

# Peers' Income and Financial Distress: Evidence from Lottery Winners and Neighboring Bankruptcies

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# Peers' Income and Financial Distress: Evidence from Lottery Winners and Neighboring Bankruptcies

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## ABSTRACT

We examine whether relative income differences among peers can generate financial distress. Using lottery winnings as plausibly exogenous variations in the relative income of peers, we find that the dollar magnitude of a lottery win of one neighbor increases subsequent borrowing and bankruptcies among other neighbors. We also examine which factors may mitigate lenders' bankruptcy risk in these neighborhoods. We show that bankruptcy filers obtain more secured but not unsecured debt, and lenders provide additional credit to low-risk but not high-risk debtors. In addition, we find evidence consistent with local lenders taking advantage of soft information to mitigate credit risk.

*Keywords:* financial distress, social comparisons among peers

*JEL Codes:* G02, D14, D31, K35

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## 1. Introduction

A large literature has examined the effect of social comparisons among peers. For example, an individual's consumption choices can be influenced by peers' consumption choices because of mechanisms such as Keeping Up with the Joneses (where individuals attempt to match the consumption of higher income peers) and Conspicuous Consumption (where individuals aim to signal status by consuming more visible goods). Recent research, however, has extended this peer-effects literature to examine the specific impact of social comparisons on the financial status of the peers. In particular, Georgarakos, Haliassos, and Pasini (2014), and Bertrand and Morse (2016) have proposed the hypothesis that if social comparisons between peers, with relative differences in income, generates an unsustainable accumulation of debt, then these social comparisons can lead to increased financial distress.

The aim of this paper is to provide new evidence on this hypothesis of Georgarakos, Haliassos, and Pasini (2014), and Bertrand and Morse (2016) linking social comparisons between peers to debt and financial distress. Relative income comparisons are important because they can increase consumption of peers (Kuhn, Kooreman, Soetevent, and Kapteyn, 2011) and lead to increases in borrowing and decreases in savings (Angelucci and De Giorgi, 2009). In addition, if these social comparisons lead to financial distress and default of the peers, then this behavior could result in losses suffered by the creditors of those peers. It is thus important to understand how creditors can attempt to protect themselves from losses associated with the possible default of these peers. In this paper, we provide evidence on these questions.

While issues of peer effects have long been of interest across many areas of finance and economics, providing empirical evidence on peer effects faces significant empirical identification challenges, such as the reflection problem (Manski, 1993). In brief, when examining how peers influence each other's choices, it is often difficult to identify who affects whom and how. Our strategy to overcome the identification challenges of the reflection problem uses exogenous income shocks from randomly sized lottery wins in the context of very small neighborhoods (in our case, Canadian six-digit postal codes containing a median of 13 households). The strategy relies on the fact that, on the date of the lottery win, the income of the lottery winner will increase by the random and exogenous size of the lottery prize, while the income of her very close neighbors will remain unchanged. Thus, we can causally identify how increasing one peer's income affects other peers' financial distress.

To examine this question, we rely on matched data on the universe of lottery winners and the universe of bankruptcy filers in a single Canadian province.<sup>1</sup> Because we can observe the name and address of the universe of lottery winners and the name and address of the universe of bankruptcy filers in that specific postal code, we can precisely identify which neighbor won the lottery and which other (nonlottery-winning) neighbors filed for bankruptcy.<sup>2</sup> Our main finding is that the larger the dollar magnitude of a lottery prize of one individual in a very small neighborhood, the more subsequent bankruptcies there will be from other individuals in that neighborhood. Our estimated coefficient implies that a lottery win equal to the median annual income in our sample (C\$29,229) increases bankruptcies of neighbors in the three years after the win by 0.03, which is a 6.59% increase relative to the average bankruptcy rate of 0.455 in all postal codes in this event window.

In addition to the main bankruptcy data set, we rely on bankruptcy filer balance sheet data and credit bureau data to examine various possible explanations for our main peer effects finding (i.e., that relative income shocks from lottery wins increase neighborhood bankruptcies). First, by exploiting our data from the asset side of the bankruptcy filer's balance sheet, we provide evidence that is consistent with conspicuous consumption increasing by the bankrupt neighbor of the lottery winner. We follow the existing empirical literature on conspicuous consumption by distinguishing between consumption assets that are more visible to neighbors (e.g., house, car) and consumption assets that are less visible to neighbors (e.g., furniture). We find that the magnitude of the neighbor's lottery win is related to the dollar value of more visible but not less visible consumption assets of neighboring bankruptcy filers. We argue that these differences are consistent with the size of the neighbor's lottery win increasing conspicuous consumption on the part of the bankrupt neighbor.

We also examine whether the neighbor of the lottery winner could take on additional financial risks to finance increased consumption, but this increased risk could result in financial distress instead. We provide evidence consistent with this argument by comparing risky financial assets (e.g., securities) with less risky financial assets (e.g., cash, insurance policies, and pension plans) on the bankruptcy balance sheet of the neighbors. We find that the magnitude of lottery

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<sup>1</sup> We are not able to identify the province because of a nondisclosure agreement with one of our data providers.

<sup>2</sup> We do not use any personal identifiable information (PII) in our analysis.

wins increases the value of risky financial assets (securities) on the balance sheets of nonlottery-winning neighbors at the time of bankruptcy filing. On the other hand, the magnitude of the lottery win has no effect on cash and (weakly) decreases less risky assets (insurance and pensions) on the bankruptcy balance sheet of winners' neighbors. We also use credit bureau data on all individuals in lottery-winning neighborhoods to show that the borrowing of all individuals in these neighborhoods increases in response to large lottery wins compared with small lottery wins. Taken together, these findings are suggestive of the increased borrowing, and risk-taking, of neighbors because of relative income comparisons, leading to financial distress.

Given these results, we then examine why lenders allow such credit expansions and whether there are any factors protecting them from the bankruptcy risk they face from making loans to such neighbors. We examine various possible factors that could mitigate lenders' increased bankruptcy risk in these neighborhoods.

First, we examine the distinction between secured and unsecured debt, given that creditors lose less from secured debt under bankruptcy law because those loans are secured by collateral. We find evidence that the magnitude of the lottery win increases the secured but not unsecured debt on bankrupt neighbors' balance sheets. We cannot observe credit supply and credit demand separately in this setup because the amounts of secured and unsecured debt on bankrupts' balance sheets are equilibrium outcomes, reflecting both credit demand from debtors and credit supply from creditors. Nevertheless, we argue that because creditors recover collateral on secured debt after the bankruptcy of winners' neighbors, our observed relationship between lottery-win size and secured debt is consistent with creditors mitigating some of their losses from these bankruptcies.

A second possible factor that may mitigate lenders' bankruptcy risk from the neighbors of lottery winners is the use of soft information on the neighborhood lottery wins by lenders who are geographically close. We provide evidence consistent with this hypothesis, using data on the exact geographical location of bank branches as well as the exact location of every lottery winner. As predicted by the soft information hypothesis, we show that the subsequent bankruptcies increase in lottery amount if there is no bank branch nearby (within 0.5 km or within 1.0 km of a winning postal code), but there are no additional bankruptcies in neighborhoods with bank branches in close proximity, which may collect very local soft information on lottery winnings.

We also provide evidence for a third possible factor that may mitigate lenders' bankruptcy risk from the neighbors of lottery winners. After a lottery win in the neighborhood, lenders may restrict increases in credit supply to high-risk debtors but could allow increases in credit supply to low-risk debtors. We are able to provide evidence that is consistent with this hypothesis by exploiting our annual credit bureau data on credit risk scores (similar to FICO scores) of every individual in the neighborhood. Specifically, we find a significant positive relationship between the magnitude of the neighbors' lottery win and various credit bureau measures of subsequent borrowing for prime, but not for subprime, borrowers. These measures of credit outstanding are equilibrium outcomes, reflecting both credit supply and demand; nevertheless, these findings are consistent with lenders being more likely to respond to increased demand from the prime neighbors of lottery winners compared with subprime neighbors of lottery winners.

## **2. Contribution to the Literature**

The closest papers to ours, in terms of our main hypothesis, are Georgarakos et al. (2014) and Bertrand and Morse (2016), who examine whether social comparison-based peer effects, arising from relative differences in income, is linked to financial distress. The data and methodologies of both of these papers, however, are very different from ours.

The main source of data of Georgarakos et al. (2014) is a survey asking respondents whether they perceive their income to be lower than that of other members of their social circle. The main finding of Georgarakos et al. (2014) is that such perceptions of low income relative to the social circle are related to both an increase in borrowing by the household as well as measures of possible future financial distress (e.g., debt service ratios and loan-to-value ratios). As noted by Georgarakos et al. (2014), however, their data only allow them to examine measures of possible future financial distress (as measured by the debt service ratio and the loan-to-value ratio), while our data allow us to capture both actual financial distress (as measured by bankruptcy filer data) as well as actual future increases in debt (as measured by the panel nature of our credit bureau data).

To address this same hypothesis, Bertrand and Morse (2016) use data from the Consumer Sentiment Survey to document that “more nonrich households report being financially worse off ... when exposed to higher top income levels in their state” (p. 864). In addition, they

document, using state-level bankruptcy data, a positive correlation between the number of bankruptcies per state-year and top income levels. An important similarity between our paper and Bertrand and Morse (2016) is their use of counts of consumer bankruptcy filings in a geographic area to measure financial distress.

While our main hypothesis is similar to that of Georgarakos et al. (2014) and Bertrand and Morse (2016), our data and identification strategy are quite different. Our use of lottery-winner data within the context of very small neighborhoods allows us to address the challenge of the reflection problem inherent in testing for peer effects. Our approach is to examine neighborhoods with a single exogenous income shock to one neighbor and no income shocks to any other neighbors. The fact that we limit our sample to only those neighbors with a single lottery win implies that we can identify exactly which peer (the lottery winner) influences which other peers (the nonlottery-winning neighbors). The randomization in our identification strategy comes from the fact that, conditional on winning the lottery, the dollar magnitude of the lottery prize will be random. This allows us to compare the impact of large lottery winners with the impact of small lottery winners, in which the magnitude of the lottery win is random.

Our identification strategy (lottery winners within small neighborhoods) is similar to Kuhn et al. (2011), who provide evidence that lottery winners increase conspicuous consumption that is visible to close neighbors and that those close neighbors of the lottery winners, in turn, also increase conspicuous consumption. However, while our identification strategy is similar to Kuhn et al. (2011), we examine a very different set of questions. Kuhn et al. (2011) examine the impact of a lottery win on neighborhood *consumption*, while we examine the impact of lottery win size on neighborhood *bankruptcies* and on neighborhood *debt*. In addition, our paper differs from the existing literature (including Kuhn et al., 2011) in that we provide new evidence on a variety of hypotheses that could explain our main peer-effect results, concerning both issues related to the increase in default risk by debtors (e.g., increased conspicuous consumption, increased financial risk-taking, increased debt) as well as issues related to factors mitigating lenders' default risk in those neighborhoods (e.g., the distinction between secured and unsecured debt, the use of soft information, the distinction between prime and subprime borrowers).

Our paper is also related to Angelucci and De Giorgi (2009), who examine the effect of exogenous government cash transfers on both the consumption and debt choices of close neighbors. They find that close neighbors of the transfer recipients increase consumption, in spite

of not receiving the transfers themselves. They show that this increased consumption by the neighbors is financed by debt and gifts and by a reduction in savings. This evidence that the increased consumption by the peers is, to some extent, financed by increased debt is consistent with the argument in this paper. Several other recent empirical papers have also provided evidence on peer effects in consumption and debt.<sup>3</sup> Coibion, Gorodnichenko, Kudlyak, and Mondragon (2017), for example, argue that banks may be reluctant to extend credit to poorer individuals in higher-inequality neighborhoods, which could curtail consumption increases owing to social comparisons and related financial distress.<sup>4</sup>

As emphasized by both Georgarakos et al. (2014) as well as Bertrand and Morse (2016), the issue of whether peer effects generate an increase in consumption is conceptually very distinct from the issue addressed in this paper, which is whether relative differences in income generate an increase in debt and financial distress. It is indeed possible that peer effects in consumption could increase peers' consumption without triggering increased financial distress. For example, the increase in peers' consumption could be financed by a reduction in savings, without recourse to increased debt. Alternatively, the increase in peers' consumption could be financed by an increase in labor supply, as documented by Neumark and Postlewaite (1998).

Our study is related to Hankins, Hoekstra, and Skiba (2011), who also exploit the exogenous variation in lottery-prize size to examine the effect of exogenous income shocks on bankruptcy. However, these authors only focus on the effect of a lottery win on the bankruptcy

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<sup>3</sup> Thompson (2016) finds an impact of income inequality on the accumulation of debt across the income distribution. Coibion et al. (2014) find that the poor in high-inequality areas have less debt than the poor in low-inequality areas, which they ascribe to banks constraining the supply of credit to the poor in high-inequality areas. Bricker, Ramcharan, and Krimmel (2014) provide evidence that is consistent with the rich attempting to "keep ahead of the Joneses" rather than the poor "keeping up with the Joneses." Evidence in favor of keeping up with the Joneses is provided by De Giorgi, Frederiksen, and Pistaferri (2016), who define social networks as including workplace colleagues. Recent theoretical work has also addressed these issues. Han and Hirshleifer (2016) argue that, because consumption is more salient than nonconsumption, individuals overestimate their peers' actual consumption, which causes them to increase their own consumption.

<sup>4</sup> The main finding of Coibion et al. (2017) is that poorer individuals in high-income inequality areas receive *less* credit from lenders, which they argue is *inconsistent* with relative incomes affecting borrowing of poorer individuals to increase consumption. There are, however, two important differences between our empirical strategy and that of Coibion et al. (2017). First, we employ the dollar magnitude of the lottery win as an exogenous shock to relative incomes within the postal code, while Coibion et al. (2017) capture changes to income inequality (or relative incomes) by measuring "initial" income inequality using Survey of Consumer Finances (SCF) income data some years before the start of their empirical sample. Second, our unit of geography is the Canadian postal code, while Coibion et al. (2017) define neighbors based on U.S. zip codes, counties, and states, which are orders of magnitude larger than the Canadian postal codes we examine.



of the lottery winner herself (Tables A3 and A4 in the Appendix), we replicate the main Hankins et al. (2011) regressions using our data). By contrast, the focus of our paper is on the effect of lottery wins on the bankruptcy filings of very close nonwinning neighbors. Hankins et al. (2011) follow many other studies that have examined the effect of various exogenous shocks on the bankruptcy of the recipient of the shock.<sup>5</sup>

While our study examines the impact of peer effects from relative income shocks on a specific financial outcome (e.g., household bankruptcy), a large literature in finance has examined peer effects in the context of various financial choices. This includes choices relating to stock market participation and financial asset allocation,<sup>6</sup> the market for loans,<sup>7</sup> choices relating to retirement plans,<sup>8</sup> and foreclosure and house prices.<sup>9</sup>

Our main hypothesis is also related to the large macroeconomics literature linking income inequality to financial distress, which is often based on the premise that greater income inequality will generate more social comparison-type behavior, thus leading to financial distress. Much of this macro income-inequality-based research is motivated by the finding of Piketty and Saez (2003 and updates) that income inequality peaked in the periods immediately before the financial crises of 1929 and 2008. Following the 2008 crisis, there has been considerable public and policy debate on whether income inequality causes financial distress.<sup>10</sup> The link between income inequality and financial distress has been more formally examined in various macroeconomics papers.<sup>11</sup>

In addition to the papers cited previously, our use of lottery winner data forms part of a growing literature using lottery winnings as a measure of exogenous income shocks in a variety

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<sup>5</sup> For example, Fay, Hurst, and White (2002); Gross and Souleles (2002); Agarwal and Song (2015); and many others.

<sup>6</sup> See Hong, Kubik, and Stein (2004); Hong, Kubik, and Stein (2005); Brown, Ivkovic, Smith, and Weisbenner (2008); Roussanov (2010); Kaustia and Knupfer (2012); Bursztyn, Ederer, Ferman, and Yuchtman (2014); Hong, Jiang, Wang, and Zhao (2014); Ozsoylev, Walden, Yavuz, and Bildik (2014); Pool, Stoffman, and Yonker (2015); and Heimer (2016).

<sup>7</sup> See Duarte, Siegel, and Young (2012).

<sup>8</sup> See Duflo and Saez (2003).

<sup>9</sup> See Campbell, Giglio, and Pathak (2011).

<sup>10</sup> See Rajan (2010); Acemoglu (2011); Becker (2011); Krugman (2013); Stiglitz (2013); and Cochrane (2014).

<sup>11</sup> See Krueger and Perri (2006); Iacoviello (2008); Bordo and Meissner (2012); and Kumhof, Ranciere, and Winant (2015).

of other contexts.<sup>12</sup> In terms of nomenclature, some papers in this lottery-winner literature refer to lottery wins as income shocks (e.g., Kuhn et al., 2011), while others refer to lottery wins as wealth shocks (e.g., Cesarini, Lindqvist, Notowidigdo, and Ostling, 2017). In this paper, while we use the term *income shocks* for a lottery win, it would also be possible to use the term *wealth shocks*, given that a lottery win can be considered either a wealth shock or an income shock.

### 3. Data

In this section, we describe the various databases we use in our analysis. Summary statistics are provided in Table 1.

#### 3.1. Lottery Data

Our data include all lottery winners with more than C\$1,000 in prizes between April 1, 2004, and March 31, 2014, from a single Canadian province, provided by the provincial lottery organization (which, under the terms of our nondisclosure agreement, we are not able to divulge). The provincial lottery corporation does not keep track of lottery wins of less than C\$1,000, so it was unable to provide us with data on such wins (which is similar to many other lottery studies in the literature). The lottery corporation provided us with data on each winner's name (first and last names), six-digit postal code, dollar magnitude of the lottery win, date of lottery win, and type of lottery game for each win. In terms of the external validity of our evidence, the Canadian Survey of Household Spending (Marshall, 2011) shows that approximately two-thirds of all Canadian adults purchase a provincial government-run lottery ticket at least once a year. These data show that purchases of government-run lottery tickets are by far the most popular form of gambling undertaken by adult Canadians.

Figure 1 provides a histogram of the dollar magnitudes of all ( $n = 7,377$ ) lottery prizes used in our sample. Figure 1 shows that, although there are a large number of smaller lottery wins of less than C\$3,000, there are a significant number of larger lottery wins. Our lottery is very similar to most other lotteries, in that there are very few exceptionally large wins (the

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<sup>12</sup> Examples include Imbens, Rubin, and Sacerdote (2001) on labor supply, earnings, savings, and consumption; Lindahl (2005) on health and mortality; Gardner and Oswald (2007) on psychological well-being; Apouey and Clark (2015) on physical and mental health; Hankins and Hoekstra (2011) on marriage and divorce; Bagues and Esteve-Volart (2016) on election outcomes; Briggs, Cesarini, Lindqvist, and Ostling (2015) on stock market participation; Cesarini, Lindqvist, Ostling, and Wallace (2016) on health and child development; and Cesarini, Lindqvist, Notowidigdo, and Ostling (2017) on household labor supply.

largest win in our sample is more than C\$41 million). For this reason, we use the same cutoff as Hankins et al. (2011) in trimming lottery prizes of more than C\$150,000 (which is equal to the 98th percentile of the prize amount) to remove these very large outliers. We describe this procedure in detail in Section 1 of the Appendix. As described previously, these dollar magnitudes of lottery wins provide the key exogenous variation for our tests.

### **3.2. Bankruptcy Filer Data**

The Canadian bankruptcy regulator, the Office of the Superintendent of Bankruptcy (OSB), has provided our individual-level bankruptcy data. Because Canada has a single bankruptcy regulator (unlike the U.S.), our data include every bankruptcy filing in Canada. A description of the bankruptcy process in Canada is provided in Section 2.2 of the Appendix.

There are two separate bankruptcy databases, which provide the dependent variables for the various empirical specifications in this paper. The first database provides complete data on the total annual counts of bankruptcy filings for each six-digit postal code in Canada for every year between 1994 and 2013. We use these postal code-level bankruptcy count data as our dependent variable to test the hypothesis that the exogenous size of lottery wins affects the count of subsequent bankruptcies among the winner's close neighbors. We label this specification *extensive margin tests* because it examines whether exogenous income shocks to one neighbor leads to additional neighboring bankruptcies.

The OSB has also provided the full balance sheet of individual bankruptcies filed electronically.<sup>13</sup> These balance sheet data are required by law from every bankruptcy filer and are submitted to the OSB using OSB Form 79. These data are all publicly available because a bankruptcy filing is by design a public legal document. We label this bankruptcy balance sheet database *intensive margin* data because it reflects the characteristics of individual filers rather than the counts of filers in a neighborhood.

### **3.3. Neighborhood Data**

Our main geographic unit of analysis and our definition of “neighborhoods” are Canadian

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<sup>13</sup> The transition to the electronic bankruptcy filing system was essentially completed in Canada by 2007. Because our sample includes earlier years, we conducted various robustness tests to examine whether the share of electronic filings to total filings in a postal code is affected by the log of lottery amount. We found no evidence that lottery amount has any effect on the share of electronic filings in a postal code.

six-digit postal codes, which contain a median of 13 households. These areas are extremely small, often smaller than a city block. All of our various databases (individual lottery-winner data, individual bankruptcy filer data, and individual credit bureau data) contain data on the six-digit postal code data for all individuals; thus, we are able to match across all of our various databases using this neighborhood-level measure.

We also use a second, slightly larger measure of neighborhood geography called a *dissemination area (DA)*, which contains approximately 200 households (i.e., the size of a few city blocks), with an average size of approximately 0.2 square km. Using a conversion tool known as the Postal Code Conversion File (PCCF), we can accurately match Statistics Canada DA-level geographies to the six-digit postal code geographies. Figure 2 provides a visual illustration of six-digit postal codes and DAs. Each small block in the map is a separate household. The map shows a single DA, outlined in red, within which falls a number of separate six-digit postal codes, each one displayed in a different color.

While our bankruptcy data allow us to measure the exact number of bankruptcies in every postal code and every DA, no other publicly available data exist to describe the characteristics of individual six-digit postal codes because these areas are so small. Statistics Canada does, however, provide data on observable neighborhood-level characteristics from census data for each DA in Canada. In particular, Statistics Canada provides DA-level data on neighborhood characteristics such as income, income distribution (which we use to compute Gini coefficients), unemployment, age, education, homeownership, and gender, which we use in our analysis.

