0.052. The point estimate is 0.0007, indicating that increasing the size of the network by ten additional households above the mean increases the effect by 0.7 percentage points. The main coefficient (measuring the main effect at the mean of size) is unaffected by the inclusion of this interaction term, at 0.022. Thus, if anything, it seems that peer effects increase slightly by the number of households. Note that this is a crude measure because a higher number of households living on a street does not necessarily reflect a higher degree of density or of urbanization per se. Results are reported in the Appendix, Table A.5.

Table 8: Debt response by quartiles of the winner's and neighbors' common tenure on a street.

	1st quartile	2nd quartile	3rd quartile	4th quartile
$Lottery_t$	0.009	0.026**	0.020*	0.019*
	(0.012)	(0.009)	(0.010)	(0.009)
N	119 180	162 040	179 732	199 024
adj. R^2	0.242	0.187	0.268	0.312

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions are for the small-prize sample, and with a linear continuous treatment. Common tenure of winner and neighbor is defined as the number of years a winner-neighbor pair has been neighbors at the time of treatment. 1st quartile of common tenure: < 4 years; 2nd quartile: 4–8 years; 3rd: 9–17 years; 4th: > 17 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. Estimates are for the small-prize sample, with a linear, continuous treatment, and with a sphere of influence equal to ten.

$+ \ p < 0.10$, $^{*} \ p < 0.05$, $^{**} \ p < 0.01$, $^{***} \ p < 0.001$

Results are reported in Table 8. Estimates of the debt response in the first quartile show a small (0.009) and statistically insignificant effect for "new neighbors" with a Common tenure of less than four years. Hence, we cannot reject that the effect for these neighbors is 0. 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 in the same ballpark as in the baseline model (i.e., 2.1 percent). The third and fourth quartiles have point estimates of 2.0 and 1.9 percent, respectively. 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, rendering the differences between them statistically insignificant. However, 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.[44]

Household structure 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, on the other hand, 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, I test whether *the Aligned household structure* of the neighbors and the winners creates stronger peer effects.

[44]Table A.7, which uses simply the neighbors' tenure on the street, irrespective of the winners' tenure, confirms these findings, and makes them even more pronounced. See Appendix Table A.7.

Table 9: 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
$Lottery_t$	0.016**	0.030***	0.019**	0.015**
	(0.006)	(0.008)	(0.006)	0.006
$Lottery_t * Aligned(0/1)$			0.007	0.016+
			(0.009)	(0.010)
N	384781	275195	659976	659976
adj. R^2	0.244	0.264	0.254	0.254

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.
$+ \ p < 0.10$, $* \ p < 0.05$, $** \ p < 0.01$, $*** \ p < 0.001$

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 18 years old 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."[45] 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 9 reports the results. The regressions with the subsamples estimate debt responses to 1.6 percent among households with an unaligned household structure, and to 3 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

[45]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, adult career-minded individuals to widows.

families than for unaligned families, although we should note that the 95 percent confidence bands of these two estimates are overlapping. Column 3 reports a smaller effect, with a coefficient on the interaction term that implies that aligned neighbors respond 0.7 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 estimated close to that in the subsamples (0.016). This estimate is significant at a 10 percent significance level. 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. Yet, even if the differences are not statistically significant at conventional acceptance levels, it seems likely that similarity when it comes to household structure matters.

5.3 Effects on income, liquid assets and imputed expenditure

Why do neighbors take up debt in response to the winner's income shocks? A natural extension of my analysis on 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 in consumption may be financed by increased labor income, reduced savings or increased debt. But the reverse 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 liquid assets, in addition to households' imputed expenditure.[46,47] Since the imputed expenditure variable contains noise (at least compared to the noise in the other observable variables), I present results for both the original small-prize sample (see Section 3.2 for details) and the untrimmed expenditure variable, and 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.[48] The regression model is identical to that of Equation 1, except that the dependent variable is replaced by $Income_t$, $Liquid_t$ and $Expenditure_t$. As before, I include the lagged dependent variable in the set of controls.

[46] I sum the stock of deposits and stocks and bonds to the variable *Liquid assets*. Results are similar if I include them as separate items, in separate regressions.

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

[48] Trimming the top and bottom 5 percent of the sample gives essentially the same results.