### **3.4. Credit Bureau Data**

While the bankruptcy filer balance sheet data provide a very detailed view of the filer's financial position at the time of filing, we use another data source to examine the dynamics of debt accumulation and borrowing over time. This data set comes from a Canadian credit bureau and includes annual data on the number and total dollar value of various kinds of credit accounts held by every individual with a credit file (including mortgage accounts, bankcard accounts, auto loans, and installment accounts). The Canadian credit bureau data capture the universe of

neighbors with a credit record in the very small neighborhoods that we examine.<sup>14</sup> These data are measured as of June 30 of each year for 2007–2016. These data capture all accounts, rather than only active accounts, which is an important advantage in our context because we are interested in capturing all sources of credit available to debtors. Summary statistics of our credit bureau data are provided in Table 9.

In terms of interpretation, we argue that in the case of credit bureau data, what we can observe in these data (i.e., the number of accounts and the dollar amount outstanding) is an equilibrium outcome reflecting both credit supply as well as credit demand. However, for the specific case of credit card debt, we can follow the literature (e.g., Gross and Souleles, 2002), which has argued that credit card limits can be interpreted as being driven by credit supply, while credit card balances can be interpreted as being driven by credit demand. Our credit bureau data set includes data on both credit card balances as well as credit card limits, thus we can follow this literature in distinguishing between credit supply and credit demand in the specific context of credit cards.

### **3.5. Relative Advantages of Bankruptcy Filer and Credit Bureau Data**

There are a number of advantages and disadvantages in using credit bureau data, relative to bankruptcy filer balance sheet data, in our context. Because we can observe the names and postal codes of all lottery winners as well as the names and postal codes of all bankruptcy filers in each postal code, we can cleanly distinguish between lottery winners and nonwinner bankrupts within a postal code. On the other hand, an important disadvantage of the credit bureau data in this context is that the Canadian credit bureau provided six-digit postal codes of the universe of individuals (for every year) but not individual names. This constraint implies that we are not able to observe the names of credit bureau individuals and distinguish between winners and nonwinners in a postal code. Our credit bureau specifications therefore include the lottery winner along with all other neighbors, rather than excluding the lottery winner, as in the bankruptcy filer specifications.

The second important concern with our credit bureau data is that for technical reasons, some lenders did not report mortgage data for some years in the early years of the sample. On the

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<sup>14</sup> A small group of postal codes with too few residents is excluded from these data to protect their privacy and confidentiality.

other hand, however, our credit bureau data do indeed reflect the universe of credit information for all other credit products (including credit cards, auto loans and installment loans). Thus, our results for credit cards, auto loans, and installment loans are not subject to this specific concern. We discuss various ways in which we deal with the issue of missing mortgage credit bureau data in Section 1.6 of the Appendix.

Despite these disadvantages to the credit bureau data, we use these data to provide corroborating evidence because it provides us with three important advantages over our bankruptcy filer data. First, the credit bureau data provide information on every neighbor of the lottery winner, rather than only those neighbors who filed for bankruptcy. In other words, by comparing our results across these two different databases, we can provide evidence on whether the magnitude of the neighbors' lottery win has different impacts on two different populations (bankruptcy filers in the neighborhood and all individuals with a credit record in the neighborhood).

A second advantage of the credit bureau data is that, unlike our bankruptcy filer database that only provides data at a single date (the date of the bankruptcy filing), the credit bureau database is an annual panel; thus we can examine the credit records of residents of the six-digit postal code for individual years, both before and after the date of the lottery win in that postal code. This feature allows us to track how credit behavior by individuals in this neighborhood changes in the years before and after the neighbors' lottery win.

A third advantage of the credit bureau data is that, because we can observe annual data for each individual, we can observe whether the same individual moves to a different postal code over time. When we use this credit bureau database, we are able to exclude the information for all individuals who moved to a different location during the course of the sample. This allows us to estimate peer effects only for those individuals who remained in the specific neighborhood of the lottery winner during the entire course of our sample, both before and after the date of the lottery win.

One additional issue could lessen the concern that the credit bureau data cannot distinguish between winners and neighbors: the possibility (unobservable to us) that after their lottery win, the winners may be systematically less likely to increase their demand for credit, relative to the neighbors, specifically because of their increased financial resources from the lottery win. If this assumption is indeed correct, then this implies that the inclusion of winners in

our credit bureau sample when estimating credit outstanding in the neighborhood may actually bias these estimates downward.

#### **4. Identification Strategy and Sample Selection**

Our identification strategy is to examine lottery wins of exogenous and random dollar magnitudes. Our strategy is similar to much of the existing literature exploiting the random nature of lottery prizes (e.g., Imbens et al., 2001; Hankins et al., 2011; Cesarini et al., 2017; and others) in that we restrict our sample to lottery winners (in our case, neighborhoods with a single lottery win) and compare large lottery wins with small lottery wins. This way, we can avoid having to compare lottery winners with nonlottery winners, who may be systematically different because nonlottery winners may be nonlottery players.

##### **4.1. Tests of Identifying Assumptions**

The central identifying assumption in the methodology comparing large and small lottery wins (e.g., Imbens et al., 2001; Hankins et al., 2011; Cesarini et al., 2017; and others) is that the size of the lottery win, conditional on winning, should be random. In other words, no observable and unobservable variables should be correlated with the size of the lottery win. We provide evidence on this assumption for neighborhood observables by using a variety of regressions as well as graphical evidence.

First, we run essentially the same test as Cesarini et al. (2017) by regressing the (log) size of the lottery win against a large number of observable variables. In our case, we are interested in neighborhood-level observables and derive the list of observables from DA-level census data as well as from our own data (the full list of these neighborhood-level observables is provided in Table 1). Table 2 reports results for this test. This OLS regression results in an F-statistic for the joint significance DA-level neighborhood variables of 1.06 with a p-value of 0.382 and an R-squared value of 0.003. In other words, these results confirm that this large list of neighborhood observables has no predictive power on the dollar magnitude of the lottery win in that neighborhood.

One possible concern with this test of the joint predictive power of neighborhood characteristics on lottery amount (as in Table 2) is multicollinearity. For this reason, in Table 3, we undertake a similar exercise, but in this case, we test whether lottery prize amounts are

correlated with each of these neighborhood-level observable characteristics in a series of separate regressions. All these coefficients across all these individual regressions are statistically insignificant, which is consistent with our argument that lottery amounts are as good as randomly assigned.

We also provide graphical evidence that the size of the lottery win is not correlated with each of the neighborhood-level observables. In Figures 3 and 4, and in Figures A3 to A21 in the Appendix, we plot the distributions of these neighborhood-observable characteristics across all DAs in our sample for each of four lottery-prize percent ranges. In other words, these figures plot four separate distributions of the neighborhood-level observable for lottery wins in the 0–25%, 26–50%, 51–75%, 76–100% percent range. If the size of the lottery win is indeed uncorrelated with the neighborhood-level observable, then the four separate distributions of neighborhood characteristics should be indistinguishable from each other. For all these 21 different figures, we find that these distributions are indistinguishable from each other, thus confirming graphically our more rigorous statistical evidence in Tables 2 and 3.

In summary, we argue that these statistical and graphical results provide evidence that is strongly consistent with our main identifying assumption that the magnitude of the lottery prize in a neighborhood is uncorrelated with 21 neighborhood-level observables in our data.

#### **4.2. Relative Magnitude of Lottery Shocks to Income**

An important element of all lottery-based studies is whether the magnitudes of the lottery prizes are significant relative to individual-income levels and sufficient to change peers' behavior. In Figure 1, we provide a histogram of the dollar magnitudes of all lottery wins in our sample, and, in Figure 3, we provide kernel densities of median DA income at the DA level taken from Canadian census data. These figures report data from the 7,377 lottery wins and matched DAs in our main sample. Figure 1 shows a large mass of small lottery prizes and a very long tail of larger lottery prizes up to a maximum of C\$150,000. Figure 3 shows that the median of the median DA income data across the DAs in our study is C\$29,229. In other words, the larger lottery prizes are clearly very significant relative to median DA income, but the smaller prizes are somewhat inconsequential. In addition, we use this measure of median DA income across the DAs in our study, in the following discussion of economic magnitudes in which we



calculate back-of-the-envelope measures of our estimated effects based on lottery wins of this dollar magnitude.

### 4.3. Other Sample Selection Issues

As described previously, our main sample selection procedure is to follow the existing lottery-winner literature by including in our sample only those postal codes in which there is only a single lottery win over the sample period. In addition to this sampling choice, we also make a variety of other sample selection choices regarding the types of lottery to include, dealing with outliers in lottery win size, and dealing with winners who also file for bankruptcy. These sample selection choices are described in detail in Section 1 of the Appendix.

## 5. Main Results

In this section, we provide the main results of this paper, which is to test the hypothesis of Georgarakos et al. (2014) and Bertrand and Morse (2016) that social comparisons between peers, from relative differences in income between the peers, is linked to financial distress. Specifically, in this section, we show that the dollar magnitude of a lottery win in a neighborhood will increase the number of subsequent bankruptcy filings among the nonlottery winners in that neighborhood.

### 5.1. Relative Income Shocks to Peers and Financial Distress

To test the hypothesis that relative income shocks increase the financial distress of nonwinning neighbors, we estimate the following basic model:

$$(1) \quad Y_p = \beta_1 \ln(\text{lottery amount})_p + \beta_2 X_d + \delta_p + \alpha_p + \varepsilon_p,$$

where subscript  $p$  represents the postal code of the winner and subscript  $d$  represents the DA of the winner. This test (along with every other test in this paper) uses the identification strategy (i.e., lottery wins in neighborhoods) outlined previously. The key independent variable is the log of the lottery amount. Because, by design, there is only a single lottery win in each postal code, the subscript  $p$  captures each separate lottery win. Similar to Hankins et al. (2011), this model uses only variation in the lottery amount to identify the effect of income shocks on various outcomes of interest.

The dependent variable  $Y_p$  measures the number of bankruptcies for neighbors of the lottery winner in postal code  $p$  (thus, it excludes cases in which the winner files for bankruptcy) in a particular year or set of years relative to the lottery-winning year. We use various numbers of years before and after the lottery win to examine how the coefficient of interest changes over various time periods after the win. Because this model examines the count of how many bankruptcies occur in a neighborhood, we label this an extensive margin test. Because our dependent variable is a count variable, we use the Poisson model. Given that this is a neighborhood-level regression (our dependent variable is the number of bankruptcies from winner's neighbors within the postal code), we only include neighborhood-level rather than the individual-level controls, denoted as  $X_d$ . Time-invariant neighborhood controls are measured at the DA neighborhood area  $d$  and are taken from census data (a full list of these controls is provided in Table 1). We also include lottery-related fixed effects,  $\delta$ , capturing the year of the lottery win (to account for business-cycle variation), and a fixed effect,  $\alpha$ , for each different type of lottery game (product) provided by the provincial lottery corporation.

In terms of event window length, we provide results for groups of years (Table 4, panel A) and individual years (Table 5). These long event windows, which examine time horizons five years before to five years after the lottery win, reflect the well-known conclusion in the bankruptcy literature that the lag between an exogenous shock and the decision to file for bankruptcy is long and variable (e.g., Hankins et al., 2011). Table 4 uses similar event windows as Hankins et al. (2011): 0 to 2 years and 3 to 5 years. In addition, we also examine an event window from 0 to 5 years to estimate very long-term effects. As is standard in the literature, we also examine event windows before the lottery wins (-1 to -2 years, -3 to -5 years, and -1 to -5 years) to test if there are statistically significant pre-trends in lottery-winning postal codes.<sup>15</sup>

Table 4, panel A, reports results for our full sample, with multiple years in each event window. We find significant and positive coefficients in the years after the lottery win, indicating that the dollar magnitude of a lottery win will increase subsequent bankruptcies of the winner's neighbors in the postal code. No coefficients are statistically significant in the years before the lottery win, which supports our identifying assumptions. The coefficient in years 0 to 2 in panel

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<sup>15</sup> The smaller number of observations for the 3- to 5-year bankruptcy count compared with the 0- to 2-year bankruptcy count is because our bankruptcy data end in 2013 and the lottery data end in 2014. Thus, for some winners in later years (up to 2011), we can observe bankruptcies in 0 to 2 years after winning but not in years 3 to 5.

A is equal to 0.02 and is significant at the 5% level, while the coefficient in the 3- to 5-year event window is significant at the 10% level. The cumulative increase in bankruptcies owing to a peer's lottery win in years 0 to 5 is equal to 0.068, and it is significant at the 1% level. In terms of economic significance, our estimated coefficient for the 0- to 2-year event window implies that a lottery win equal to a median annual income (C\$29,229 in our sample) will increase bankruptcies by 0.03. As shown in the summary statistics (Table 1), this increase in bankruptcies is equal to 6.6% of the average number of 0.455 bankruptcies per postal code over the 0- to 2-year-event window.<sup>16</sup> A shock to the postal code's relative income in the form of a C\$100,000 lottery win would increase bankruptcies by 0.052 or 11% in years 0 to 2 after the lottery win.<sup>17</sup>

Table 5 presents results for the effect of lotteries on a winner's neighbors' bankruptcies for each individual year from  $t = -5$  to  $t = 5$  (in which the winning date is year  $t = 0$ ). Columns (1) and (2) of this table show estimated coefficients and their standard errors. The only year with a statistically significant coefficient is year 3, with the coefficient of 0.0129, and it is significant at the 5% level. This coefficient implies that a lottery win equal to the annual median income (C\$29,229) would increase winner's neighbors' bankruptcies by 0.02, which is a 13.5% increase relative to the average value of 0.148 bankruptcies per postal code in year 3.

## 5.2. Placebo Tests on More Distant Neighbors

Our tests in the previous section (Table 4, panel A, and Table 5) find differences in relative incomes among peers owing to lottery wins increasing bankruptcies of nonlottery-winning neighbors. A key assumption behind our neighborhood-based identification strategy is that peer effects operate only within very small neighborhoods (i.e., peers are very close neighbors). We can provide further evidence on this identifying assumption by running a placebo-type test of the effect of lottery winners on neighbors that are slightly farther away than the immediate neighbors. Our prediction from these placebo tests is that there should be reduced or insignificant peer effects among slightly more distant neighbors.

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<sup>16</sup> A lottery win equal to the median annual income would increase bankruptcies of a winner's neighbors by 0.043 or 10.5% in years 3 to 5 and by 0.114 or 14% in years 0 to 5.

<sup>17</sup> These economic effects are estimated using margins command in Stata and comparing the number of bankruptcies at the mean values of all variables with the number of bankruptcies when the lottery amount increases by C\$29,229 or C\$100,000.

Our placebo test results are reported in Table 4, panel B, and Table A5 in the Appendix, in which we examine bankruptcy counts per postal code occurring within the winner's DA area (outlined in red in Figure 2) but excluding the winner's own postal code. We denote these slightly more distant neighbors as outer ring neighbors. We measure bankruptcy counts per postal code in the outer ring as the annual number of bankruptcies divided by the number of postal codes in the outer ring. Table 4, panel B, provides the results of these tests for outer ring neighbors for event windows of multiple years, while Table A5 in the Appendix provides the results for individual years. As predicted, we find no statistically significant results for the slightly more distant neighbors in the outer rings. These placebo test results are thus consistent with our argument that very close neighbors are a more relevant peer group compared with neighbors who are slightly farther away and that the effect of peer comparisons is very local and quickly dissipates with distance.

## **6. How Lottery Wins Can Lead to Neighbors' Bankruptcy**

In this and the following sections of the paper, we provide evidence on various possible hypotheses that could explain our main finding in Section 5 that relative income shocks from lottery wins increase financial distress of nonwinning peers. In Section 6, we examine how choices made by the neighbors of the lottery winners result in increased financial distress. These are (1) increased conspicuous consumption, (2) increased financial risk taking, and (3) increased debt outstanding. We argue that these three choices are not necessarily mutually exclusive because they can all contribute to increased financial distress. In Section 7, we examine which factors may mitigate lenders' losses from neighbors' bankruptcies.

### **6.1. Conspicuous Consumption by the Bankrupt Neighbors**

The hypothesis of conspicuous consumption (originating with authors such as Veblen, 1899, and Duesenberry, 1949) states that individuals will attempt to signal increased status by consuming high-status goods that are more visible to their social reference groups. Bertrand and Morse (2016) provide evidence of relative income differences generating such social signaling by showing that "the budget shares that nonrich households allocate to more visible goods and services increase with top income levels" (p. 864). Their use of the relative visibility of goods

and services to provide evidence of social signaling and conspicuous consumption follows a large literature (e.g., Charles, Hurst, and Roussanov, 2009; Heffetz, 2011).

In this section, we adopt a similar approach to examine conspicuous consumption by the bankrupt neighbors of the lottery winner by exploiting data on the dollar magnitudes of more visible consumption assets (e.g., house, car), relative to less visible consumption assets (e.g., furniture), reported on the balance sheets of individual bankruptcy filers. Our test of conspicuous consumption examines whether the size of the neighbor's lottery win is more likely to impact more visible, relative to less visible, assets on the bankruptcy balance sheet of the neighbor.

It is important to note, however, that because our bankruptcy balance sheet data only report assets owned by the bankruptcy filer as of the date of the bankruptcy filing, we are not able to observe the date at which any asset was purchased. This feature of our data implies that we are not able, for example, to examine whether the purchase of any asset occurred before or after the date of the neighbor's lottery win. However, the consumption assets that appear on the bankruptcy balance sheet (e.g., house, car, motorcycle, recreational equipment, furniture),<sup>18</sup> can all be considered durable to some extent. Thus, all these assets provide a stream of consumption services over time, and all can be disposed of at any time. Therefore, the fact that a bankruptcy filer owns a particular consumer durable asset at the date of the bankruptcy filing and did not dispose of it before the filing date is indicative of choices made by that individual regarding the stream of consumption services from that asset in the period between the date of the lottery win and the date of the bankruptcy filing.

Our measure of consumer durable assets, taken from a balance sheet at a single point in time, is somewhat similar to that of Kuhn et al. (2011) in their study of lottery wins on the consumption of close neighbors. Kuhn et al. (2011) find that the neighbors of lottery winners own more cars at a specific date after the lottery win. They measure car assets via survey data of neighbors using a measure that “combines information on both the number and quality of cars” (p. 2238) at the specific date of the survey. Our approach is similar to that of Kuhn et al. (2011), in that we also measure the assets of neighbors at a specific date after the lottery win (i.e., the

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<sup>18</sup> Our OSB Bankruptcy balance sheet data provide various other categories of consumer durable assets (including cottage, land, snowmobile, other vehicle), but we do not include these categories in our analysis because these data are sparsely populated.

date of the neighbor’s bankruptcy filing), although in our case, we can observe the actual dollar value of the car (or house, motorcycle, furniture, etc.) as reported to the bankruptcy regulator.

Kuhn et al. (2011) also provide evidence that lottery wins can affect exterior house renovations of lottery winners, thus increasing the housing values of winners. This visible consumption mechanism could explain how the neighbors of lottery winners can attempt to keep up with the winners using exterior home renovations and increase their house value. While we are not able to observe specific renovation expenditures of winners’ neighbors to test this mechanism directly, this argument regarding visible house renovations is consistent with our findings regarding house values reported on the bankruptcy balance sheet of the neighbors.

Our specification to test this hypothesis is as follows:

$$(2) \quad Y_i = \beta_0 + \beta_1 \ln(\text{lottery amount})_i + \beta_2 X_i + \delta_p + \alpha_p + \varepsilon_i.$$

The dependent variable  $Y_i$  is the value of a particular asset appearing on the balance sheet of a bankruptcy-filing neighbor of a lottery winner. These asset values are measured at the level of individual bankruptcy filer  $i$ . Summary statistics for all of the various bankruptcy balance sheet assets are reported in Table 6. Because the distributions of these bankruptcy balance sheet assets have very long right tails, we take the log of these asset values and use them as dependent variables. We also add 1 to all values to make the log of 0 value equal 0, not missing.

Because this specification is at the level of the individual (nonlottery-winning, bankruptcy-filing neighbor), we can also include individual-level controls, denoted  $X_i$ , of the bankruptcy-filing neighbor in addition to the neighborhood controls used in equation (1). Table 6 provides details of these controls. In addition, because these tests are at the level of the individual bankruptcy filer, we can also include dummies for each of the 17 different categorical “reasons for financial distress” given by filers when they file, as reported by the OSB. Table 6 also describes these reasons. Similar to equation (1), we include lottery-specific fixed effects (i.e., lottery product and lottery-winning year fixed effects) in this model to account for differences among lottery products in the sample.

We report results for these tests for multiple years in Table 7 (Tables A6 and A7 in the Appendix report results for individual years). In this table, we only report a single coefficient (on the log of lottery win size) from each regression. Our main result in Table 7 is that we find statistically significant coefficients for cars and houses (and motorcycles at the 10% level of

significance) in the 0- to 2-year window but find no significant results for recreational equipment and furniture. In all cases, we find insignificant results in periods before lottery wins, which is consistent with the parallel trends assumption. Our results indicate that, while a large (relative to a small) lottery win by a neighbor will increase the value of more visible consumption assets (house and car) reported on bankruptcy balance sheets, such a relationship is not evident for less visible consumption assets on the bankruptcy balance sheets of the neighbors (recreational equipment and furniture).<sup>19</sup> We argue that these results are consistent with the bankrupt neighbors of lottery winners engaging in conspicuous consumption to signal their status.

In terms of economic magnitudes, the results in Table 7 imply that, in years 0 to 2 after the date of the lottery win, a 1% increase in the size of the lottery win leads to a 0.27% increase in the house value of neighboring bankruptcy filers and a 0.21% increase in the car value of neighboring bankruptcy filers on the date of the bankruptcy filing.

## **6.2. Financial Risk-Taking by the Bankrupt Neighbor**

In Section 6.1, we compared the choices made by bankrupt neighbors between more visible (e.g., house, car) and less visible (e.g., furniture) consumption assets. In this section, we examine another (not mutually exclusive) choice — made by the bankrupt neighbors — between different kinds of *financial* assets, as observable on their bankruptcy balance sheets.

The argument is that to finance the increased conspicuous consumption (which we document in Section 6.1), the neighbor of a lottery winner may choose to increase her level of financial risk. This increased financial risk-taking could generate financial distress for the neighbor.

We provide evidence for this hypothesis by exploiting our bankruptcy balance sheet data on more or less risky financial assets held by the bankrupt neighbors of lottery winners. On the one hand, we are able to observe data on the total value of securities on the balance sheets of bankruptcy filers, which we define as *high-risk* financial assets. On the other hand, we are able to observe two different financial assets, which we define as *low-risk* assets: (1) the cash surrender value of insurance and pension accounts, and (2) cash on hand.

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<sup>19</sup> While various assets could be classified as *recreational equipment*, we note that there are separate OSB balance sheet categories for more visible assets such as snowmobiles and other vehicles (see previous footnote).

If the neighbors of lottery winners file for bankruptcy because they acquired riskier financial assets, we would expect that the value of the risky assets on their bankruptcy balance sheets (i.e., securities) would increase in the lottery amount, while the value of less risky assets (i.e., insurance, pension, cash) would be unrelated to the dollar magnitude of the lottery win.

To provide evidence on this hypothesis, we use essentially the identical model used in equation (2), except with different dependent variables (assets). We report these results in Table 8. This table shows that the lottery amount increases the log value of securities appearing on the balance sheets of bankruptcy filers, while it has no effect on cash and a (marginally significant) negative effect on the holding of insurance and pensions. These findings are consistent with the hypothesis that the peers of lottery winners take more financial risks (specifically holding more securities) if they are exposed to large wins compared with the small wins of the neighbors.

It is important to note that our findings regarding conspicuous consumption (Section 6.1) and increased financial risk-taking (Section 6.2) are not necessarily mutually exclusive because they both relate to different choices that can be made by the bankrupt neighbor as reflected on the bankruptcy balance sheet. Indeed, the neighbors of lottery winners may be taking more financial risks to finance their increased conspicuous consumption.

### **6.3. Increased Borrowing by the Neighbors of Lottery Winners**

Both Bertrand and Morse (2016) and Georgarakos et al. (2014) highlight the role of unsustainable debt accumulation by relatively less well-off individuals as an explanation for why relative income differentials can lead to financial distress. In this section, we provide evidence on this issue. Specifically, we use our credit bureau data described in Sections 3.4 and 3.5 and in Section 1.6 in the Appendix to examine how the magnitude of a neighbor's lottery win affects a variety of credit bureau outcomes (number of accounts, total balances outstanding) on four types of credit (mortgages, bankcards, auto loans, installment loans). Table 9 summarizes the Credit Bureau Data used in this section.

In most cases, we argue that our credit bureau data on the amount of credit outstanding is an equilibrium outcome that reflects both credit supply choices by the creditor as well as credit demand choices by the debtor. Thus, a finding, for example, of increased credit outstanding by the neighbors of a lottery winner implies that both the debtor as well as the creditor made choices that allowed this increase in credit outstanding to occur. The one exception to our measure of



credit outstanding being an equilibrium outcome is our credit bureau data on bankcards, in which we can separately observe both credit supply (card limits) as well as credit demand (card balances).

In these credit bureau specifications, we use the following model to estimate how lottery-win size affects individual credit outcomes in lottery-winning postal codes:

$$(3) \quad Y_{jt} = \beta_0 + \sum_{s=-5}^5 \beta_{1s} T_{st} \times \ln(\text{lottery amount})_p + \beta_2 \ln(\text{lottery amount})_p + \sum_{s=-5}^5 \beta_{3s} T_{st} + \delta_p + \alpha_p + \mu_j + \varepsilon_{jt}.$$

The dependent variable  $Y$  is one of the outcome variables taken from our credit bureau data, described next. Subscript  $j$  denotes individuals, and  $t$  denotes time relative to lottery-winning time ( $t = 0$ ). The major independent variables of interest are interactions of event-time dummies  $T_s$  (defined relative to the lottery-winning year,  $t = 0$ ) with the log of the lottery amount. The coefficients on these interactions show the effect of the lottery size on the credit variables before and after the lottery-winning date. As in our previous specifications, we also include lottery product and winning-year fixed effects. This specification includes individual fixed effects to control for individual-level unobservables. We also cluster our standard errors at the individual level.

We use a large number of outcome variables ( $Y$ ), each one of which is taken from our individual-level credit bureau data. As described in Section 1.6 in the Appendix, our credit bureau data contain the universe of credit files for every individual and every year in 2007–2016 for all but one credit product (including bankcards, auto loans, and installment loans). The one exception to this is mortgage data, where some banks did not report mortgage amounts to the credit bureau in some years, especially in the early years of our sample. Our credit bureau data also provide information on the total amount of all credit outstanding, aggregated across all types of liabilities. Because of the issues regarding missing mortgage data in the early years of the sample, we report two separate aggregate measures of total credit outstanding: (1) total credit outstanding across all accounts including mortgages and (2) total credit outstanding across all accounts with the exception of mortgages.

For all the various types of liability (with the exception of bankcards), we report two separate measures of the dependent variable  $Y$  — the total number of accounts by that individual as well as the total dollar value of these accounts. For the specific case of bankcards, our credit

bureau data allow us to capture three variables: total accounts opened, total bankcard credit limits, and total bankcard balances. When our dependent variable  $Y$  is a dollar amount reflecting the total balances of a particular credit product, we add 1 to its value and log that variable. In these cases, therefore, the specification becomes a log-log specification so the coefficients can be interpreted as elasticities.

Because we can observe annual credit bureau data for every individual in the neighborhood, this specification is an annual panel in which we examine all years. The base year is one year prior to a lottery win so all the effects should be interpreted as relative to this year. In most cases, we report results for four years before to five years after the event date. (As described in Section 1.6 in the Appendix, to address the issue of missing mortgage data in the early years of our sample, we drop the first two years of mortgage data. As a result, we are only able to report results from years from -4 to -2 before the lottery win year for mortgage data and for aggregate debt which includes mortgages.) Because the credit bureau data include every individual with a credit record in a postal code for every year, the sample sizes of these panel regressions are orders of magnitude larger than our bankruptcy balance sheet sample sizes (in which our data only include bankruptcy filers in the specific year of the filing).

Tables 10 and 11 summarize our results for these credit bureau variables. These tables only report coefficients  $\beta_{1s}$  on the interactions of the log of the lottery amount with event-time dummies. Each column in Tables 10 and 11 reports results for a different credit bureau liability measure.

Our main conclusion from Tables 10 and 11, across all of the various columns, is that the magnitude of the neighbors' lottery win has a positive and significant effect on the number of accounts and amount of credit outstanding. Relative to our base year -1, we find no significant coefficients in years prior to the lottery win but many statistically significant coefficients in years subsequent to the neighbors' lottery win. In all cases, the significant coefficients are positive, indicating an increase in credit. In other words, the results in Tables 10 and 11 suggest that across all of these various definitions of credit outstanding, the larger the size of the lottery win in the neighborhood, the greater the subsequent increase in credit in that neighborhood across all of these various types of credit.

Columns (1) and (2) of Table 10 provide results for the total of all liabilities, and columns (5) and (6) provide results for aggregate totals across all liabilities excluding mortgages. For both

of these measures, we find a very significant increase in the number of credit lines in the period after the lottery win in the neighborhood. Columns (3) and (4) provide results for mortgages. These results show significant increases in both the number of mortgage accounts opened in the neighborhood, as well as the total dollar amount outstanding on these mortgages, in the period after the lottery win in the neighborhood.<sup>20</sup> In terms of economic magnitudes, the coefficients on all accounts imply that a 10% increase in lottery amount would increase all accounts by 0.0037 and balances by 0.2% in year 4. The economic magnitudes are similar in other years.

In Table 11, Columns (1), (2) and (3) report results for bankcard data. Column (1) shows that there is an increase in the number of bankcards for every year from year 0 to year 5, column (2) shows there is an increase in outstanding balances on bankcards (reflecting an increase in credit demand) in year 1, while column (3) shows that there is an increase in bankcard limits (reflecting an increase in credit supply) in years 0, 1, and 2. These coefficients imply that a 10% increase in lottery amount would increase bankcards by 0.0007, card balance by 0.12%, and limits by 0.13 %. In summary, these results show that the larger the lottery win in the neighborhood, the larger the subsequent increase in the demand for bankcard credit, as well as the subsequent increase in supply of bankcard credit, in that neighborhood.

Columns (4) and (5) report results for auto loans, while columns (6) and (7) report results for installment loans. For both of these credit products, we find that the greater the size of the lottery win in the neighborhood, the greater the subsequent increase in the number of credit lines outstanding in that neighborhood.

Overall, the results in Tables 10 and 11 show that the size of the lottery win in the neighborhood leads to a significant subsequent increase in the amount of credit in that neighborhood across a large number of different credit products. These results thus provide direct evidence of the argument of Bertrand and Morse (2016) and Georgarakos et al. (2014), concerning the role of debt when individuals react to the incomes of their peers.

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<sup>20</sup> Canadian debtors are able to increase their demand for secured mortgage credit in two ways. First, large fractions of Canadian mortgages are bundled with home equity lines of credit (HELOCs). These hybrid mortgages automatically allow debtors to borrow using home equity once they have started to pay down their initial mortgage balance. Second, in Canada, debtors refinance mortgages multiple times before they pay off the mortgage in full. This “forced” mortgage refinancing at the conclusion of each mortgage contract allows debtors and creditors to renegotiate all the terms of the contract (including the amount of mortgage credit demanded by the debtor and supplied by the creditor). We provide details of these mortgage market features in Section 2.1 of the Appendix.

In summary, Section 6 provides evidence on various choices made by the bankrupt neighbors of lottery winners (conspicuous consumption in Section 6.1 and financial risk-taking in Section 6.2) or on equilibrium outcomes reflecting choices by both debtors and creditors (increased debt outstanding in Section 6.3). We argue that findings in these three subsections are consistent with an increase in bankruptcy risk of the neighbors of lottery winners.

## **7. Factors Mitigating Lenders' Risk from Bankruptcy of Neighbors of Lottery Winners**

Our findings in Section 5 — that the neighbors of lottery winners have a higher bankruptcy risk — raises the question of whether any factors may mitigate creditors' increased default risk in these neighborhoods. While Section 6 examined hypotheses to explain why there is an increased bankruptcy risk of neighbors of lottery winners, this section examines factors that may mitigate this bankruptcy risk from the neighbors of lottery winners.

First (in Section 7.1), we examine whether lenders receive more collateral in bankruptcy that can reduce their losses from bankruptcies in lottery-winning neighborhoods. Second (in Section 7.2), we explore whether the soft information available to geographically close lenders may mitigate bankruptcy risks following a lottery win in that neighborhood. Third (in Section 7.3), we test whether the credit score information may be used by lenders to mitigate their bankruptcy risk.

### **7.1. Secured and Unsecured Debt Under Bankruptcy Law**

In many jurisdictions, including Canada, the distinction between secured and unsecured debt plays a central role in the bankruptcy process. Bankruptcy law states that if the liability of the debtor is secured, then the creditors will have a claim on the collateral (e.g., a lender will have a claim on the house if the bankruptcy filer has secured mortgage debt). On the other hand, under bankruptcy law, creditors holding claims on unsecured debt (e.g., credit card debt) are entitled to receive proceeds from a bankruptcy estate only after all secured and preferred creditor claims are satisfied. In practice, this provision means that unsecured creditors usually recover very little on their unsecured claims. In Section 2.2 of the Appendix, we provide further details about bankruptcy institutions in Canada.

Because of the importance of the distinction between secured and unsecured debt under bankruptcy law, in this section, we examine whether there is a statistical relationship between the

lottery-win amount and the amounts of secured and unsecured debt on the balance sheet of neighboring bankruptcy filers. A finding of a positive and significant relationship between lottery-win size and secured debt — but not unsecured debt — of bankrupts can be considered beneficial to creditors because, under bankruptcy law, creditors have a claim on the collateral of the secured debt but not unsecured debt outstanding.

An important caveat is that, as we described in the previous sections, the amounts of secured and unsecured debt that we can observe on the bankruptcy balance sheets are equilibrium outcomes, reflecting both the demand for credit by debtors and the supply of credit by creditors. Thus, we cannot identify whether any observed relationship between lottery-win size and the amounts of debt outstanding is driven by credit supply, credit demand, or both. Nevertheless, we can argue that creditors may recover collateral on secured debt after the bankruptcy of winners' neighbors, which mitigates their losses from these bankruptcies.

Summary statistics for our data from the liabilities side of the balance sheet of individual bankruptcy filers are provided in Table 6, reporting a variety of different measures. For each filer, we can measure total debt, unsecured debt, and secured debt. We also provide debt-to-total asset ratios (which we can measure from the asset side of the bankruptcy balance sheet) for the various categories as a measure of leverage of the bankruptcy filer.

To test the effects of lottery prizes on winners' neighbors, we use the same specification as in equation (2). However, in this model, the dependent variables are various measures taken from the liabilities side of bankruptcy filers' balance sheets, as described in Table 6. In other words, all these tests examine the effect of the size of the neighbor's lottery win on the various bankruptcy balance sheet liability measures.

Results for these tests are provided in Table 12 for the same event windows used in Table 7. Our key finding from Table 12 is that the lottery amount increases the secured liabilities of the bankrupt neighbors of lottery winners, but it has no significant effect on unsecured liabilities of nonwinning neighbors. Specifically, we find that in the 0- to 2-year-event window, a 10% increase in lottery amount leads to a 3% increase in secured debt, 0.003 units increase in the secured debt-to-total assets ratio (leverage) and a 0.002 unit increase in the secured debt-to-total debt ratio. For all these variables, we find no significant coefficients for any event window before the lottery-winning date, indicating that our results are consistent with the parallel trends

assumption. For all other specifications, measuring either unsecured liabilities or total liabilities, we find no significant coefficient either before or after a lottery win.

We thus argue that these results, showing a secured debt channel but not an unsecured debt channel, are consistent with lenders recovering more collateral from the bankruptcy of lottery winners' neighbors. While this outcome can be considered beneficial to lenders, the amount of debt that we can observe (i.e., liabilities on a bankruptcy filer's balance sheet) is an equilibrium outcome, reflecting both the demand for credit by the bankrupt individual as well as the supply of credit by the lenders. Thus, our finding of the lottery amount increasing secured liabilities is consistent with both lenders willing to extend this type of credit and borrowers willing to accept it. On the other hand, we do not find such a relationship for unsecured credit.

## **7.2. Soft Information and Distance from Lenders**

In the previous section, we provided evidence that lottery amounts increase secured debt, but not unsecured debt, on bankrupts' balance sheets. In this section, we examine whether soft information may help lenders mitigate bankruptcy risk, in which soft information is assumed to vary with the geographic distance between the lottery winner (and her neighbors) and the nearest lender.

It is well documented in the banking literature (e.g., Petersen and Rajan, 2002; and Agarwal and Hauswald, 2010) that the geographic distance between the lender and the debtor decreases the amount of soft information acquired by the lender concerning the debtor, in which soft information includes information that is not systematically collected by creditors about debtors. To the best of our knowledge, lenders or credit bureaus do not systematically collect data on lottery wins; thus, our neighborhood lottery win events clearly constitute very localized soft information. In this section, we provide evidence on the hypothesis that geographically close lenders are likely to have more soft information on neighborhood lottery wins and are thus more likely to reduce their exposure to losses from neighborhood bankruptcies.

### **7.2.1. Data on Location of Lenders and Lottery Winners**

We use Geographic Information System (GIS) data on the location of every branch of all major banks in Canada to provide evidence on the soft information hypothesis that lenders close to the lottery winner will be able to reduce bankruptcy losses. Specifically, we use the Enhanced

Points of Interest geographic information database, which provides the exact coordinates (longitude and latitude) of the physical location (e.g., bank branch) of every large financial institution in the province.<sup>21</sup> These data allow us to measure the geographic distance from the postal code of every lottery winner in our study to the geographically closest financial institution. We use this measure of distance to capture the extent to which lenders have soft information about that specific lottery win.

We only include physical branches rather than automated teller machines (ATMs) in our data because it is unlikely that ATMs will capture soft information. One caveat to these soft information tests is that our bankruptcy filer data do not allow us to observe which specific lenders are the creditor(s) of the bankruptcy filers. For this reason, our definition in this section is whether any lender is within a certain distance of the filer. However, given that the Canadian consumer banking system is highly concentrated and is dominated by five very large national banks, this definition of close lenders implies that at least one major bank within the Canadian system is very close to the specific bankruptcy filer.

### **7.2.2. Soft Information Evidence Using Bankruptcy Filer Data**

An important issue with the tests of soft information is that, because of the fundamentally amorphous nature of “soft information,” there is no specific distance at which the soft information hypothesis is theoretically expected to operate. Rather, we argue that the actual distance from the bank at which soft information operates needs to be determined by empirical evidence. To provide such empirical evidence, we use two distances to define lenders proximity. The first measure is 0.5 km, which is approximate walking distance from a lender. The second measure is 1 km, which is the median distance to a lender in our sample.

To test how lenders’ proximity and their ability to collect soft information affects bankruptcies of lottery winners’ neighbors, we run the same exact tests described in equation (1) on two subsamples of the main data. The first subsample includes postal codes without lenders within a specific distance (0.5 km or 1.0 km), which we label as “far” neighborhoods, while the

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<sup>21</sup> We include branch locations from the following banks: Canadian Imperial Bank of Commerce, Toronto–Dominion Bank, Bank of Montreal, Royal Bank of Canada, Scotia Bank, HSBC Group, and the largest provincial lender. Since the first five lenders, broadly speaking, account for approximately 85% of the consumer banking market in Canada, they are often called the “big five” banks.

second comprises postal codes with proximate lenders within that distance (0.5 km or 1.0 km), which we label “near” neighborhoods.

Table 13 (for both 0.5 km and 1.0 km) reports how lottery-winning amounts affect bankruptcies of nonwinning neighbors in these two groups. Our main results from Table 13 are that (whether we use 0.5 km or 1.0 km to define proximity) we find lottery win size increases bankruptcies in neighborhoods that are far from the closest lender. However, we find no significant effect of lottery amount on the bankruptcies of neighbors in the neighborhoods that are near to the closest lender.

In other words, these results imply that our main finding that lottery wins increase neighbors’ bankruptcies is being driven by neighborhoods without nearby lenders. These results are consistent whether the relevant distance is either 0.5 km or 1.0 km. We argue that these findings are consistent with the hypothesis that proximate lenders are more likely to access the very local soft information concerning neighborhood lottery wins. Thus, these proximate lenders act to avoid bankruptcies in that neighborhood after a large lottery win.

### **7.2.3. Soft Information Evidence from Credit Bureau Data**

In this section, we provide additional corroborating evidence on the soft information hypothesis using our credit bureau data. In particular, we examine how credit increases after lottery wins vary with the neighborhood’s proximity to bank branches. As in Section 7.2.2, we split the sample of individual credit bureau data into two subsamples based on the availability of a lender within a specific distance (0.5 km or 1.0 km) of lottery-winning postal codes (which we label near and far samples). In Tables 14 and 15, we report results for 0.5 km, while in Tables A8 and A9 in the Appendix, we replicate these results for the relevant distance being 1.0 km. These credit bureau results are more mixed than our bankruptcy filer results reported in the previous section. While we find that there are more significant coefficients in the neighborhoods without proximate lenders, relative to the neighborhoods with lenders nearby (which is consistent with the soft information hypothesis), we still find some significant coefficients in the neighborhoods near lenders.

There is one possible interpretation of our strong findings that is consistent with the soft information hypothesis in the bankruptcy filer data but somewhat more mixed findings in the credit bureau data: Lenders may be more concerned with using soft information in that fraction



of their debtors who are more likely to file for bankruptcy (as measured by our bankruptcy filer data) and less concerned when dealing with the complete universe of debtors (as measured by our credit bureau data).

### **7.3. Credit Scores and Risk Mitigation**

In this section, we provide evidence on a third possible factor that can reduce lenders' losses from bankruptcies of lottery winners' neighbors. Lenders can segment debtors in neighborhoods of large lottery wins based on credit risk ratings observable to them (e.g., risk scores) and restrict credit increases to high-risk debtors in those neighborhoods.

We provide evidence for this hypothesis by exploiting our credit bureau data and, in particular, the fact that these data allow us to observe the credit risk score for each individual for each year. We are able to segment the total population of individuals appearing in the credit bureau data into either prime or subprime debtors. Note that because the key requirement of providing evidence on this hypothesis is the availability of credit score data, we are not able to use bankruptcy filer data to examine this hypothesis because the bankruptcy filer data do not contain data on the credit scores of individuals. Furthermore, as in various previous sections, we emphasize that credit bureau measures of debt outstanding should be interpreted as equilibrium outcomes reflecting both the supply and demand of credit. However, in the current context of using credit scores to distinguish between prime and subprime debtors, we argue that this distinction may reflect a choice regarding who should receive credit, made by creditors rather than by debtors.

Specifically, we define individuals as being subprime borrowers if their risk score is less than or equal to 660 for any of the five years before the date of the lottery win in their neighborhood. We classify all other debtors (i.e., whose risk score never falls below 660 in the five years before the lottery win in their neighborhood) as prime debtors.<sup>22</sup> By segmenting our total sample into prime and subprime debtors, we can examine the effect of the magnitude of the neighbors' lottery win on credit use in the years after the lottery win for these prime and subprime subsamples.

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<sup>22</sup> In Tables A10 and A11 in the Appendix, we report results in which we define subprime individuals as those with a risk score of lower than 660 in the one year (rather than any of the previous five years reported in Tables 16 and 17) prior to lottery win. Our results are very similar across these various definitions of subprime debtors.

Tables 16 and 17 report results for prime and subprime debtors, respectively. The specifications used in these tables are identical to those used in the previous credit bureau tests (i.e., equation (3) and Tables 10 and 11), with the exception that our sample is now split into prime and subprime groups. Our main conclusion from comparing these two tables as well as Tables 10 and 11 (the results for the full sample) is that credit increases are evident in the prime group but not in the subprime group. Our prime results in Table 16 are similar to those of the full sample in Tables 10 and 11, with the exception that the point estimates of the coefficients are sometimes larger for the prime sample compared with the full sample. This evidence is consistent with lenders using credit scores in allocating credit to prime borrowers who are considered to be less risky and not providing additional credit to subprime borrowers who are more risky. Thus, we argue that lenders can use risk assessment measures such as risk scores to mitigate bankruptcy risk from relative income shocks to neighbors, which we document in Section 5.<sup>23</sup>

## **8. Ruling Out Alternative Mechanisms**

In the preceding sections, we exploited various elements of the individual's bankruptcy filing to provide evidence on various possible mechanisms that could explain our main result that the magnitude of the lottery win increased subsequent bankruptcy filings of winner's neighbors. For example, in Sections 6.1 and 6.2, we used data on bankruptcy assets, while in Section 7.1, we used data on bankruptcy liabilities to provide evidence of various possible mechanisms. In this section, we use other data from the winner's neighbors' bankruptcy filing to examine (and rule out) various other possible mechanisms.