Table 10: 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	0.021***	0.031***	0.031***
	(0.005)	(0.006)	(0.007)
Income	0.001	0.001	-0.001
	(0.002)	(0.002)	(0.002)
Liquid assets	-0.001	-0.006	-0.009
	(0.004)	(0.005)	(0.006)
Expenditure†	0.013*	0.013+	-0.005
	(0.006)	(0.007)	(0.009)
Expenditure‡	0.016***	0.015**	0.005
(*trimmed sample*)	(0.005)	(0.006)	0.006
N	659 976	641 677	617 901
N trimmed sample	615 327	598 014	575 322

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 a lagged dependent variable, time-varying variables as described in Section 4, and household fixed effects and time fixed effects. Estimates are for a sphere of influence equal to ten.*
$+\ p < 0.10$, $*\ p < 0.05$, $**\ p < 0.01$, $***\ p < 0.001$

Table 10 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 3a. The first take-away from the table is that neighbors do not change income (Row 2) or liquid assets (Row 3) in the years following the winners' prize. Point estimates are essentially 0, and insignificant. Thus, the households' budget constraint implies that the debt neighbors acquire finances higher spending.

Row 3 reports the estimated effects on the imputed expenditure of neighbors, without any extra sample restrictions. It shows a marginally significant effect (p-value 0.038) in the treatment year: Neighbors increase spending by 1.3 percent of the winners' prize. This flow of increased spending is upheld in the year thereafter, with an additional 1.3 percent (p-value 0.075). At $t+2$ there is no sign of any effect on expenditure, suggesting that spending levels are back to pretreatment levels. In terms of total spending over the two years, it amounts to 2.6 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 (see the estimates in row 1: The stock of debt is 2.1 percent higher at time t and 3.1 percent higher at time $t+1$.), and note that a zero response in $t+2$ is consistent with the stable debt levels implied by the unaltered debt estimates between $t+1$ and $t+2$.

Moving to the final row, where the sample is winsorized at the top/bottom one percent of expenditure, this picture is even clearer. These estimates are broadly similar to those in the untrimmed sample but less noisy, as expected. Point estimates increase to 1.6 and 1.5 percent in t and $t+1$, respectively, bringing the total spending even closer to that in the debt-level responses. Estimates are significant at (at least) a one percent significance level. As before, spending responses at $t+2$ are estimated to be closer to zero and insignificant, in line with unchanged debt levels.

6 Conclusion

I have conducted an empirical micro-level investigation of the causal link between the income of one household, and debt accumulation of its neighbors. 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 signs of pretreatment responses and observables are unable to predict timing and intensity of treatment. 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 2.1 percent increase in debt, measured in terms of the lottery prize. A non-linear model suggests that the effect decreases in the prize size, with a 7 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.

I back up my baseline finding by showing that my estimated debt responses vary with observable characteristics that are likely to influence 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). I find that age, household size and pretreatment debt levels affect the magnitude of the response. Debt responses increase by household size and decrease by age and initial debt level. There is some evidence that suggests that average debt increases more among the very closest neighbors than among more distant neighbors, although these differences are not statistically significant. My results also show a tendency for stronger peer effects among neighbors living in single-household dwellings (houses) than among households living in apartments, and among neighbors with a similar (aligned) household structure to that of the winner than among neighbors with a dissimilar (unaligned) household structure to that of the winner. Peer effects are weaker (or even nonexistent) among neighbors with a relatively short tenure as neighbors of the winner. Finally, I show that estimated expenditure responses support the interpretation that neighbors take on debt to finance increased spending.

In total, it is worth underscoring that, although not always statistically significant, all my results on the heterogeneity of peer effects line up well with the a priori assumptions about peer effects, and therefore lend support to the overall finding that I have in fact identified a peer effect. Thus, the combined evidence from this paper points to a peer effect from income shocks to debt, implying a causal link between income inequality and debt growth. The evidence also sheds some light on the factors that form and affect this link.

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. Estimated average household-level responses are admittedly moderate in size. However, from a macro perspective, they are non-negligible for three reasons. First, the size of the neighborhoods is 22 households at its mean. Thus, in total a 2.1 percent increase in average debt is economically significant. Furthermore, it seems unlikely that all neighbors increase debt. If so, this implies that among those who do respond to the winner's income shock, debt accumulation is likely substantially higher. Second, to make identification credible, I have focused on a transitory income shock that affects one household only. Thus, the peer signal is 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 timing of debt accumulation (like expenditure shifting), but also seems relevant for longer-term debt levels.

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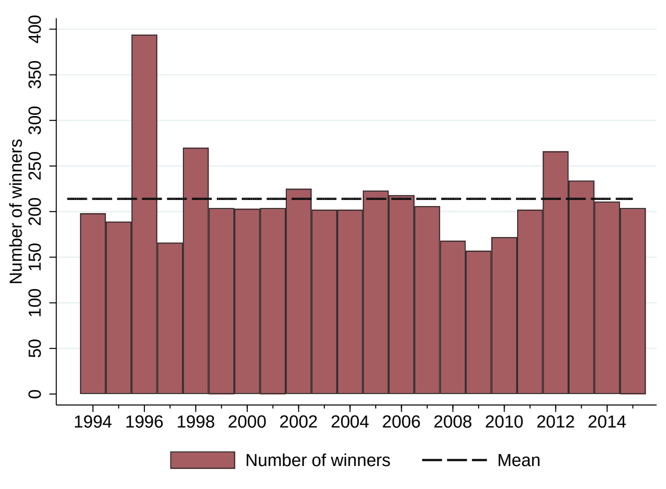
A Figures and Tables

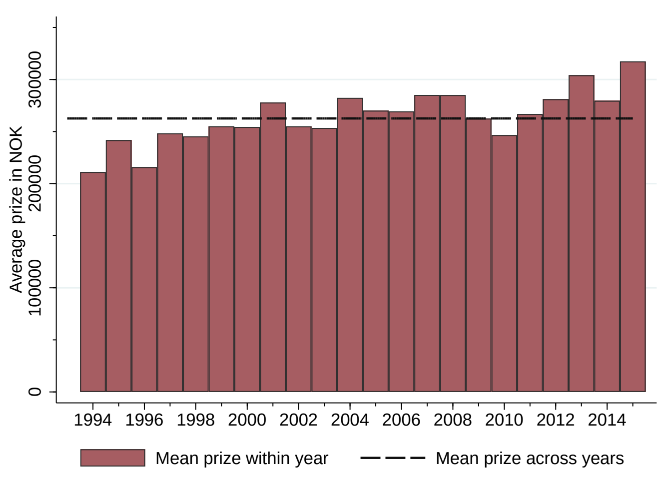
A Appendix

A.1 Extra figures and tables, robustness

Figure A.1: Number of winners and average lottery prize per year in the big-prize sample.



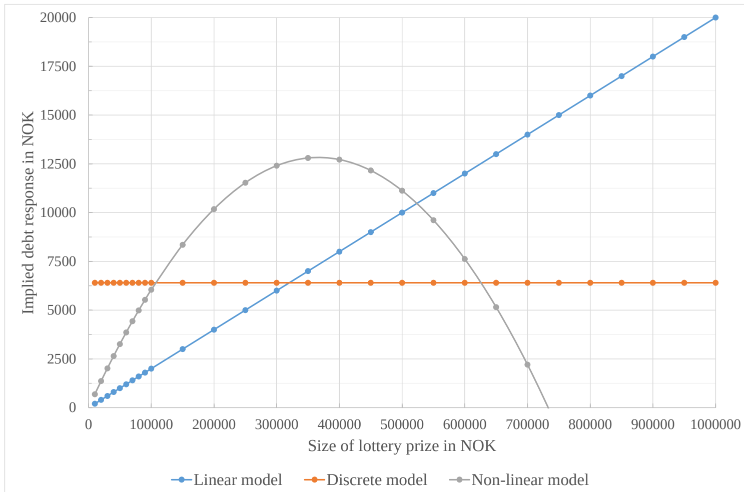
(a) Number of winners.