Specifically, we exploit a unique element of our bankruptcy filer data, which is that every bankruptcy filer in Canada is required by the Canadian bankruptcy regulator (OSB) to answer the textual question "Give Reasons for Your Financial Distress" in their bankruptcy filing. The OSB has used textual analysis software to code these textual answers into 17 distinct categories, which we transformed into dummy variables for each reason (summary statistics reported in Table 6).

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<sup>23</sup> One possible concern with this specification is that we may be picking up age effects (young versus old) rather than risk effects (prime versus subprime), given that young individuals tend to have worse risk scores because they do not have a long credit history. We address this concern by examining four different samples, split by both age (young and old) and risk (subprime and prime). We report these results in four Appendix Tables A12 to A15. Overall, these results show that the prime versus subprime results hold true in both the young and old subsamples.

Examples of such reasons include marital breakdown, unemployment, and health concerns. An individual bankruptcy filer can provide many reasons for their financial distress or no reason at all. One possible caveat with these data is that they are self-reported by the bankruptcy filer.

Each of these 17 different reasons for financial distress could serve as a possible explanation of why the magnitude of the neighbors' lottery win results in more bankruptcies in the neighborhood. For example, tests could be done on whether larger lottery wins lead to bankruptcies of nonwinning neighbors via increased marital breakdown or any other reason among the 17 options recorded in the data.

To provide evidence on this hypothesis, in this section, we run 17 separate regressions of a dummy variable for each of the 17 reasons for financial distress against log lottery-win size. As an illustrative example for the case of marital breakdown, we regress the dummy variable indicating that marital breakdown was a reason for financial distress against log lottery-win size. A significant and positive coefficient on lottery amount in this regression would indicate that the larger the magnitude of the neighborhood lottery win, the larger the probability of filing for bankruptcy because of marital breakdown in this neighborhood. In such a case, it could be argued that marital breakdown was indeed a possible mechanism to explain our main results.

We report our results of these tests in Table 18. In all cases (with one exception of bankruptcy due to business failure), we find no statistically significant relationship between the lottery-win size of the neighbor and each of the 17 categorical reasons for bankruptcy. In other words, for all of these reasons for financial distress, we do not find support for the hypothesis that any of these various reasons for financial distress could explain our main results. The one exception is business failure in the 0- to 2-year event window. However, the estimated effect of lottery amount on this reason is negative and economically small. This specific result implies that neighbor's lottery-win size *reduces* the probability of filing for bankruptcy because of business failure. Thus, business failure cannot explain the *increased* bankruptcy filings after lottery wins, which we document in Section 5.

Of the 17 different categorical reasons for bankruptcy that we can observe in our data, one particularly interesting categorical reason for bankruptcy in our lottery-related context is bankruptcy owing to gambling. This reason allows us to test the hypothesis that some neighbors of lottery winners may have the (irrational) belief that "good luck transmits." Such a belief would imply that neighbors of a large lottery winner would increase their own gambling

activities in the hope that they would be as successful at gambling as their neighbor. Such increased gambling activities could also result in financial distress; thus, this hypothesis could be a mechanism to explain our main finding in Section 5. However, our specific result in Table 18, showing no effect of the size of the neighbor's lottery win on the number of bankruptcy filers in the neighborhood who listed gambling as a cause of their financial distress, allows us to provide evidence against this "good luck transmits" hypothesis.

Note that in all our individual-level tests with bankruptcy balance sheet data, shown previously, we include all of these 17 categorical dummy variables as control variables. Thus, we control for possible confounding factors as measured by bankruptcy reasons in all these tests.

## **9. Conclusion**

This paper provides new evidence on the hypothesis that social comparisons among peers with different levels of income can generate unsustainable increases in debt and financial distress. Our identification strategy relies on random magnitudes of lottery prizes as plausibly exogenous shocks to relative income of peers in very small neighborhoods with a median of 13 households. Our main finding is that the dollar magnitude of the lottery prize increases the number of subsequent bankruptcy filings in that neighborhood.

We propose and test a variety of possible explanations for our main finding. In terms of the behavior of the bankrupt neighbors of large lottery winners, we provide evidence consistent with conspicuous consumption and increased financial risk-taking by bankrupt neighbors as an explanation for our main results. We also find that borrowing in the entire neighborhood increases with lottery amounts. These findings are consistent with additional risk-taking and debt accumulation to finance conspicuous consumption leading to financial difficulties and bankruptcy for nonwinning peers.

We also provide evidence on three possible factors that can mitigate the additional bankruptcy risks that lenders face from the neighbors of large lottery winners. First, we show that secured liabilities increase in lottery-winning neighborhoods, while there is no relationship between unsecured liabilities and lottery-win size. This finding is consistent with lenders being able to recover more collateral in these bankruptcies because secured debt provides collateral while unsecured debt does not provide it. Second, we show that soft information on borrowers and local relative income shocks is important in this context because our main results are driven

by neighborhoods that are relatively farther away from the nearest lender. Third, we provide evidence that credit increases for prime borrowers but not for ex ante more risky subprime borrowers, which is consistent with observable credit risk (as measured by credit scores) playing an important role in reducing lenders bankruptcy losses.

While the results in this paper are based on microgeographies that examine the very close neighbors of lottery winners, future research could possibly examine whether these results are important for the entire economy. For example, it is possible that our results could have implications for the arguments linking income inequality and financial distress, based on the well-known findings of Piketty and Saez (2003) that income inequality peaked in the periods immediately prior to the 1929 and 2008 financial crises.

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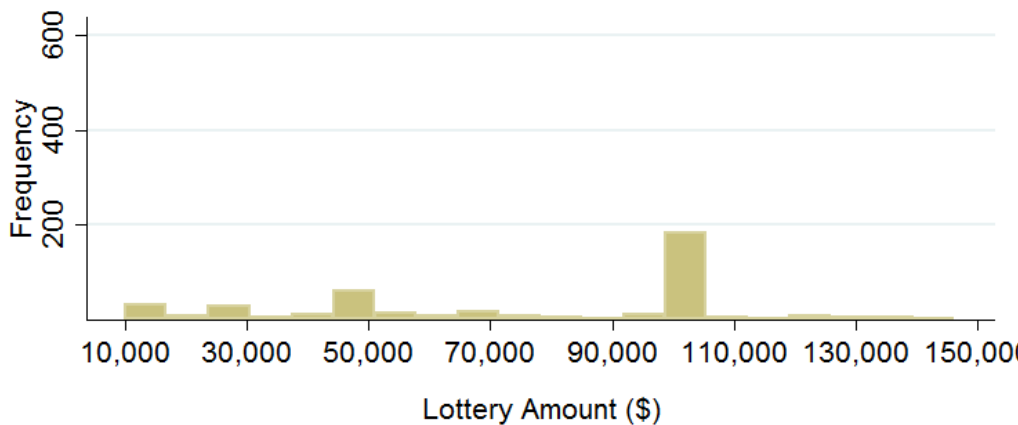
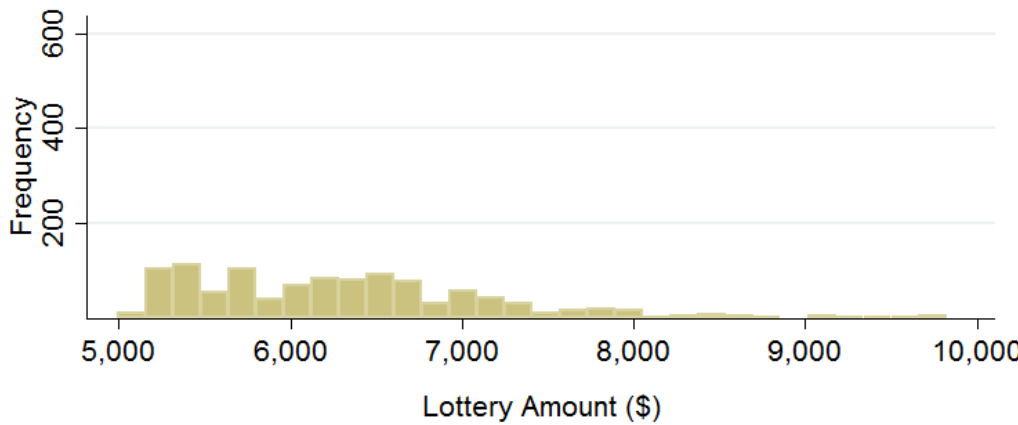
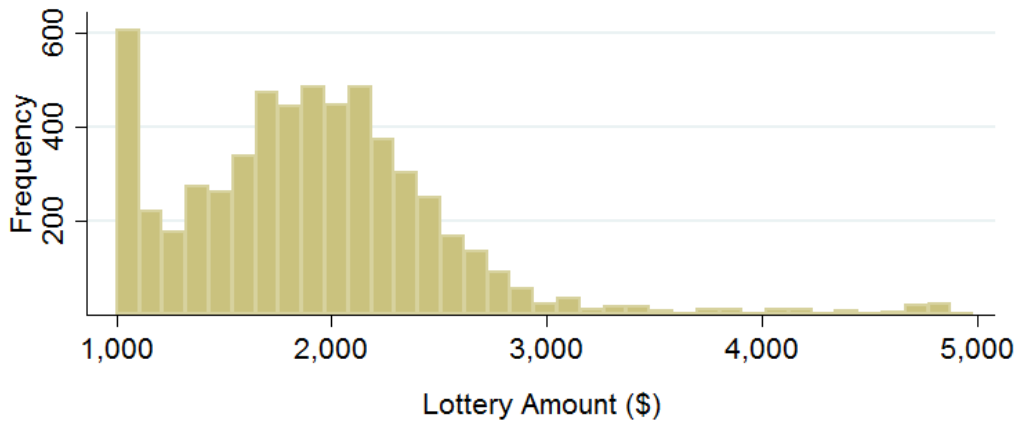
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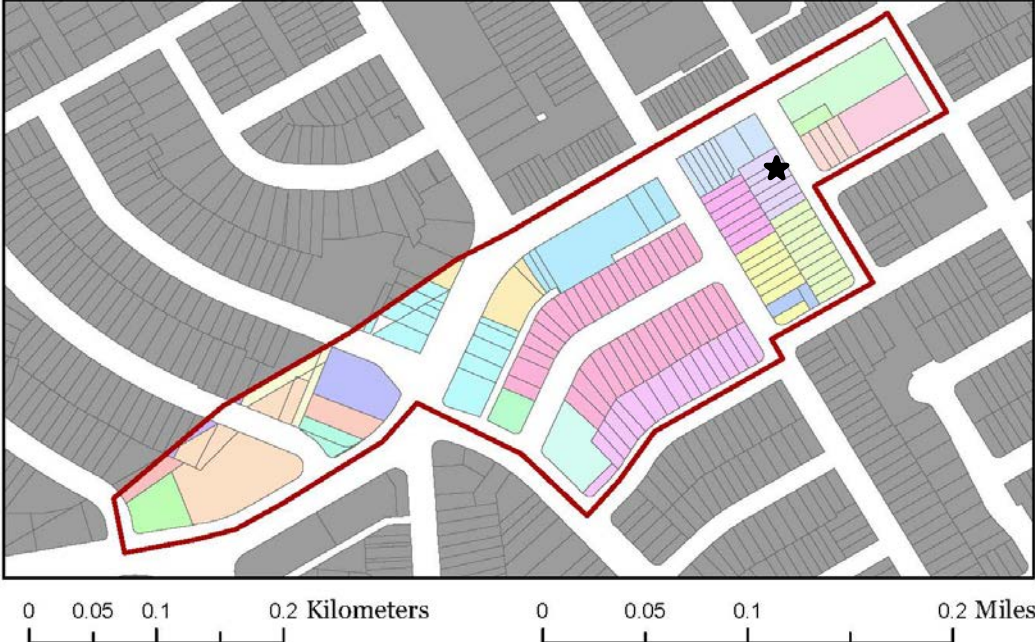
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**Figure 1. The Distribution of Lottery Prizes Among Postal Codes with Single Winners of Less Than C\$150,000**

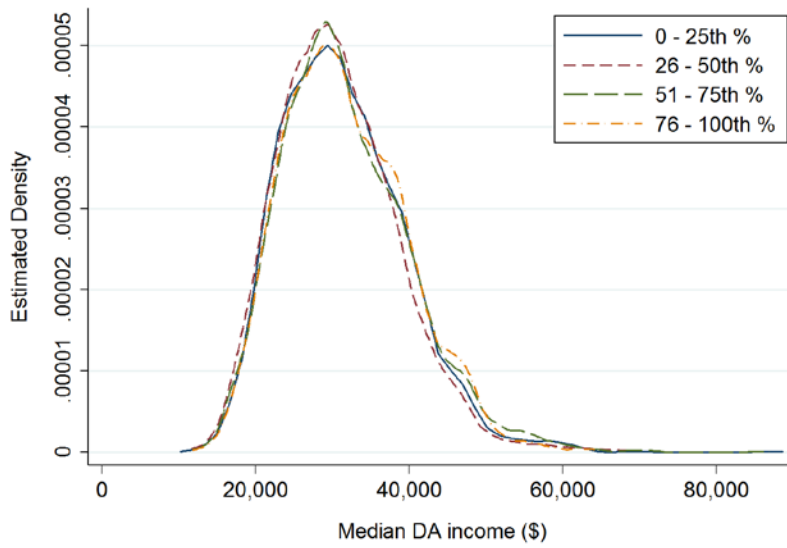


**Figure 2. Postal Codes and Dissemination Areas**



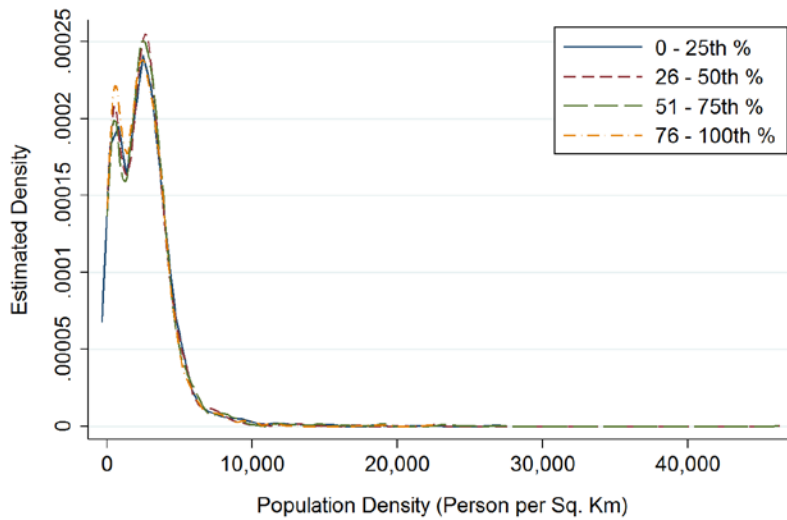
*Notes:* This map shows the size of postal code and DA neighborhoods used in this study. The smallest rectangular shapes are building lots. The colored sets of lots are postal codes. The red-lined figure is a census DA. The star represents a lottery winner. A median postal code has 13 households (dwellings). An average DA has 200 households, and its area is 0.2 square km. The source of the data in this map is the City of Toronto’s Open Data portal. This map was created by Lauren Lambie-Hanson.

**Figure 3. No Relation Between Lottery Amount and DA Median Income**



*Notes:* This figure shows DA median income distributions for the four ranges of the lottery-winning amount. There is no relation between the median income in the DA and lottery amounts.

**Figure 4. No Relation Between Lottery Amount and DA Population Density**



*Notes:* This figure shows DA population density distributions for the four ranges of the lottery-winning amount. There is no relation between population density in the DA and lottery amounts.

**Table 1. Summary Statistics of Winning Postal Code Data**

<b>Variable</b>	<b>Obs.</b>	<b>Mean</b>	<b>Std. Dev.</b>
Winning amount (\$)	7,377	6,911	19,086
Log of winning amount	7,377	7.903	0.963
Winning year	7,377	2009	3
Bankruptcy rate relative to the winning time, years:			
-1 to -5	7,377	0.701	1.348
-3 to -5	7,377	0.407	0.93
-1 to -2	7,377	0.294	0.724
0 to 2	5,352	0.455	0.967
3 to 5	2,586	0.41	0.91
0 to 5	2,586	0.799	1.474
DA Gini coefficient	7,377	0.424	0.049
Median income (\$)	7,377	31,451	8,059
Population density (persons per sq. km)	7,377	2,550	2,276
Region type (1 to 8 score)	7,377	1.578	1.241
Unemployment rate (%)	7,377	4.1	3.356
Numerical literacy score (between 100 and 500)	7,377	277	11
Divorced (proportion of DA population)	7,377	0.078	0.032
Separated (proportion of DA population)	7,377	0.028	0.015
Widowed (proportion of DA population)	7,377	0.046	0.044
High school (proportion of DA population)	7,377	0.234	0.069
Apprenticeship (proportion of DA population)	7,377	0.122	0.058
College (DA) (proportion of DA population)	7,377	0.207	0.064
University (DA) (proportion of DA population)	7,377	0.189	0.105
Graduate (DA) (proportion of DA population)	7,377	0.063	0.062
Homeownership (proportion of DA population)	7,377	0.386	0.079
Male (proportion of DA population)	7,377	0.498	0.028
Age between 20 and 39 years (proportion of DA population)	7,377	0.301	0.097
Age between 40 and 64 years (proportion of DA population)	7,377	0.335	0.067
Age over 65 years (proportion of DA population)	7,377	0.109	0.087

**Table 2. Test of Randomization: The Effect of Neighborhood Characteristics on Lottery Amount**

<b>Dependent Variable: Log (Lottery Dollar-Win Size)</b>	<b>Coefficient</b>	<b>Std. Err.</b>
DA Gini coefficient	0.274	(0.279)
Median income (\$)	5.33e-07	(2.23e-06)
Population density (persons per sq. km)	-2.82e-06	(5.46e-06)
Region type (1 to 8 score):		
2	0.0469	(0.0425)
3	0.0560	(0.0434)
4	-0.125	(0.155)
5	0.0765	(0.0741)
6	-0.116*	(0.0639)
Unemployment rate (%)	-0.00234	(0.00350)
Numerical literacy score (between 100 and 500)	0.00425*	(0.00243)
Divorced (proportion of DA population)	0.595	(0.566)
Separated (proportion of DA population)	-1.058	(1.076)
Widowed (proportion of DA population)	0.285	(0.592)
High school (proportion of DA population)	0.0502	(0.227)
Apprenticeship (proportion of DA population)	-0.0422	(0.276)
College (DA) (proportion of DA population)	-0.340	(0.251)
University (DA) (proportion of DA population)	-0.0444	(0.252)
Graduate (DA) (proportion of DA population)	-0.648**	(0.305)
Homeownership	-0.114	(0.300)
Male	-0.0878	(0.523)
Age between 20 and 39 years	0.175	(0.344)
Age between 40 and 64 years	0.219	(0.353)
Age over 65 years	0.0869	(0.389)
Constant	6.636***	(0.659)
Observations	7,377	
R-squared value	0.003	
Adj R <sup>2</sup>	0.000190	
F-test	1.061	
Prob > F	0.3822	

*Notes:* This table reports test results for the hypothesis that the log of the winning amount is affected by the region's observable attributes. The results suggest that no observable characteristic affects lottery amount, which is consistent with lottery amount being random with respect to observables. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 3. No Correlation Between Lottery Prize Size and Neighborhood Characteristics**

<b>Dependent Variable: Log (Lottery Dollar-Win Size)</b>	<b>Coefficient</b>	<b>Std. Err.</b>
DA Gini coefficient	0.135	(0.231)
Median income (\$)	1.47e-06	(1.39e-06)
Population density (persons per sq. km)	-4.99e-06	(4.93e-06)
Region type (1 to 8 score):		
2	0.0511	(0.0399)
3	0.0646	(0.0394)
4	-0.101	(0.153)
5	0.0839	(0.0699)
6	-0.111*	(0.0584)
Unemployment rate (%)	-0.00467	(0.00334)
Numerical literacy score (between 100 and 500)	0.00169*	(0.000991)
Divorced (proportion of DA population)	0.0996	(0.347)
Separated (proportion of DA population)	-0.743	(0.758)
Widowed (proportion of DA population)	-0.108	(0.254)
High school (proportion of DA population)	0.119	(0.163)
Apprenticeship (proportion of DA population)	0.0964	(0.193)
College (DA) (proportion of DA population)	-9.60e-05	(0.176)
University (DA) (proportion of DA population)	0.0863	(0.106)
Graduate (DA) (proportion of DA population)	-0.167	(0.180)
Homeownership	0.0101	(0.142)
Male	0.0210	(0.399)
Age between 20 and 39 years	0.0615	(0.115)
Age between 40 and 64 years	0.0103	(0.166)
Age over 65 years	-0.0715	(0.129)

*Notes:* This table reports test results for the hypothesis that the log of the winning amount is correlated with the region's attributes. Each row reports results from a regression of the log of the lottery amount on a variable reported in the first column. These regressions are run separately. The results suggest that no neighborhood characteristic affects lottery amount. We consider postal codes with one winning in the sample period and randomly sized prizes of above C\$1,000 and below C\$150,000. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.



**Table 4. The Effect of Lottery Win Size on the Number of Neighboring Bankruptcies**

Event Window (Years)	-1 to -5	-3 to -5	-1 to -2	0 to 2	3 to 5	0 to 5
Panel A. Winners postal codes						
Log of winning amount	0.0046 (0.0112)	-0.0045 (0.0086)	0.0096 (0.0073)	0.0199** (0.0101)	0.0266* (0.0139)	0.0681*** (0.0189)
Number of observations	7,377	7,377	7,377	5,352	2,586	2,586
Panel B. Winners outer rings (DAs-postal codes)						
Log of winning amount	-0.0192 (0.0122)	-0.0124* (0.0072)	-0.0069 (0.0056)	-0.0088 (0.0083)	0.0024 (0.0112)	-0.0086 (0.0240)
Number of observations	7,361	7,361	7,361	5,342	2,582	2,582

*Notes:* This table reports the marginal effect of the log of the lottery prize on the number of bankruptcies in the winner's closest neighborhood (postal code), excluding the winner's own bankruptcy in six event windows. This effect is estimated using a Poisson model in panel A and OLS in panel B. The number of bankruptcies per postal code in the outer ring is defined as all DA bankruptcies divided by the number of DA postal codes minus 1. This number is not an integer; hence, OLS are used with these data. All specifications include lottery product and winning-year fixed effects. Control variables are described in the text. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5%, and 1% levels, respectively.