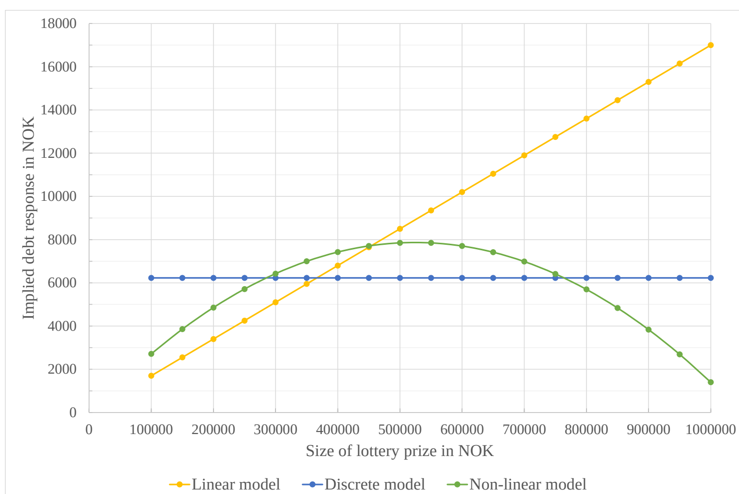
(b) Average lottery prize

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.2: Krone responses implied by linear, discrete and non-linear coefficient estimates



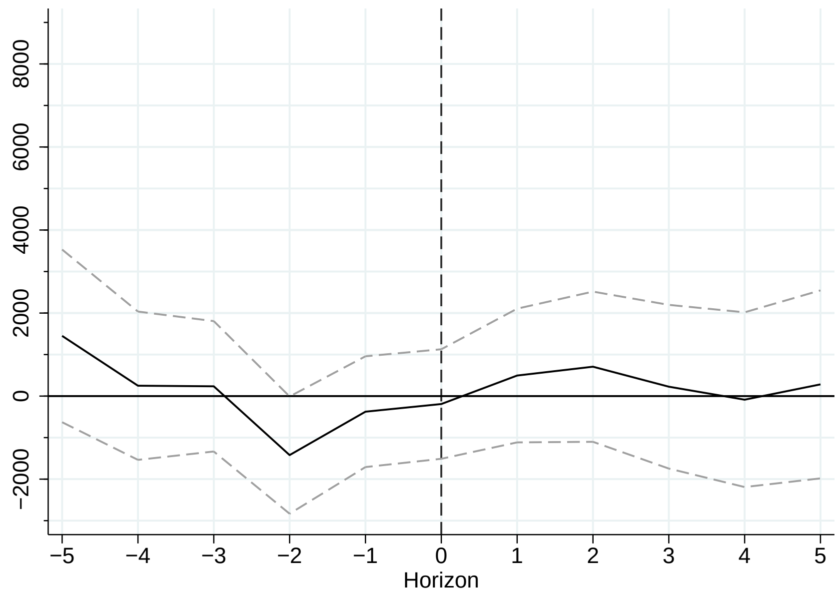
(a) Small-prize sample



(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.3: Dynamic treatment effect in control group (placebo treatment).



(a) Discrete treatment

Notes: The figures plot the point estimates of the debt response of households living in different streets but on the same zip code as a lottery winner (i.e. the control group described in section 3.3) at different horizons relative to the year a lottery winner wins. Placebo treatment is a discrete variable, that is one in the treatment year and zero otherwise. Regressions are run on a ten percent random sample from the control 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, independently of 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.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	Top 1%	distance 0	distance 20
$Lottery_t$	0.021	0.024	0.016	0.025	0.023	0.021	0.030	0.020	0.018
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)	(0.005)	(0.004)
N	659 976	657 862	661 621	672 074	723 107	667 146	761 946	696 685	696 685

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, 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.
No significance stars, all coefficients have p-values < 0.000

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	Top 1%	distance 0	distance 20
$Lottery_t$	0.017	0.018	0.014	0.024	0.017	0.016	0.027	0.017	0.018
	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)	(0.004)	(0.007)	(0.004)	(0.004)
N	253 401	249 131	255 976	260 121	278 155	256 031	295 941	267 631	267 631

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, 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.
No significance stars, all coefficients have p-values$<$0.000

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

	Model excl. $Debt_{t-1}$		Pooled OLS w/$Debt_{t-1}$		DV: $\Delta Debt_t$	
	SPS	BPS	SPS	BPS	SPS	BPS
$Lottery_t$	0.029***	0.024***	0.018***	0.014***	0.015**	0.012**
	(0.006)	(0.006)	(0.004)	(0.003)	(0.005)	(0.004)
N	659 976	253 401	673 031	267 631	659 976	253 401
adj. R^2	0.018	0.034	0.89	0.91	0.006	0.005

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions include age, household size, a dummy variable capturing that the household moves, and lagged income, deposits, stocks and bonds, and inheritance, in addition to time fixed effects. Columns 2–3 and 6–7 include household fixed effects, but not $Debt_{t-1}$ as a control variable. Columns 4–5 report pooled OLS estimators, i.e., regression models without household fixed effects, but otherwise equal to the baseline regression model, Equation 1. Columns 6–7 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.