**Table 5. The Effect of a Lottery Prize on the Count of Neighborhood Bankruptcies (Single-Year Event Windows)**

Years Relative to Winning	(1)	(2)
	Postal Code Bankruptcies	se
-5	-0.0033	(0.0047)
-4	0.0020	(0.0050)
-3	-0.0030	(0.0052)
-2	0.0075	(0.0050)
-1	0.0019	(0.0053)
0	-0.0037	(0.0054)
1	0.0064	(0.0055)
2	0.0092	(0.0056)
3	0.0129**	(0.0066)
4	0.0053	(0.0072)
5	-0.0041	(0.0080)

*Notes:* This table reports the marginal effect of the log of the lottery prize on the count of bankruptcies in the winner's closest neighborhood (postal code). Bankruptcy numbers exclude the winner's own bankruptcy. These effects are estimated using Poisson models. All specifications include lottery product and winning-year fixed effects. Control variables are described in the text. Standard errors (se) are in parentheses. \*, \*\*, \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

**Table 6. Summary Statistics of Bankruptcy Balance Sheet Data**

<b>Variable</b>	<b>Obs.</b>	<b>Mean</b>	<b>Std. Dev.</b>
<b>Characteristics of neighbor's lottery wins</b>			
Neighbor's winning amount (\$)	8,747	6,817	18,665
Log of neighbor's winning amount	8,747	7.909	0.954
Neighbor's winning year	8,747	2010	3
<b>Consumption assets values on a bankruptcy balance sheet</b>			
Cars	8,747	6,697	11,239
Houses	8,747	84,172	148,311
Motorcycles	8,747	155	1,506
Recreational equipment	8,747	739	4,761
Furniture	8,747	1,541	1,405
<b>Financial assets values on a bankruptcy balance sheet</b>			
Cash	8,747	75	1,110
Securities	8,747	622	6,911
Insurance and pensions	8,747	4,121	20,134
<b>Liabilities of bankruptcy filers</b>			
Unsecured debt	8,747	61,534	68,338
Secured debt	8,747	85,436	143,594
Total debt	8,747	147,022	174,248
Ratio of secured-to-total debt	8,747	0.289	0.345
Total debt to assets	8,693	35.471	981.652
Secured debt to assets	8,693	0.439	0.573
Unsecured debt to assets	8,693	35.017	981.663

**Table 6. Summary Statistics of Bankruptcy Balance Sheet Data**  
(continued)

<b>Variable</b>	<b>Obs.</b>	<b>Mean</b>	<b>Std. Dev.</b>
<b>Local and Individual Bankruptcy Filer Data</b>			
DA Gini coefficient	8,747	0.419	0.046
Population density (persons per sq. km)	8,747	3,016	3,579
Region type (1 to 8 score)	8,747	1.715	1.414
Filer's age (years)	8,747	43	13
Household size (count)	8,747	2.145	1.374
Divorced indicator	8,747	0.143	0.35
Prior defaults indicator	8,747	0.201	0.401
Filed after the 2009 reform indicator	8,747	0.567	0.495
Self-employed indicator	8,747	0.08	0.271
<b>Individual Reasons for Bankruptcy (Dummy)</b>			
Overuse of credit (0 or 1)	8,747	0.548	0.498
Marital breakdown (0 or 1)	8,747	0.195	0.396
Unemployment (0 or 1)	8,747	0.265	0.442
Insufficient income (0 or 1)	8,747	0.332	0.471
Business failure (0 or 1)	8,747	0.142	0.349
Health concerns (0 or 1)	8,747	0.225	0.418
Accidents/emergencies (0 or 1)	8,747	0.027	0.163
Student loans (0 or 1)	8,747	0.006	0.075
Gambling (0 or 1)	8,747	0.031	0.173
Tax liabilities (0 or 1)	8,747	0.067	0.249
Loans cosigning (0 or 1)	8,747	0.015	0.121
Bad/poor investments (0 or 1)	8,747	0.033	0.178
Garnishee (0 or 1)	8,747	0.012	0.109
Legal action (0 or 1)	8,747	0.022	0.146
Moving/relocation (0 or 1)	8,747	0.044	0.205
Substance abuse (0 or 1)	8,747	0.023	0.149
Supporting relatives (0 or 1)	8,747	0.079	0.269

**Table 7. The Effect of Lottery Prizes on Durable Consumption Assets of Neighboring Bankruptcies**

Event Window (Years)	-3 to -5	-1 to -2	0 to 2	3 to 5
Cars	-0.1042 -0.1317	0.1291 (0.0908)	0.2142** (0.0910)	-0.0024 (0.1149)
Houses	0.1624 -0.1532	0.1245 (0.1204)	0.2714** (0.1285)	-0.0006 (0.1659)
Motorcycles	-0.0351 -0.0383	0.0032 (0.0286)	0.0573* (0.0293)	0.0031 (0.0352)
Recreational equipment	-0.0357 -0.0636	0.0654 (0.0437)	0.0278 (0.0456)	-0.0761 (0.0665)
Furniture	-0.0325 -0.0834	0.0998* (0.0548)	-0.0245 (0.0532)	0.1131 (0.0718)
Number of observations	1,477	2,764	2,617	1,259

*Notes:* This table reports the effect of the log of lottery prize on the log of asset value. All coefficients are from separate OLS regressions with the log of assets value + 1 as the dependent variable. All specifications include lottery product and winning-year fixed effects. Control variables are described in Table 6 and the text. These coefficients may imply that the value of more conspicuous consumption assets in bankruptcy increases in lottery size for filers after a neighbor's lottery winning. Lottery size has no effect on the ownership of less-visible consumption assets. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 8. The Effect of Lottery Prizes on the Financial Assets of Neighboring Bankruptcies**

Event Window (Years)	-3 to -5	-1 to -2	0 to 2	3 to 5
Cash	0.0307 -0.0388	0.0268 (0.0285)	-0.0004 (0.0266)	0.0048 (0.0352)
Securities	-0.0261 -0.0678	-0.0151 (0.0388)	0.0867** (0.0373)	0.1033** (0.0493)
Insurance and pensions	0.0975 -0.1112	0.0355 (0.0822)	-0.1581* (0.0853)	-0.1604 (0.1145)
Number of observations	1,477	2,764	2,617	1,259

*Notes:* This table reports the effect of the log of lottery prize on the log of asset value. All coefficients are from separate OLS regressions with the log of assets value + 1 as the dependent variable. All specifications include lottery product and winning-year fixed effects. Control variables are described in Table 6 and the text. These coefficients may imply that the value of more financial assets in bankruptcy increases in lottery size for filers after a neighbor's lottery winning. Lottery size has no or a negative effect on the ownership of less risky financial assets. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 9. Summary Statistics of Credit Bureau Data**

<b>Variable</b>	<b>Obs.</b>	<b>Mean</b>	<b>Std. Dev.</b>
Winning amount (\$)	1,906,124	6,170	17,204
Log of winning amount	1,906,124	7.91	0.92
Winning year	1,906,124	2010	2
<b>Credit Bureau Variables</b>			
Number of credit accounts	1,487,095	11.44	9.45
Log of total balance of all accounts	1,487,095	7.78	4.25
Number of mortgage accounts	1,487,095	0.36	0.85
Log of total mortgage balance	1,487,095	2.06	4.56
Number of accounts excluding mortgages	1,906,124	10.6	9.09
Log of total balance of all accounts excluding mortgages	1,906,124	7.01	4.13
Number of bankcards	1,906,124	3.32	3.21
Log of total bankcard balance	1,906,124	4.87	3.85
Log of total bankcard limit	1,906,124	7.19	4.07
Number of auto loans	1,906,124	0.6	1.2
Log of auto loan balance	1,906,124	1.11	3.08
Number of installment loans	1,906,124	2.84	3.86
Log of installment loan balance	1,906,124	2.58	4.32

**Table 10. The Lottery Prize Effect on Credit Outcomes Within Winners' Postal Codes**

Years relative to winning	(1) Number of credit accounts	(2) Total balance of all accounts	(3) Number of mortgage accounts	(4) Total mortgage balance	(5) Number of credit accounts excluding mortgages	(6) Total balance of all accounts excluding mortgages
year -5					-0.021 (0.013)	-0.021 (0.015)
year -4	-0.030* (0.017)	-0.018 (0.018)	-0.004 (0.005)	-0.041 (0.028)	0.008 (0.009)	-0.016 (0.011)
year -3	-0.010 (0.010)	-0.009 (0.012)	-0.000 (0.003)	0.018 (0.018)	-0.001 (0.006)	-0.009 (0.008)
year -2	0.001 (0.005)	0.008 (0.008)	0.001 (0.002)	0.013 (0.011)	-0.002 (0.004)	0.006 (0.006)
year 0	0.006 (0.004)	-0.001 (0.006)	0.002* (0.001)	0.007 (0.008)	0.013*** (0.003)	-0.000 (0.005)
year 1	0.012** (0.006)	0.007 (0.007)	0.003* (0.002)	0.013 (0.011)	0.020*** (0.004)	-0.003 (0.006)
year 2	0.024*** (0.008)	0.012 (0.008)	0.004** (0.002)	0.020 (0.013)	0.028*** (0.006)	0.005 (0.007)
year 3	0.035*** (0.009)	0.013 (0.009)	0.008*** (0.002)	0.032** (0.014)	0.036*** (0.007)	0.005 (0.007)
year 4	0.037*** (0.011)	0.019** (0.010)	0.010*** (0.003)	0.053*** (0.016)	0.035*** (0.009)	0.003 (0.009)
year 5	0.048*** (0.012)	0.019* (0.011)	0.014*** (0.003)	0.067*** (0.018)	0.043*** (0.011)	0.007 (0.010)
Observations	1,487,095	1,487,095	1,487,095	1,487,095	1,906,124	1,906,124
# of individuals	296,065	296,065	296,065	296,065	330,732	330,732
R-squared within	0.102	0.0150	0.136	0.0950	0.110	0.00144
R-squared overall	0.00382	0.000905	0.0183	0.0180	0.00515	0.00260

*Notes:* This table reports the effect of the log of lottery prize on the credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. The first two years of mortgage data were removed because of a missing data problem. For this reason, mortgage regressions start in year -4 and not -5. These coefficients imply that the number and amount of mortgages increase after large lottery wins in a postal code. The number and amount of all trades also increase. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.



**Table 11. The Lottery Prize Effect on Credit Outcomes Within Winners' Postal Codes**

<b>Years relative to winning</b>	<b>(1) Number of bankcards</b>	<b>(2) Total bankcard balance</b>	<b>(3) Total bankcard limit</b>	<b>(4) Number of auto loans</b>	<b>(5) Auto loan balance</b>	<b>(6) Number of installment loans</b>	<b>(7) Installment loan balance</b>
year -5	-0.003 (0.006)	-0.003 (0.014)	-0.001 (0.012)	-0.004* (0.002)	-0.004 (0.017)	-0.009 (0.005)	-0.011 -0.02
year -4	0.002 (0.004)	-0.014 (0.010)	0.009 (0.009)	-0.001 (0.002)	-0.002 (0.013)	-0.003 (0.004)	0.005 -0.016
year -3	-0.001 (0.003)	-0.009 (0.008)	-0.011 (0.007)	-0.002 (0.001)	-0.012 (0.010)	-0.005* (0.003)	-0.013 -0.012
year -2	-0.001 (0.002)	0.006 (0.006)	-0.006 (0.005)	0.000 (0.001)	0.008 (0.006)	-0.002 (0.002)	0.005 -0.008
year 0	0.004*** (0.001)	0.005 (0.005)	0.008** (0.004)	0.001* (0.001)	0.000 (0.005)	0.003** (0.001)	0.001 -0.006
year 1	0.007*** (0.002)	0.012** (0.006)	0.013*** (0.005)	0.001* (0.001)	-0.004 (0.007)	0.005*** (0.002)	-0.003 -0.009
year 2	0.009*** (0.003)	0.011* (0.007)	0.013** (0.006)	0.002** (0.001)	0.007 (0.008)	0.009*** (0.002)	0.016 -0.01
year 3	0.011*** (0.003)	0.006 (0.007)	0.005 (0.006)	0.003** (0.001)	0.007 (0.009)	0.010*** (0.003)	0.009 -0.011
year 4	0.011** (0.004)	0.004 (0.009)	0.005 (0.008)	0.002* (0.001)	0.004 (0.010)	0.011*** (0.004)	0 -0.013
year 5	0.012** (0.005)	0.000 (0.010)	0.012 (0.009)	0.003* (0.002)	0.005 (0.012)	0.014*** (0.005)	0.008 -0.015
Observations	1,906,124	1,906,124	1,906,124	1,906,124	1,906,124	1,906,124	1,906,124
# of individuals	330,732	330,732	330,732	330,732	330,732	330,732	330,732
R-squared within	0.126	0.00250	0.00514	0.0408	0.00921	0.0406	0.00334
R-squared overall	0.007	0.001	0.0006	0.0015	0.0017	0.0018	0.000

*Notes:* This table reports the effect of the log of lottery prize on the credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. These coefficients imply that the number and amount of bankcards increase after large lottery wins in a postal code. The number of auto loans and installment loans also increase. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 12. The Lottery Prize Effect on the Liabilities of Bankruptcy Filers Within Winners' Postal Codes**

Event Window (Years)	-3 to -5	-1 to -2	0 to 2	3 to 5
Log of secured debt	0.061 (0.169)	0.079 (0.120)	0.311** (0.125)	-0.107 (0.159)
Number of observations	1,477	2,764	2,617	1,259
Ratio of secured debt-to-assets	0.000 (0.013)	0.008 (0.009)	0.031*** (0.010)	-0.009 (0.032)
Number of observations	1,462	2,751	2,603	1,253
Ratio of secured debt-to-total debt	0.011 (0.010)	0.007 (0.007)	0.021*** (0.008)	-0.010 (0.010)
Number of observations	1,477	2,764	2,617	1,259
Log of unsecured debt	-0.035 (0.024)	0.002 (0.017)	-0.018 (0.017)	0.028 (0.022)
Number of observations	1,477	2,764	2,617	1,259
Ratio of unsecured debt-to-assets	0.139 (17.281)	30.622 (35.847)	-3.808 (17.427)	1.968 (3.082)
Number of observations	1,462	2,751	2,603	1,253
Log of total debt	-0.002 (0.032)	0.018 (0.024)	0.037 (0.025)	-0.003 (0.031)
Number of observations	1,477	2,764	2,617	1,259
Ratio of total debt-to-assets	0.107 (17.282)	30.628 (35.846)	-3.751 (17.426)	1.959 (3.080)
Number of observations	1,462	2,751	2,603	1,253

*Notes:* This table reports the effect of the log of the lottery prize on the liabilities of bankruptcy filers in the years specified in the first row. All coefficients are from separate OLS regressions with dependent variables specified in the first column. All specifications include lottery product and winning-year fixed effects. Control variables are described in Table 6 and the text. These coefficients imply that secured liabilities (and their ratios) increase in lottery size for bankruptcy filers after a neighbor's lottery win. Lottery size has no effect on unsecured liabilities (or their ratios) and no effect on total liabilities. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 13. The Effect of a Lottery Win on the Number of Bankrupt Neighbors (with and Without Proximate Lenders)**

Event Window (years)	-1 to -5	-3 to -5	-1 to -2	0 to 2	3 to 5	0 to 5
<b>Panel A. No financial institutions within 0.5 km</b>						
Log of winning amount	0.0019 (0.0127)	-0.0089 (0.0097)	0.0115 (0.0082)	0.0216* (0.0113)	0.0313** (0.0156)	0.0817*** (0.0208)
Number of observations	5,924	5,924	5,924	4,309	2,110	2,110
<b>Panel B. With a financial institution within 0.5 km</b>						
Log of winning amount	0.0197 (0.0251)	0.0147 (0.0191)	0.0039 (0.0163)	0.0096 (0.0235)	0.0025 (0.0326)	-0.0031 (0.0478)
Number of observations	1,453	1,453	1,453	1,043	476	476
<b>Panel C. No financial institutions within 1 km</b>						
Log of winning amount	0.0046 (0.0112)	-0.0045 (0.0086)	0.0096 (0.0073)	0.0199** (0.0101)	0.0266* (0.0139)	0.0681*** (0.0189)
Number of observations	7,377	7,377	7,377	5,352	2,586	2,586
<b>Panel D. With a financial institution within 1 km</b>						
Log of winning amount	0.0076 (0.0152)	0.0040 (0.0116)	0.0033 (0.0099)	0.0178 (0.0138)	0.0052 (0.0195)	0.0235 (0.0267)
Number of observations	3,870	3,870	3,870	2,812	1,342	1,342

*Notes:* This table reports the marginal effect of the log of the lottery prize on the number of bankruptcies in the winner's closest neighborhood (postal code) excluding the winner's own bankruptcy in six event windows. This effect is estimated using a Poisson model. Panel A reports the results for a sample without a lender within 0.5 km, and panel B reports the results for a sample with a lender within 0.5 km. Panel C reports the results for a sample without a lender within 1 km, which is the median distance to a lender in this sample. Panel D reports the results for a sample with a lender within 1 km. All specifications include lottery product and winning-year fixed effects. Control variables are described in the text. These results imply that our main results, concerning lottery win and the number of neighborhood bankruptcies, do not hold when there is a proximate lender. These results are consistent with the geographically proximate lender being able to reduce bankruptcy risks after lottery wins. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 14. The Lottery Prize Effect on Credit Outcomes Within Winners' Postal Codes Without a Lender Within 0.5 km**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.022 (0.015)	-0.024 (0.016)	-0.006 (0.007)	-0.006 (0.015)	-0.005 (0.014)
year -4	-0.004 (0.006)	-0.047 (0.032)	0.019* (0.010)	-0.012 (0.012)	0.005 (0.005)	-0.007 (0.012)	0.015 (0.010)
year -3	-0.000 (0.003)	0.016 (0.020)	0.004 (0.007)	-0.006 (0.009)	-0.002 (0.003)	-0.007 (0.009)	-0.010 (0.008)
year -2	0.002 (0.002)	0.020 (0.013)	0.003 (0.004)	0.012* (0.007)	-0.001 (0.002)	0.008 (0.007)	-0.003 (0.005)
year 0	0.002* (0.001)	0.008 (0.010)	0.011*** (0.003)	0.002 (0.005)	0.004*** (0.002)	0.005 (0.005)	0.006 (0.004)
year 1	0.003 (0.002)	0.012 (0.013)	0.019*** (0.005)	-0.004 (0.007)	0.008*** (0.002)	0.014** (0.006)	0.009* (0.005)
year 2	0.004* (0.002)	0.015 (0.015)	0.029*** (0.006)	0.002 (0.008)	0.011*** (0.003)	0.007 (0.008)	0.007 (0.006)
year 3	0.008*** (0.003)	0.031* (0.016)	0.036*** (0.008)	0.004 (0.008)	0.014*** (0.004)	0.001 (0.008)	-0.002 (0.007)
year 4	0.010*** (0.003)	0.055*** (0.018)	0.040*** (0.010)	0.001 (0.010)	0.015*** (0.005)	-0.003 (0.010)	-0.002 (0.008)
year 5	0.014*** (0.003)	0.078*** (0.021)	0.047*** (0.013)	-0.002 (0.011)	0.017*** (0.006)	-0.013 (0.011)	0.005 (0.010)
Observations # of individuals	1,173,611	1,173,611	1,506,125	1,506,125	1,506,125	1,506,125	1,506,125
R-squared within	0.143	0.100	0.117	0.00182	0.132	0.00309	0.00595
R-squared overall	0.0193	0.0194	0.00490	0.00571	0.00738	0.00349	0.00225

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. The first two years of mortgage data were removed because of the missing data problem. For this reason, mortgage regressions start in year -4 and not -5. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 15. The Lottery Prize Effect on Credit Outcomes Within Winners' Postal Codes With a Lender Within 0.5 km**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			0.007 (0.027)	-0.004 (0.032)	0.015 (0.013)	0.011 (0.030)	0.025 (0.027)
year -4	-0.003 (0.009)	-0.034 (0.052)	-0.015 (0.020)	-0.024 (0.023)	-0.002 (0.010)	-0.040* (0.023)	-0.009 (0.022)
year -3	0.006 (0.006)	0.054 (0.036)	-0.013 (0.013)	-0.019 (0.017)	0.005 (0.006)	-0.017 (0.017)	-0.014 (0.015)
year -2	-0.001 (0.003)	0.008 (0.021)	-0.012 (0.007)	-0.013 (0.012)	0.003 (0.004)	-0.003 (0.012)	-0.010 (0.010)
year 0	-0.000 (0.002)	-0.003 (0.015)	0.017*** (0.006)	-0.009 (0.010)	0.004 (0.003)	0.003 (0.010)	0.014* (0.008)
year 1	0.002 (0.003)	0.005 (0.020)	0.021** (0.009)	-0.002 (0.012)	0.004 (0.004)	0.002 (0.012)	0.024** (0.010)
year 2	0.004 (0.004)	0.033 (0.024)	0.027** (0.012)	0.011 (0.014)	0.003 (0.006)	0.023* (0.014)	0.031*** (0.012)
year 3	0.005 (0.004)	0.026 (0.027)	0.036** (0.015)	0.006 (0.015)	0.002 (0.007)	0.021 (0.016)	0.032** (0.014)
year 4	0.009* (0.005)	0.038 (0.031)	0.018 (0.019)	0.006 (0.018)	-0.003 (0.008)	0.027 (0.018)	0.028* (0.016)
year 5	0.014** (0.006)	0.036 (0.035)	0.033 (0.023)	0.036* (0.021)	-0.001 (0.010)	0.047** (0.020)	0.034* (0.019)
Observations	313,484	313,484	399,999	399,999	399,999	399,999	399,999
# of individuals	64,908	64,908	72,297	72,297	72,297	72,297	72,297
R-squared within	0.105	0.0735	0.0822	0.000704	0.0992	0.000919	0.00274
R-squared overall	0.0117	0.0129	0.00415	0.000145	0.00450	0.000400	0.000104

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. The first two years of mortgage data were removed because of the missing data problem. For this reason, mortgage regressions start in year -4 and not -5. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table 16. Lottery Prize Effect on Credit Outcomes of Prime Borrowers**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.026*	-0.015	-0.001	0.010	0.010
			(0.014)	(0.016)	(0.007)	(0.015)	(0.012)
year -4	-0.012**	-0.076**	0.008	-0.011	0.003	-0.015	0.003
	(0.006)	(0.033)	(0.010)	(0.012)	(0.005)	(0.011)	(0.009)
year -3	-0.006*	-0.004	0.002	-0.002	0.000	-0.005	-0.007
	(0.004)	(0.021)	(0.007)	(0.009)	(0.003)	(0.009)	(0.007)
year -2	-0.001	0.008	-0.003	0.009	0.000	0.009	-0.003
	(0.002)	(0.014)	(0.004)	(0.007)	(0.002)	(0.007)	(0.004)
year 0	0.003*	0.009	0.013***	0.003	0.004**	0.005	0.005
	(0.002)	(0.011)	(0.003)	(0.006)	(0.002)	(0.006)	(0.004)
year 1	0.004*	0.011	0.025***	0.002	0.009***	0.012*	0.011**
	(0.002)	(0.013)	(0.005)	(0.007)	(0.002)	(0.007)	(0.005)
year 2	0.006**	0.025	0.034***	0.013*	0.011***	0.017**	0.012**
	(0.002)	(0.016)	(0.006)	(0.008)	(0.003)	(0.007)	(0.006)
year 3	0.010***	0.041**	0.040***	0.012	0.015***	0.007	0.007
	(0.003)	(0.017)	(0.008)	(0.009)	(0.004)	(0.008)	(0.007)
year 4	0.012***	0.062***	0.035***	0.017*	0.014***	0.007	0.006
	(0.003)	(0.019)	(0.010)	(0.010)	(0.005)	(0.009)	(0.008)
year 5	0.014***	0.059***	0.041***	0.024**	0.015***	0.009	0.010
	(0.003)	(0.021)	(0.012)	(0.011)	(0.006)	(0.010)	(0.009)
Observations	1,064,510	1,064,510	1,396,151	1,396,151	1,396,151	1,396,151	1,396,151
# of individuals	206,545	206,545	229,849	229,849	229,849	229,849	229,849
R-squared within	0.133	0.0924	0.106	0.00152	0.139	0.00417	0.00698
R-squared overall	0.0160	0.0162	0.00711	0.00272	0.00743	0.00124	0.00172

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. The first two years of mortgage data were removed because of the missing data problem. For this reason, mortgage regressions start in year -4 and not -5. These results are qualitatively similar to the results in Tables 10 and 11 (full sample), which implies that our full sample results are being driven by prime borrowers. Prime borrowers are defined as those with risk scores above 660 in all years prior to the lottery win. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table 17. Lottery Prize Effect on Credit Outcomes of Subprime Borrowers**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.004 (0.032)	-0.033 (0.031)	-0.007 (0.015)	-0.045 (0.033)	-0.039 (0.036)
year -4	0.020** (0.008)	0.052 (0.051)	0.010 (0.020)	-0.025 (0.022)	0.001 (0.009)	-0.011 (0.024)	0.024 (0.025)
year -3	0.015*** (0.005)	0.072** (0.032)	-0.006 (0.014)	-0.025 (0.017)	-0.003 (0.007)	-0.019 (0.018)	-0.024 (0.019)
year -2	0.005* (0.003)	0.023 (0.019)	0.002 (0.008)	-0.002 (0.011)	-0.003 (0.004)	-0.003 (0.012)	-0.016 (0.012)
year 0	0.001 (0.002)	0.004 (0.013)	0.013** (0.006)	-0.008 (0.008)	0.005** (0.003)	0.004 (0.009)	0.014 (0.009)
year 1	0.001 (0.003)	0.017 (0.018)	0.004 (0.009)	-0.018 (0.011)	0.003 (0.004)	0.012 (0.011)	0.014 (0.011)
year 2	0.001 (0.003)	0.009 (0.022)	0.012 (0.012)	-0.018 (0.013)	0.004 (0.005)	-0.005 (0.014)	0.014 (0.014)
year 3	0.003 (0.004)	0.007 (0.024)	0.027* (0.015)	-0.013 (0.015)	0.004 (0.007)	0.003 (0.016)	-0.001 (0.016)
year 4	0.006 (0.005)	0.025 (0.030)	0.036* (0.020)	-0.040** (0.018)	0.003 (0.009)	-0.002 (0.020)	-0.002 (0.019)
year 5	0.016*** (0.006)	0.102*** (0.037)	0.043 (0.026)	-0.054** (0.024)	0.002 (0.012)	-0.030 (0.026)	0.011 (0.024)
Observations	422,585	422,585	509,973	509,973	509,973	509,973	509,973
# of individuals	89,520	89,520	100,883	100,883	100,883	100,883	100,883
R-squared within	0.144	0.104	0.139	0.00285	0.0947	0.00157	0.00375
R-squared overall	0.0243	0.0247	0.00186	3.18e-05	0.00276	0.000318	9.96e-07

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. The first two years of mortgage data were removed because of the missing data problem. For this reason, mortgage regressions start in year -4 and not -5. In contrast to Table 16, these results show insignificant relationships between lottery amount and credit variables for subprime borrowers, which is consistent with our full sample results (Tables 10 and 11) being driven by prime borrowers. Subprime borrowers are defined as those with risk scores less than or equal 660 in any year prior to the lottery win. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table 18. The Effect of a Lottery Win on 17 Categorical Reasons for Bankruptcy Provided by Filers**

<b>Dependent Variable</b>	<b>(1)</b> <b>Years 0 to 2</b>	<b>(2)</b> <b>Standard</b> <b>Errors</b>	<b>(3)</b> <b>Years 3 to 5</b>	<b>(4)</b> <b>Standard</b> <b>Errors</b>
Marital breakdown	0.0050	(0.0086)	0.0014	(0.0108)
Unemployment	0.0040	(0.0101)	0.0066	(0.0124)
Insufficient income	0.0156	(0.0105)	-0.0186	(0.0142)
Business failure	-0.0269***	(0.0095)	-0.0031	(0.0109)
Health concerns	0.0056	(0.0095)	-0.0127	(0.0117)
Accidents/emergencies	0.0023	(0.0034)	0.0033	(0.0064)
Overuse of credit	0.0054	(0.0115)	0.0095	(0.0146)
Gambling	0.0009	(0.0037)	-0.0004	(0.0061)
Tax liabilities	0.0017	(0.0056)	-0.0029	(0.0082)
Loans cosigning	-0.0050	(0.0043)	-0.0004	(0.0044)
Bad/poor investments	-0.0058	(0.0053)	0.0027	(0.0045)
Garnishee	0.0026	(0.0027)	-0.0048	(0.0053)
Legal action	-0.0058	(0.0045)	-0.0048	(0.0057)
Moving/relocation	0.0056	(0.0043)	0.0014	(0.0059)
Substance abuse	-0.0056	(0.0044)	-0.0024	(0.0059)
Supporting relatives	-0.0058	(0.0066)	-0.0029	(0.0101)
Student loans	0.0000	(0.0049)	-	-

*Notes:* This table reports the effect of the log of the lottery prize on the reasons for bankruptcy filing. All coefficients are from separate logistical regressions with an indicator for a particular reason as the dependent variable. All specifications include lottery product and winning-year fixed effects. Insignificant coefficients may imply that lottery size does not increase probability of filing for any of these 17 categorical reasons. The negative coefficient for business failure suggests that the lottery amount may decrease bankruptcies because of this reason. Standard errors (se) are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively. The regression for student loans in years 0 to 5 did not converge.



**Peers' Income and Financial Distress:  
Evidence from Lottery Winners and Neighboring Bankruptcies**

Sumit Agarwal, Vyacheslav Mikhed, and Barry Scholnick

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**Online Appendix**

## **1. Sample Selection**

### **1.1. The Inclusion of Postal Codes with Only a Single Lottery Win**

In this paper, we use neighborhoods with only a single lottery win in the postal code over the period of our sample (April 1, 2004–March 31, 2014). The primary reason for this restriction is that we examine the long-term effects of lottery wins (five years before and after the lottery win to test for pretrends) and the difficulties in interpreting the effects of multiple events (i.e., multiple lottery wins in the postal code) within the same study period. By restricting our sample to only postal codes with a single lottery win over our sample period, we have a clean test with a single exogenous shock in which the magnitude of the shock is random. The reason we require such long event windows reflects the finding from the bankruptcy literature (e.g., Hankins, Hoekstra, and Skiba, 2011, and many others) that the lags between an exogenous shock and a bankruptcy filing are long and variable.

### **1.2. Removal of Fixed-Prize Lotteries**

A central element of our identification strategy is that the dollar magnitude of the win should be randomly assigned. Our lottery winner data include details of the exact nature of each type of lottery game; thus, we are able to identify and remove lottery wins in which there is a fixed rather than a random payout. In the majority of lottery games included in our data, the amount of the win is determined by dividing the size of the pool by the number of winners (i.e., the amount of the win is random). In some of the lottery games in our data, the amount won is determined by how many correct numbers are chosen (e.g., all six correct numbers result in a payment of C\$100,000, while each fewer correct number chosen results in sequentially lower payments). Because of the variation in the amount won across winners, which is conditional on the number of random numbers chosen, we include such games in our study. Some lottery games in our data, however, have a fixed payout (e.g., “every winner wins C\$1,000”), which we exclude from our study. These excluded fixed-lottery prizes have no influence on the sample of included random lottery prizes because random lottery prizes are independent of fixed lottery prizes or any other shocks to the neighborhoods, which we show in the main text.

### 1.3. The Removal of Very Large Winners

We also exclude from our sample all very large lottery wins of more than C\$150,000 (which can be many million dollars in magnitude) to reduce the possible impact of very large outliers. Hankins et al. (2011) use the same cutoff of US\$150,000 to exclude extremely large lottery winners from their sample. Our largest winner, for example, is more than C\$41 million. There are only 151 winners with prizes above C\$150,000. The 98th percentile of lottery amount in our sample is C\$151,578; thus, when we exclude prizes above C\$150,000, we basically remove wins that are larger than the 98th percentile of all the prizes in the sample (see Figure A1). In particular, there are 92 winners with prizes between C\$150,000 and \$1 million, 45 winners between C\$1 million and C\$5 million, and 14 winners above C\$5 million. The log transformation of the lottery amount lessens the effect of the issue, but it does not solve it completely (see Figure A2). We can reduce the effect of outliers by winsorizing or trimming. Winsorizing is simply replacing extreme values (e.g., a win of more than C\$41 million) with the winsorized percentile value (e.g., C\$151,578), if we winsorize at the 98th percentile. We do not believe this is an appropriate strategy in this case because these values are so dramatically different. Thus, we would introduce a large measurement error by performing this procedure. An alternative often offered alongside winsorizing is trimming. We argue that trimming is a lesser evil in this particular case. Thus, using essentially the same cutoff as Hankins et al. (2011), we trim at the 98th percentile of the original lottery amount distribution and, in total, delete 151 observations, representing 2% of the original sample.

In addition to these econometric motivations concerning removing the very largest lottery wins from the data, there are at least two institutional reasons why this is also important. The first reason is the issue raised by Hankins et al. (2011) in their study of the effect of a lottery win on the winners' (rather than the neighbors') bankruptcy, regarding the issue of lottery winners moving to another house. The argument of Hankins et al. (2011) is that winners of very large lottery prizes are much more likely to move to another neighborhood. Thus, they choose to remove very large winners from their data. To address this issue, we follow the methodology of Hankins et al. (2011) exactly in removing extremely large lottery winners from our sample.

The second institutional motivation for our dropping large lottery wins relates to an important assumption we need to make, which is that neighbors are unaware that the source of the income shock to the winner is a lottery win. If the neighbor is aware that the income shock is

from a lottery win, then this shock may not generate peer effects if the neighbor believes the income shock is random and is unlikely to be repeated. In our data, we are not able to see whether the neighbor can observe that the lottery win is the source of the income shock. However, we can exploit the fact that larger lottery wins are typically reported in local news media as part of lottery organizer's advertising campaigns. By removing the very largest lottery wins from our sample, we are increasing the probability that neighbors do not hear about the lottery win through the local news media and limit the effect of this potential confounding factor.

#### **1.4. Removal of Winners Who Also File for Bankruptcy**

Our research focuses on nonlottery-winning neighbors of a lottery winner who subsequently file for bankruptcy; thus, it is not appropriate to include in our data winners who filed for bankruptcy. Hence, in our bankruptcy filer data, we identify and exclude lottery winners in a postal code who also filed for bankruptcy.<sup>24</sup> There are 824 lottery winners in our sample who filed for bankruptcy at some stage (either before or after the lottery win), all of whom we exclude from our main bankruptcy filer sample. To identify such individuals, we exploit the fact that our data include the first and last names and six-digit postal codes of all bankruptcy filers and of all lottery winners. Because of the very small size of postal codes (median of 13 households), we argue that it is unlikely that two individuals with the same first and last names would live in the same postal code. We argue that our ability to match individuals based on first and last names and six-digit postal codes is very high.

Even though the main focus of this paper is on the impact of lottery winners on the bankruptcy filings of *neighbors*, in Section 4 of this Appendix, we also report regression results examining the impact of lottery winnings on the winner's *own* bankruptcy filings, which is very similar to the tests run by Hankins et al. (2011). Overall, these results are somewhat similar to those reported by Hankins et al. (2011), showing that larger lottery wins postpone bankruptcies of winners relative to small lottery wins. However, these coefficients are only marginally statistically significant (at the 10% levels) possibly because of a small sample size and low statistical power issues.

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<sup>24</sup> We delete any personal identifiable information (PII) after this exercise. No PII is used in any analysis.

### 1.5. Winners Who Subsequently Move from the Neighborhood

An important issue raised by Hankins et al. (2011) is the possibility that large lottery winners may be more likely to move out of the neighborhood after their lottery win compared with small lottery winners.

Our first response to this issue notes that an important advantage of our credit bureau data (described in Sections 3.4 and 3.5 of the main text) is that we can identify and remove all individuals in the neighborhood (both winners as well as neighbors) who move to a new neighborhood. This is because the credit bureau data are annual panels, with an updated postal code reported for each individual for each year. In all our tests using credit bureau data, we have removed all individuals who change postal codes during the course of the sample.

However, as is the case with Hankins et al. (2011), our bankruptcy and lottery data do not allow us to observe whether lottery winners subsequently move from the neighborhood. We argue, however, that our study examining the impact of winners on their *neighbors* provides us with important econometric advantages relative to most studies in the lotteries literature. This lets us examine the impact of lottery wins on the winners *themselves*. If large winners in our study moved to new neighborhoods after their win, we argue that they would at least have *some* influence on their original neighbors during the period from the date of their win to the date of their move. If there were a reduced impact on neighbors because large winners moved out of the neighborhood, this reduced influence would bias our estimated coefficients (which reflect the extent to which winners influence neighbors) toward zero. In other words, the significant coefficients that we report in our main text are significant despite the possibility that some large winners may have moved from the neighborhood at some date after their win.

By comparison, in many winner-focused lottery studies, if a winner moves before the outcome of interest (e.g., bankruptcy), then that winner would typically not appear in the data. Because data matching in these studies typically involves matching names and addresses in both lottery data and outcome data, if an individual winner moves before the outcome of interest occurs, she would not be matched in the data. However, in our neighborhood-based study, *all* lottery-winning neighborhoods (postal codes) appear in our data whether or not the lottery winner subsequently moves to another neighborhood.

Hankins et al. (2011) also provide some suggestive evidence on the issue of moving by showing that there is no significant difference between large and small lottery winners appearing

at the same address in telephone books in the years following the date of their lottery win. This indicates that for the larger lottery winners in their sample (a maximum of US\$150,000), there does not seem to be a systematic tendency for large winners to move. (They argue that this evidence is only suggestive because telephone book listings of landline telephones are only partially reflective of all addresses.) This suggestive evidence is useful to us particularly because the magnitudes of the lottery wins in our study are very similar, by design, to the magnitude of lottery wins in the Hankins et al. (2011) study.

### **1.6. Missing Data in Credit Bureau Mortgage Data**

One possible concern with our credit bureau data is that, for technical reasons, some banks did not report information on mortgage accounts to the credit bureau in the early years of our sample. This issue was particularly prevalent in the years 2007 and 2008, the first two years of our sample. Because our credit bureau data report the sum of credit outstanding across all accounts of a specific type of credit (e.g., sum across all mortgage accounts), it is not possible for us to identify exactly which individuals in the data are subject to this problem.

It is important to emphasize, however, that this problem only affects credit bureau *mortgage* data and not any other credit bureau data for other types of credit products. Our credit bureau data for all other credit products we examine in our tests (including bankcard data, auto loan data, and installment credit data) all reflect the universe of credit file information from every bank for every time period in our sample. Thus, our results for bankcards, auto loans, and installment loans are not at all affected by this missing data issue. We also included two separate measures of total liabilities: (1) total liabilities including mortgages and (2) total liabilities excluding mortgages, in which the issue of the missing mortgage data affects the former but not the latter.

Based on institutional details on this missing mortgage data issue, we adopt a few econometric arguments and procedures to attempt to address this issue. First, the data show that this issue of missing mortgage information was particularly severe in the first two years of our sample (2007 and 2008). For this reason, we eliminate these two years for all our tests involving mortgage data. (We use all data for all years when examining other credit products, such as bankcards, auto loans, and installment loans.) The implication of dropping these early years from

our mortgage data (and data on total liabilities including mortgages) is that we have observations for four years (rather than five years) prior to lottery wins for mortgage data.

Second, because the missing data problem occurred early in our sample period, we use as our base year -1 relative to the lottery-win date to minimize the effect of this missing data issue on our panel data results. Because we know that the missing mortgage data problem occurred in the early years of our sample, by using a base year -1, it is less likely that the base year could be affected by the missing data problem, compared with an earlier base year such as year -5.

In addition to these empirical strategies, we can also make an econometric argument that the decision of a specific bank — whether or not they reported their mortgage data to the credit bureau in a specific year — is orthogonal to our key independent variable (the magnitude of the lottery win in a neighborhood). Because the reason for the missing data (whether a bank reported mortgage data to the credit bureau in a specific year) is orthogonal to our plausibly exogenous variation, we argue that the missing mortgage data should not cause any systematic bias in our results comparing different individuals whose neighbors won different-sized lottery prizes.

In summary, while we have attempted to address this issue of missing mortgage data, we believe that our credit bureau results containing other types of credit (bankcards, auto loans, installment credit as well as total credit outstanding excluding mortgages) are more rigorous than the tests involving mortgage data (and total credit outstanding including mortgages). This is because none of these other kinds of nonmortgage credit products are subject to the problem of missing data described here.

## **2. Institutional Details**

In this section, we describe institutional details regarding mortgage debt as well as consumer bankruptcy in Canada. In particular, we highlight significant differences between Canadian and U.S. institutions in these contexts.

### **2.1. Mortgage Debt in Canada**

Our discussion summarizes a more detailed discussion of Canadian mortgage institutions in the Bank of Canada (2013), the Canadian Association of Accredited Mortgage Professionals (2015), Crawford (2015), the Financial Consumer Agency of Canada (2017), and Bordo, Reddish, and Rockoff (2015).

There are two possible mechanisms by which Canadian homeowners can increase their holdings of mortgage credit secured by their property. The first is through a home equity line of credit (HELOC), which enables the individual to borrow for consumption purposes on the secured mortgage. The second is through the relatively short-term nature of Canadian mortgage contracts (typically from six months to five years), which allows the debtor to periodically refinance a mortgage and renegotiate the terms of the mortgage contract, including the total amount of credit allocated. We discuss each of these channels in turn.

We first discuss HELOCs, which are automatically included with many Canadian mortgages. A Canadian “readvanceable” mortgage is essentially a standard amortized secured mortgage that is packaged with a HELOC (see Financial Consumer Agency of Canada (2017) for detailed institutional descriptions). In simple terms, a readvanceable mortgage will be issued for a certain maximum mortgage amount (i.e., a mortgage credit limit). As the borrower begins to pay off the balance amount owing on the mortgage, there will be an increasing gap between the initial mortgage credit limit and the current principal balance amount owing on the mortgage. Under a readvanceable mortgage contract, the borrower will be able to borrow the difference between the original principal balance on the mortgage and the current mortgage principal balance outstanding for any purposes chosen by the borrower, including consumption, etc. The readvanced (i.e., HELOC) loan as well as the remaining mortgage principal balance outstanding are both secured by the value of the house. It is also possible for the debtor to maintain use of the HELOC portion of the readvanceable mortgage secured by the house *after* the original mortgage is paid in full, thus allowing access to the full line of credit up to the original loan balance for consumption purposes. One possible reason why bankrupts may have been using their HELOCs secured by their houses to fund their increased consumption is that HELOC-based secured loans are typically offered at significantly lower interest rates compared with other types of credit such as credit card loans.

Further institutional details about readvanceable mortgages and HELOCs in Canada are provided by the Financial Consumer Agency of Canada (2017). The Financial Consumer Agency of Canada (2017, p. 2) provides evidence that HELOCs constituted “nearly 40% of non-mortgage consumer debt in 2010. In comparison, credit cards have consistently represented around 15% of non-mortgage consumer debt.” Across Canada, HELOCs have “an average outstanding balance of \$70,000” (p. 3). In addition, the Financial Consumer Agency of Canada



(2017, p. 4) notes that “in practice, readvanceable mortgages ... serve as the default option for consumers purchasing a home with a down payment of at least 20 percent. During the industry review, banks explained that creditworthy customers are generally steered towards readvanceable mortgages, rather than traditional, amortized mortgages.” While a few HELOC accounts are issued without being bundled with a mortgage, approximately 80% of HELOCs are bundled with mortgages as readvanceable mortgages.

The second distinctive element of the Canadian mortgage system through which borrowers can increase their access to mortgage credit is the short duration of mortgage contracts. Mortgage contracts in Canada are available for various durations, from six months to five years; 67% of mortgages have five-year terms (CAAMP, 2015); the remainder have less than five-year terms. Under the Canadian system, when the mortgage contract ends, the consumer has the freedom to select a new mortgage contract with new terms from any mortgage provider in the market. Both the debtor and the creditor must agree to any new contract, so either could seek new terms to the contract. The standard amortization period in Canada is 25 years (compared with 30 years in the U.S.); thus, a mortgage consumer who only selects to use a succession of five-year mortgages will have at least five different mortgage contracts over time until the mortgage is paid off in full. Under some conditions, the borrower can also negotiate an early end to an old contract before the contract end date. An important element of this mortgage refinancing in our context is that, with a new mortgage contract, the debtor may request and negotiate a larger limit on the new mortgage. The difference between the balance on the new mortgage and the remaining balance on the old mortgage can be used for additional consumption or other purposes. This setup is similar to the cash out refinancing option used in the U.S. during the housing boom.