$+\ p < 0.10$, $*\ p < 0.05$, $**\ p < 0.01$, $***\ p < 0.001$

Table A.4: Lag robustness: Sensitivity to the number of lags of debt in the regression model

	Small-prize sample			Big-prize sample		
	Number of lags of Debt			Number of lags of Debt		
	1 (baseline)	2	3	1 (baseline)	2	3
$Lottery_t$	0.021***	0.021***	0.020***	0.017***	0.016***	0.017***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)
N	659 976	659 976	509 350	253 401	253 401	205 397
adj. R^2	0.254	0.254	0.236	0.401	0.402	0.396

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. All regressions use a sphere of influence equal to ten, and the linear, continuous treatment variable. All regressions include a lagged dependent variable ($Debt_{t-1}$), age, 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. 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.

$+\ p < 0.10$, $*\ p < 0.05$, $**\ p < 0.01$, $***\ p < 0.001$

Table A.5: Neighborhood size/density

Interaction variable	Main coefficient	Interaction term
$\#Neighborhood\,Size$	0.022***	0.0007+
	-0.005	0.0003

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. N = 659 976, adjusted R^2 = 0.25. Neighborhood size is the number of households on the street living within the sphere of influence equal to ten. The #NeighborhoodSize interaction variable is centered at the mean equal to 22. 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. All regressions are for the small-prize sample, with a linear continuous treatment, and a sphere of influence equal to ten. 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.

$+ \; p < 0.10$, $* \; p < 0.05$, $** \; p < 0.01$, $*** \; p < 0.001$

Table A.6: 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
$Lottery_t$	0.047**	0.024*	0.049**	0.049**
	(0.017)	(0.011)	(0.017)	(0.017)
$Lottery_t * Apartments(0/1)$			-0.027	-0.026
			(0.020)	(0.020)
N	65 002	58 489	120 491	120 491
adj. R^2	0.240	0.214	0.238	0.238

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 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.

$+ \; p < 0.10$, $* \; p < 0.05$, $** \; p < 0.01$, $*** \; p < 0.001$

Table A.7: 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	0.024*	0.025**	0.018**
	(0.013)	(0.011)	(0.009)	(0.006)
N	86 232	152 783	189 722	231 239
adj. R^2	0.077	0.229	0.303	0.326

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. 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.

$+ \ p < 0.10$, $^{*} \ p < 0.05$, $^{**} \ p < 0.01$, $^{***} \ p < 0.001$

Table A.8: Debt response by single and non-single households

	Household members		Interaction
	Single	Non-single	Model
$Lottery_t$	0.012*	0.025***	0.028***
	(0.005)	(0.006)	(0.006)
$Lottery_t * Single(0/1)$			-0.018*
			(0.008)
N	203 519	456 457	659 976
adj. R^2	0.226	0.260	0.254

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.

$+ \ p < 0.10$, $^{*} \ p < 0.05$, $^{**} \ p < 0.01$, $^{***} \ p < 0.001$

A.2 IV estimates

Table A.9: Instrumental variable regressions: Dependent variable, neighbors' debt. Winners' consumption, instrumented with lottery prizes as explanatory variables.

	Small-prize sample		Big-prize sample	
Dependent	1st stage	2nd stage	1st stage	2nd stage
variable:	Winners' expenditure	Neighbors' debt	Winners' expenditure	Neighbors' debt
$Lottery_t$	0.447***		0.377***	
	(0.096)		(0.059)	
$Winners'$		0.043**		0.038**
$expenditure$		(0.014)		(0.013)
N	564 350	564 350	208 050	208 050
F	14.24		10.59	

Notes: Clustered standard errors in parentheses. Cluster variable is street ID. Winners' imputed consumption is instrumented with the lottery prize they win. The first stage uses the winners' imputed expenditure as the dependent variable, and $Lottery_t$ as the key explanatory variable. The second stage uses neighbors' debt as the dependent variable, and winners' predicted expenditure responses from the first stage as the key explanatory variable. All first- and second-stage regressions include a lagged dependent variable, the time-varying variables (listed in Section 4) measured for the neighbors of the winner, and household fixed effects and time fixed effects. Estimates are with a sphere of influence equal to ten. 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.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$