## **2.2. Personal Bankruptcy in Canada**

Our discussion of Canadian consumer bankruptcy law and institutions is based on Wood (2015) and Houlden, Morawetz, and Sarra (2015). An important distinction between the regulation of bankruptcy in Canada and the United States is that bankruptcy in Canada is federally regulated by a single regulator: the Office of the Superintendent of Bankruptcy (OSB). In the U.S., there are 94 separate bankruptcy court districts, each of which receives bankruptcy petitions. The fact that there is only a single bankruptcy regulator in Canada allows us to access

our bankruptcy filing data from a single source: the OSB. Thus, our data contain the *universe* of bankruptcy filings in Canada, because under Canadian bankruptcy law, every bankruptcy filing has to be made to the OSB. In addition, under Canadian bankruptcy law, filers are required to submit their full balance sheet to the OSB as of the date of the bankruptcy filing. The OSB provides the balance sheet data, which are an important part of our data in this paper.

The Canadian bankruptcy system is similar to the U.S. system in that there are two main types of consumer bankruptcy in Canada, which are somewhat similar to Chapter 7 and Chapter 13 bankruptcies in the U.S. The legal process called “bankruptcy” in Canada resembles Chapter 7 bankruptcy in the U.S. in that a successful filer can write off debt in exchange for liquidating the nonexempt assets used to repay debts to creditors. The legal process called a “proposal” in Canada resembles Chapter 13 bankruptcy in the U.S., in that there is a negotiated agreement between the debtor and creditors to reduce or delay debt repayments without any liquidation of assets. Both Canadian bankruptcies as well as Canadian proposals are regulated by the OSB under the terms of the Canadian Bankruptcy and Insolvency Act (BIA). All our data in this paper include both Canadian bankruptcies as well as Canadian proposals. For the purposes of this paper, to be consistent with the nomenclature of the existing literature, we refer to the total of Canadian bankruptcies and Canadian proposals as “bankruptcies.”

Another important element of Canadian bankruptcy law is that a licensed bankruptcy trustee is the only one who can submit a bankruptcy filing to the OSB. A trustee is a professional (typically an accountant), specifically licensed by the OSB to conduct the process of bankruptcy. The role of the trustee is to act as an impartial officer of the court between debtors and creditors. The fact that the trustee is an impartial officer of the court is important in our context because the trustee determines the values of all balance sheet data used in this paper, based on legal standards established by the OSB. For example, the trustee is required to determine the value of all assets held by the debtor (e.g., houses), based on an impartial assessment of current market values of those assets.

Canadian bankruptcy law (similar to bankruptcy law in many other countries) makes a critical distinction in the ways that secured and unsecured debts are treated in bankruptcy. Broadly speaking, under Canadian bankruptcy law, creditors have a claim on the value of the collateral on the secured debt, while unsecured creditors receive the remainder of the bankrupt’s estate after secured creditors are paid. In practice, unsecured creditors often receive close to

nothing. In this paper, we argue that one possible implication of these differences in bankruptcy law is that creditors, who are concerned with future bankruptcy by a debtor, may be more likely to provide secured, rather than unsecured, credit. This is because, if the credit is secured, the creditor will have a claim on the value of the collateral after a bankruptcy filing.

The bankruptcy literature has highlighted various other institutional issues, which could affect bankruptcy filings, such as filing fees and strategic default. Our main argument in this regard is that our lottery-based identification strategy hinges on the assumption that the dollar magnitude of the neighbors' lottery win should be uncorrelated with institutional issues such as filing fees and strategic default. The OSB completely regulates these filing fees in Canada. There was a change in these filing fee regulations in 2009. For this reason, we include a dummy variable to reflect this regulatory change in our balance sheet regressions. Strategic default has often been described in the literature, but because of its strategic nature, it is usually considered difficult to identify. In our context, it can be considered to be unobservable. Our key point is that there does not seem to be any reason why unobservables such as strategic default should be correlated with the random magnitude of the lottery win in the neighborhood.

### **3. Heterogeneous Effects of Lottery Wins**

In Section 3.3 of the main text, we describe our DA-level census data, which provides observable characteristics for every DA in Canada. In this section, we use these data to examine various dimensions of neighborhood heterogeneity by splitting our main sample of bankruptcy count data into subsamples based on various DA-level observables.

#### **3.1. Neighborhood Heterogeneity: Percentage of Low-Income Individuals**

Table A1, panels A and B, report the results when we split the sample based on Statistics Canada measures of the prevalence of low-income individuals in each DA. We use the low-income cutoff (LICO) measure used by governments in a wide variety of programs targeting low-income individuals. Panel A shows the results for low-income neighborhoods (DAs) in which the proportion of low-income individuals is above the median level across DAs, and panel B shows the results for high-income neighborhoods in which the proportion of low-income individuals is below the median level of this proportion across all DAs.

The coefficients in Table A1, panel A, are larger and different from zero at higher levels of significance (5% level for years 0 to 2 after a lottery win and 1% level for the 3- to 5-year-event window) than the full sample results reported in Table 4, panel A. Table A1, panel B, shows that no coefficients are significant for postal codes with few low-income individuals (in which the proportion of low-income individuals is below the median in the sample). These results are, therefore, consistent with the argument that financial distress because of relative income shocks documented in this study (lottery winners causing neighboring bankruptcy filings) is stronger in poorer neighborhoods.

### **3.2. Neighborhood Heterogeneity: Income Inequality**

In Table A1, panels C and D report coefficient estimates based on splitting the sample at the median level of DA Gini coefficient, which is a measure of income inequality. Panel C reports results for DAs with above-median income inequality, and panel D shows results for areas with below-median income inequality. Panel C shows significant coefficients for the 3- to 5-year event window (at the 1% level), which are both larger and more precisely estimated compared with our full sample results in Table 3, panel A. In Table A1, panel D shows that no coefficients are significant for the sample of below-median income inequality DAs. In other words, these results are consistent with our main bankruptcy results being more important in high-income inequality relative to low-income inequality neighborhoods.

### **3.3. Neighborhood Heterogeneity: Urban versus Rural**

In Table A1, panels E and F report the results when we examine a different form of heterogeneity based on the urban or rural character of neighborhoods. Table A1, panel E, provides coefficients for a sample of postal codes within DAs in Canada classified by Statistics Canada as being urban and inside metropolitan statistical areas. Canada is a largely urban country, which is reflected in the sample size of the urban postal codes in Table A1, panel E, which is a relatively large fraction of the total sample. The results in panel E show that, in the years 0 to 2 after lottery wins, the estimated coefficient is both larger and estimated with greater precision compared with the full sample estimate in Table 4, panel A (significant at 1%, whereas the full sample estimate was only significant at 5%).

Table A1, panel F, shows results for postal codes that are not classified as being urban by Statistics Canada. Interestingly, while the coefficient for the 0- to 2-year-event window is not statistically significant, the 3- to 5-year-event window's coefficient is large and significant at the 5% level. This result may suggest that financial distress after relative income shocks may take longer to appear in rural neighborhoods compared with urban neighborhoods examined in Table A1, panel E. This finding is roughly consistent with the argument that it is easier for urban individuals relative to rural individuals to observe the consumption patterns of neighbors.

### **3.4. Heterogeneity and Evidence of Status Contests**

In this section, we use DA (Census Dissemination Area) level data on a wide variety of neighborhood (DA-level) observables to examine one specific theory of social interaction, which states that social signaling may occur predominantly among those individuals who engage in status contests. In the context of our study, this argument implies that individuals involved in status contests will be more likely to respond to a lottery win of their neighbor by increasing consumption and increasing bankruptcy outcomes.

To test this hypothesis, in Table A2, we replicate our main model (equation (1) in the text), with the exception that we interact our main exogenous variable, the size of lottery win, with observable characteristics of the neighborhood (DA level), reflecting various neighborhood observables that could be indicative of status contest type behavior. We use six DA-level variables including average age, marriage rate, income inequality, prevalence of low income, share of immigrants, and population density. We have selected these six variables because an argument could be made that individuals with these characteristics could engage in “status contests” (e.g., live in young neighborhoods, live in unmarried neighborhoods, live in unequal neighborhoods, live in low-income neighborhoods, live in immigrant neighborhoods, live in high-population density neighborhoods).

Our results for these specifications are presented in Table A2. Similar to Table 4, we report these coefficients for the six event windows from year -5 prior to the lottery win time to year 5 after the lottery win. We find that the interaction of the neighborhood average age with the lottery amount is negative and statistically significant in a regression with the number of postal code's bankruptcies on the left-hand side. This result can thus be argued to be consistent with the “status contest” hypothesis (i.e., that young people may be more prone to income comparisons,

compared with old people, and may be more likely to file for bankruptcy after neighbor's large lottery wins compared with small lottery wins). On the other hand, however, we do not find significant coefficients for all of the other five neighborhood variables we interacted with log lottery wins (marriage rate, income inequality, prevalence of low income, share of immigrants, and population density), so we can conclude that "status contest" hypothesis is not supported by the data in case of these five characteristics.

#### **4. The Effect of Lottery Wins on Winners' Own Bankruptcy**

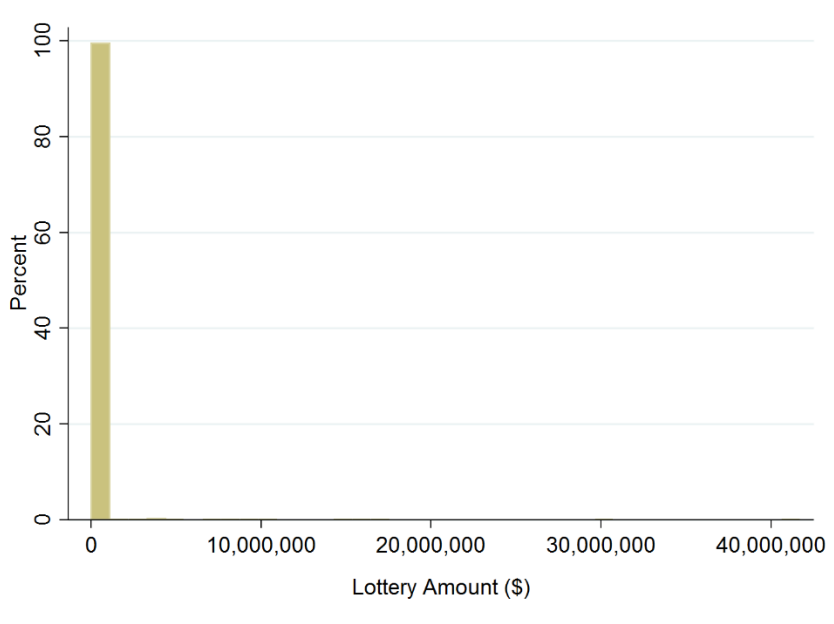
Even though our focus in this paper is on the effect of lottery winners on the bankruptcy filings of *neighbors*, in this section, we report results from regressions examining the effect of lottery winnings on the winners' *own* bankruptcy filings, which is very similar to tests run by Hankins et al. (2011). These models use data from all individual winners, with the log of the lottery-win size being the main exogenous independent variable of interest. The dependent variable is a binary indicator capturing whether a winner filed for bankruptcy in years before or after lottery wins. Given that neighbors play no role in this regression (which examines lottery-win size on the probability of the winners' *own* bankruptcy filings), we are not required to restrict our sample to only postal codes with a single lottery win, as in the main part of the paper.

Summary statistics for these data are provided in Table A3, and test results are presented in Table A4. Table A4, panel A, replicates the sample used in the main text (only a single winner in each postal code), while panels B and C include all lottery winners in the sample. Most coefficients in Table A4 are statistically insignificant, but we do find evidence (at the 10% significance level) that the size of winning increases bankruptcies of winners in the 3- to 5-year-event window in panel C. This result is roughly consistent with the findings reported by Hankins et al. (2011) that large prize winners file for bankruptcies later than small prize winners. Our smaller sample size compared with Hankins et al. (2011) may explain the low statistical power issues in our case.

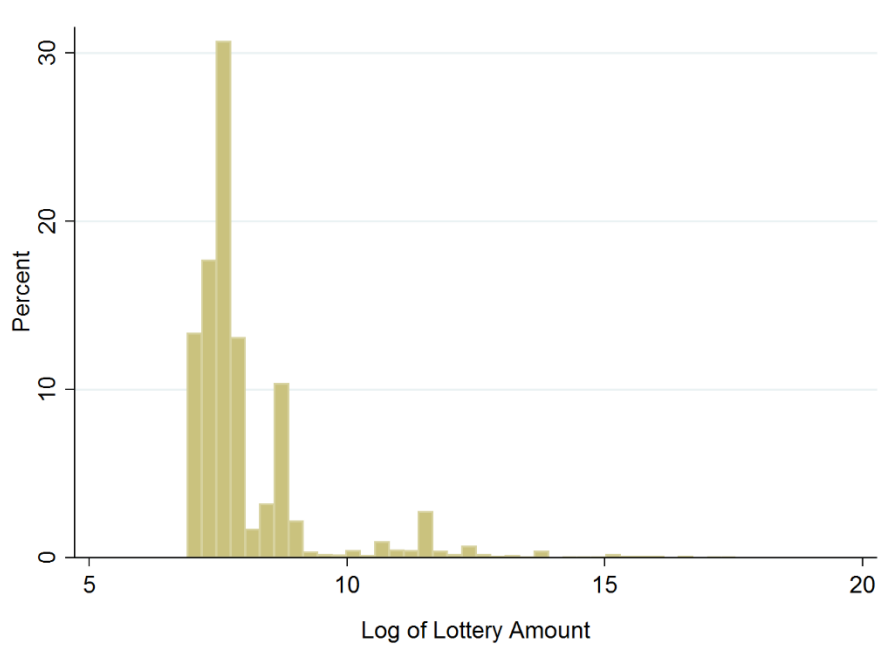
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**Figure A1. Lottery Prize Amounts**

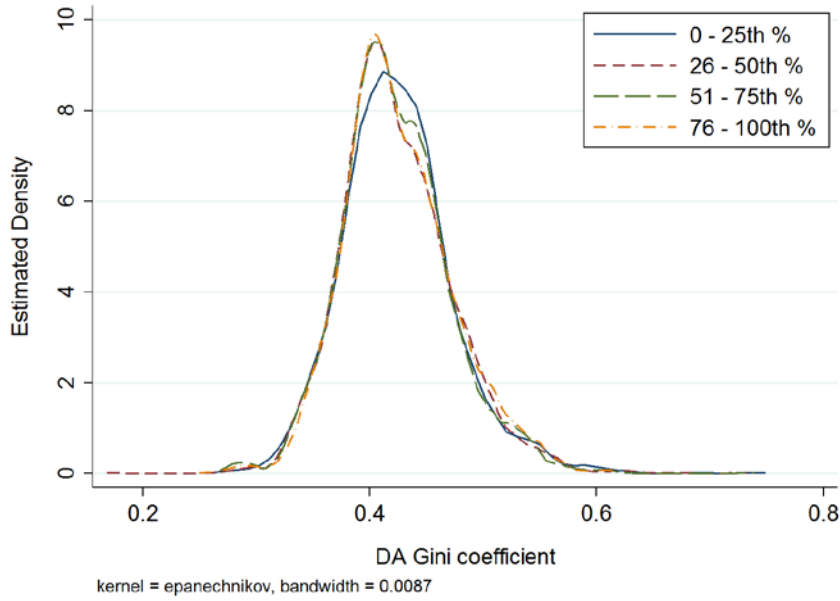


**Figure A2. Log of Lottery Prizes**



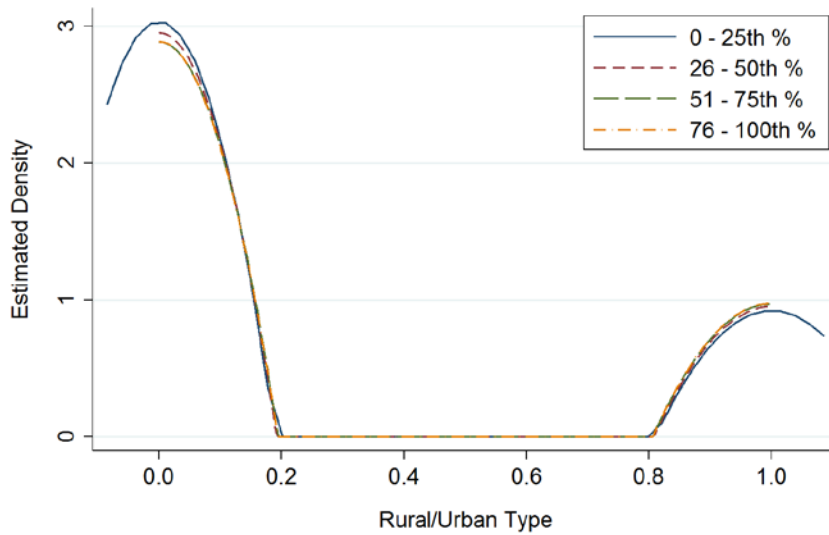


**Figure A3. No Relation Between Lottery-Winning Amounts and DA Gini Coefficients**



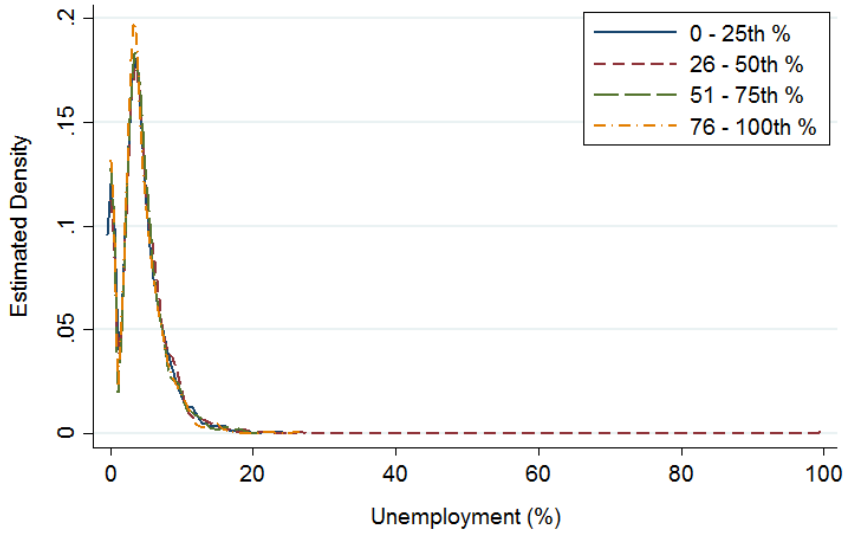
*Notes:* This figure shows DA Gini coefficient distributions for the four ranges of the lottery-winning amount. There is no relation between the Gini coefficients and lottery amounts.

**Figure A4. No Relation Between Lottery-Winning Amounts and DA Rural/Urban Type**



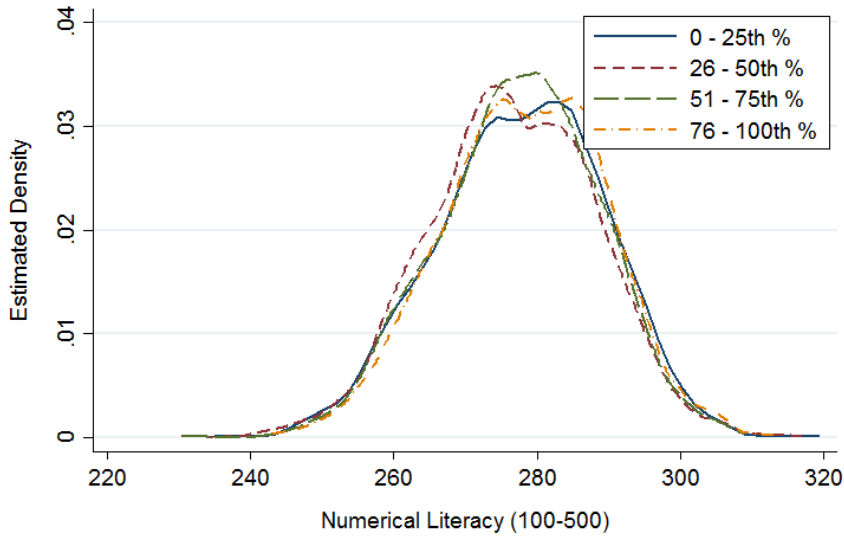
*Notes:* This figure shows DA distributions of rural (non-MSA) and urban (MSA) region types for the four ranges of the lottery-winning amount. There is no relation between the rural/urban region type and lottery amounts.

**Figure A5. No Relation Between Lottery-Winning Amounts and DA Unemployment**



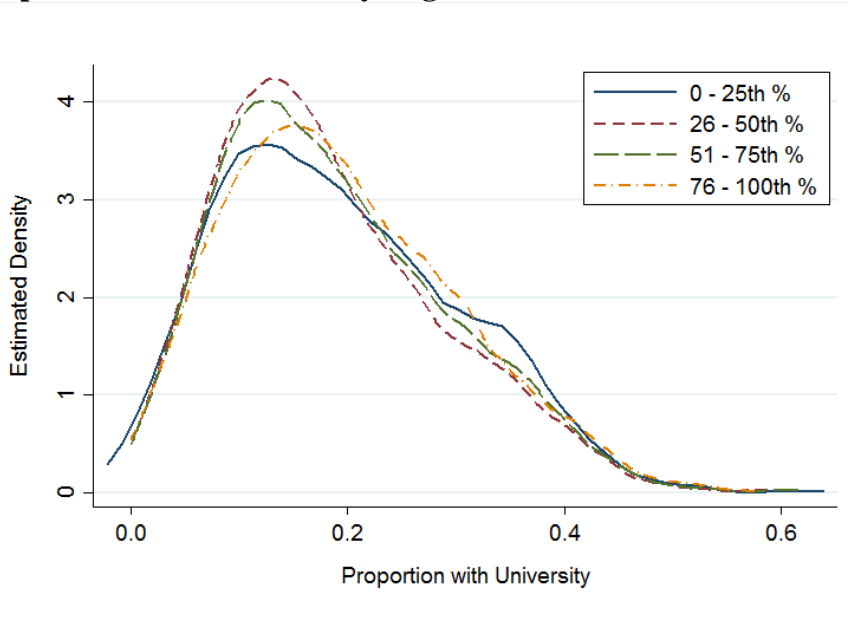
*Notes:* This figure shows DA unemployment rate distributions for the four ranges of the lottery-winning amount. There is no relation between the unemployment rate and lottery amounts.

**Figure A6. No Relation Between Lottery-Winning Amounts and DA Numerical Literacy**



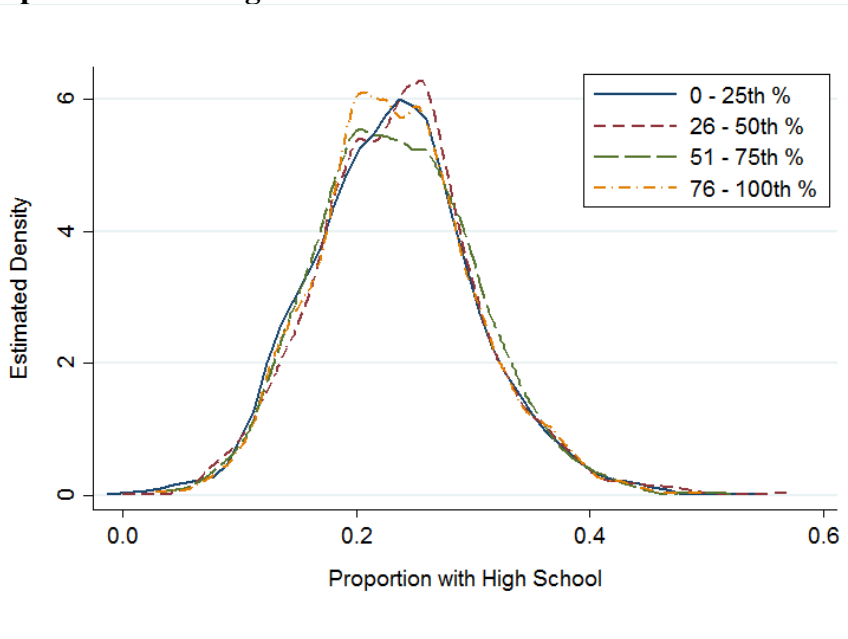
*Notes:* This figure shows DA numerical literacy distributions for the four ranges of the lottery-winning amount. There is no relation between numerical literacy and lottery amounts.

**Figure A7. No Relation Between Lottery-Winning Amounts and Proportion of DA Population with a University Degree**



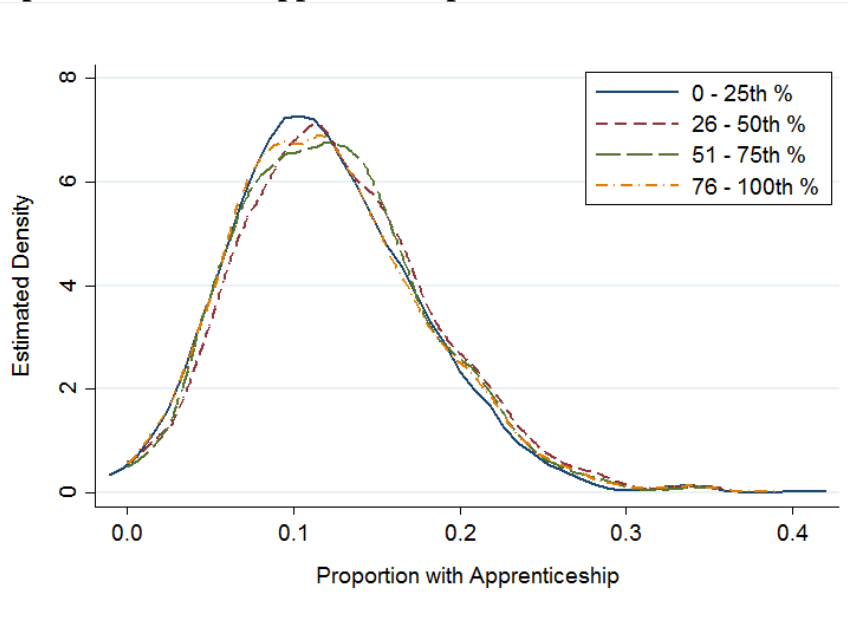
*Notes:* This figure shows distributions of DA proportions of population with a university degree for the four ranges of the lottery-winning amount. There is no relation between the proportion with a university degree and lottery amounts.

**Figure A8. No Relation Between Lottery-Winning Amounts and Proportion of DA Population with High School**



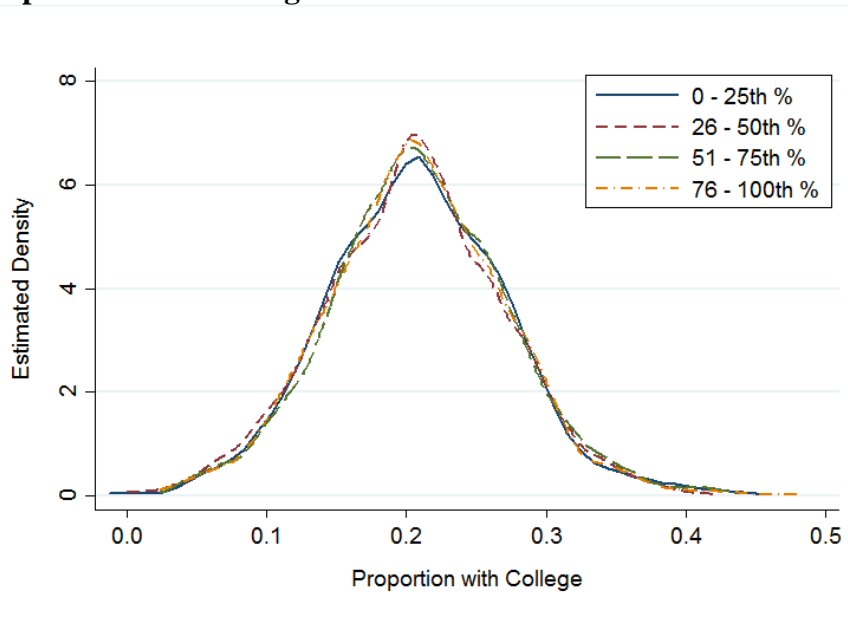
*Notes:* This figure shows distributions of DA proportions of population with high school for the four ranges of the lottery-winning amount. There is no relation between the proportion with high school and lottery amounts.

**Figure A9. No Relation Between Lottery-Winning Amounts and DA Proportion of Population with an Apprenticeship**



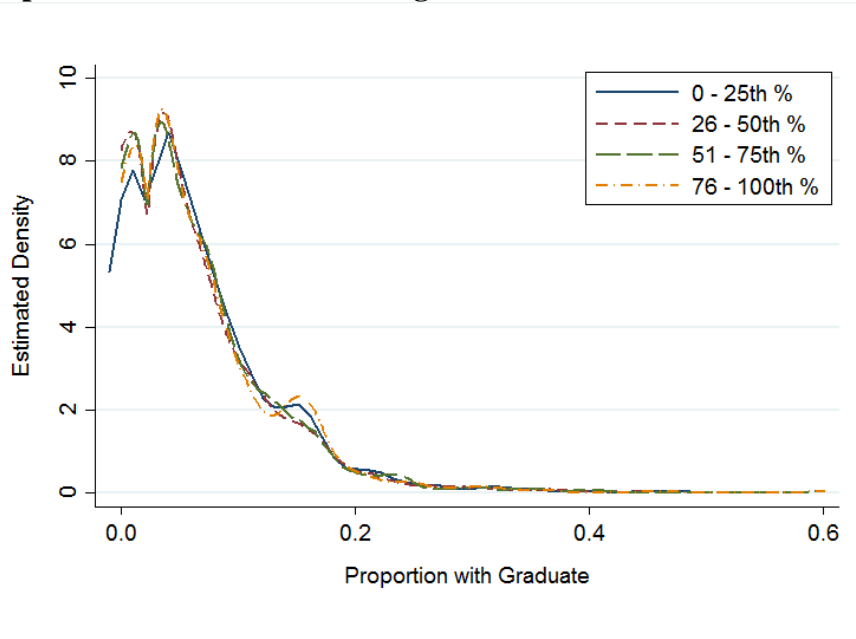
*Notes:* This figure shows distributions of DA proportions of population with an apprenticeship for the four ranges of the lottery-winning amount. There is no relation between the proportion with an apprenticeship and lottery amounts.

**Figure A10. No Relation Between Lottery-Winning Amounts and DA Proportion of Population with College**



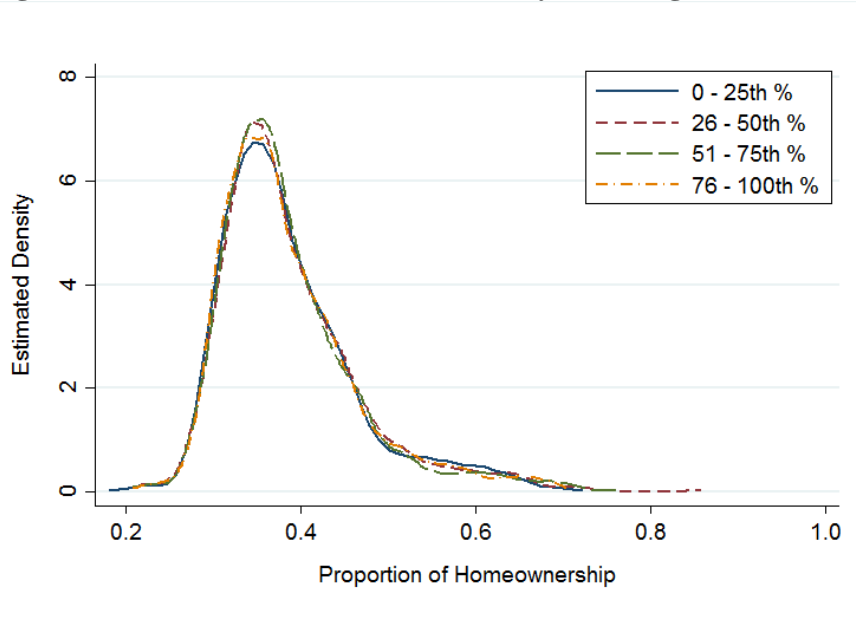
*Notes:* This figure shows distributions of DA proportions of population with college for the four ranges of the lottery-winning amount. There is no relation between the proportion with college and lottery amounts.

**Figure A11. No Relation Between Lottery-Winning Amounts and DA Proportion of Population with a Graduate Degree**



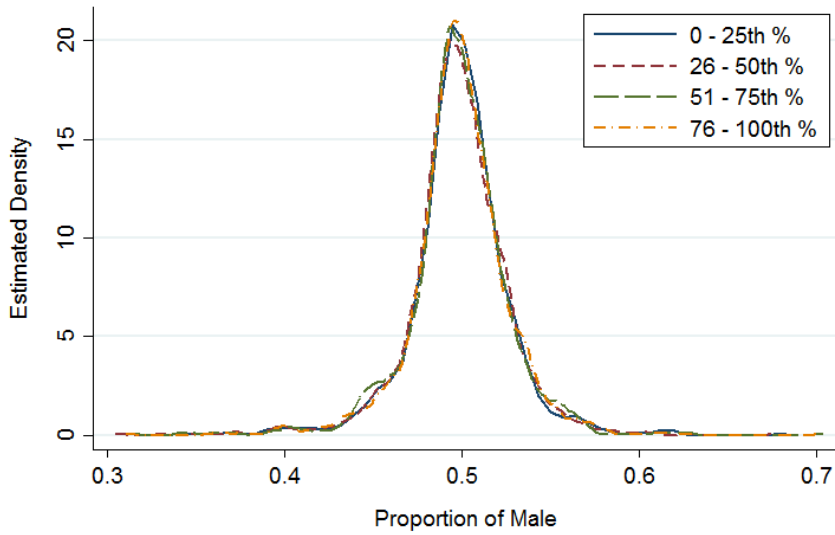
*Notes:* This figure shows distributions of DA proportions of population with a graduate degree for the four ranges of the lottery-winning amount. There is no relation between the proportion with a graduate degree and lottery amounts.

**Figure A12. No Relation Between Lottery-Winning Amounts and DA Homeownership**



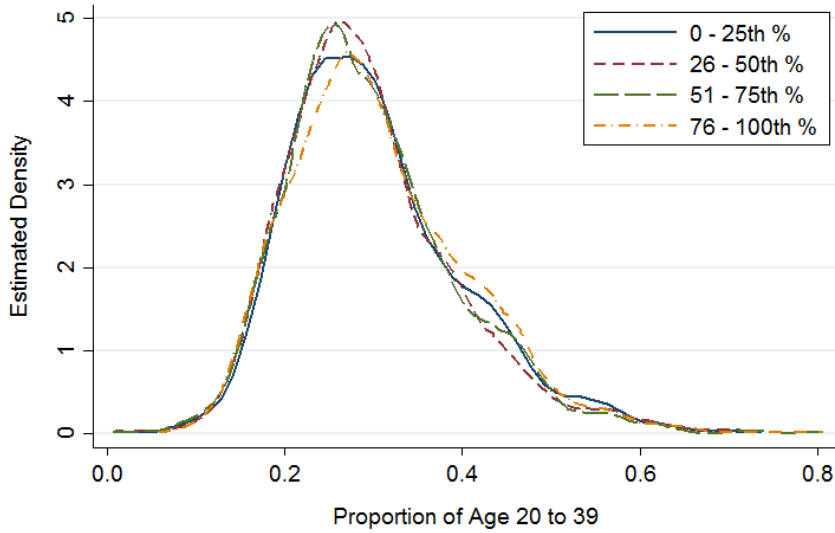
*Notes:* This figure shows distributions of the DA homeownership rate for the four ranges of the lottery-winning amount. There is no relation between the homeownership rate and lottery amounts.

**Figure A13. No Relation Between Lottery-Winning Amounts and Gender in DA**



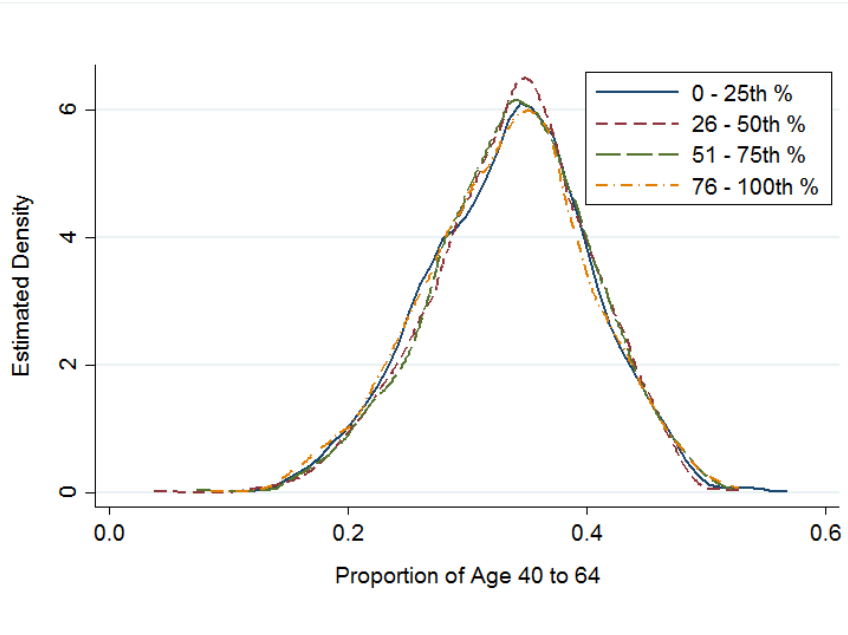
*Notes:* This figure shows distributions of DA gender composition for the four ranges of the lottery-winning amount. There is no relation between gender composition and lottery amounts.

**Figure A14. No Relation Between Lottery-Winning Amounts and DA Proportion of Young**



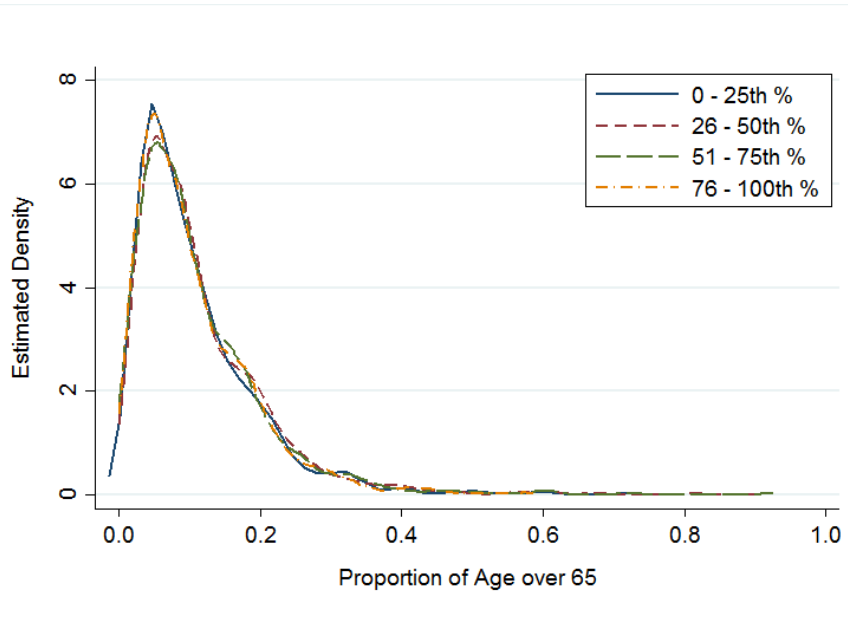
*Notes:* This figure shows distributions of the DA proportion aged 20–39 for the four ranges of the lottery-winning amount. There is no relation between the proportion aged 20–39 and lottery amounts.

**Figure A15. No Relation Between Lottery-Winning Amounts and DA Proportion Aged 40–64**



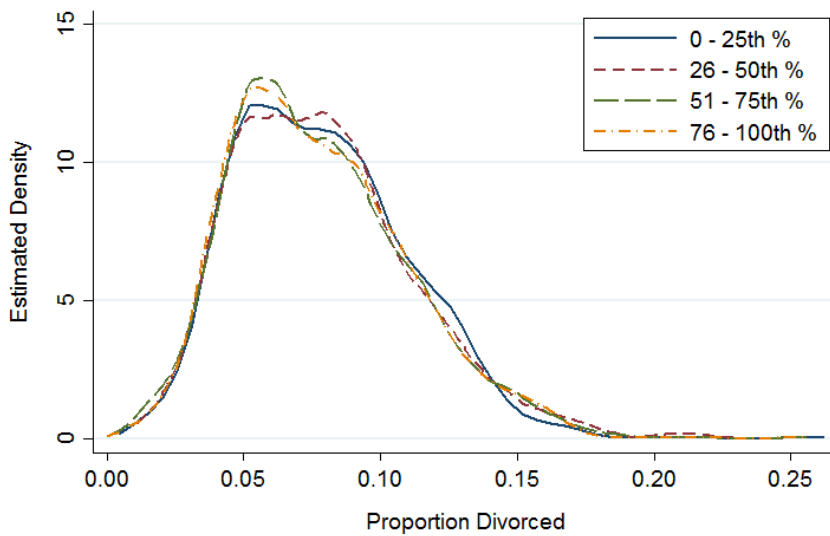
Notes: This figure shows distributions of the DA proportion aged 40–64 for the four ranges of the lottery-winning amount. There is no relation between the proportion aged 40–64 and lottery amounts.

**Figure A16. No Relation Between Lottery-Winning Amounts and DA Proportion Aged 65 and Over**



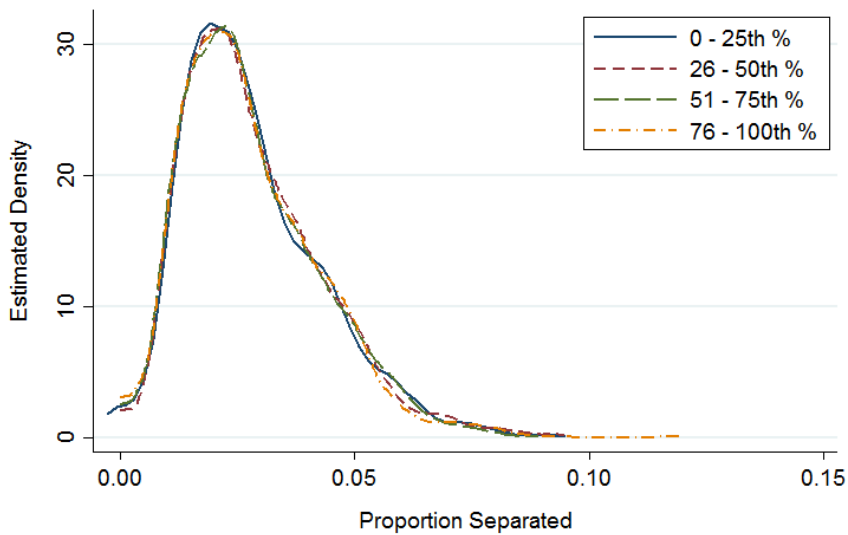
Notes: This figure shows distributions of the DA proportion aged 65 and over for the four ranges of the lottery-winning amount. There is no relation between the proportion aged 65 and over and lottery amounts.

**Figure A17. No Relation Between Lottery-Winning Amounts and DA Proportion Divorced**



*Notes:* This figure shows distributions of the DA proportion divorced for the four ranges of the lottery-winning amount. There is no relation between the proportion divorced and lottery amounts.

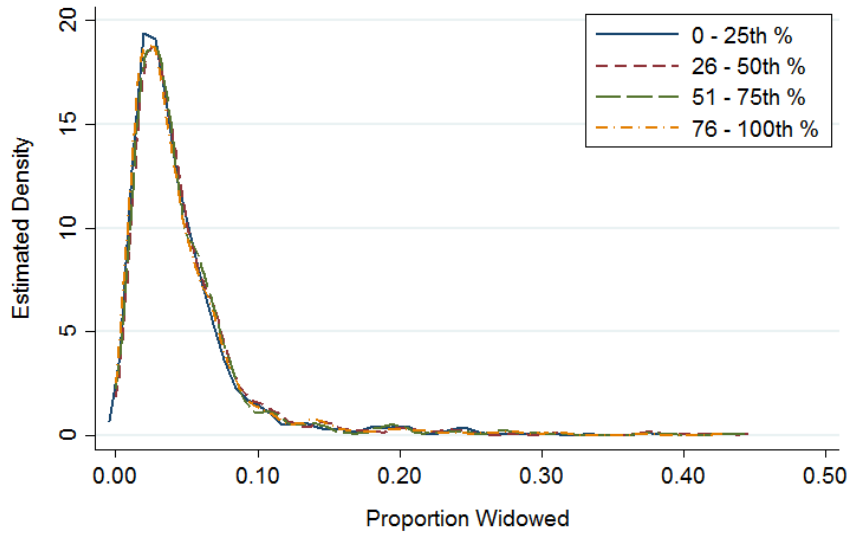
**Figure A18. No Relation Between Lottery-Winning Amounts and DA Proportion Separated**



*Notes:* This figure shows distributions of the DA proportion separated for the four ranges of the lottery-winning amount. There is no relation between the proportion separated and lottery amounts.

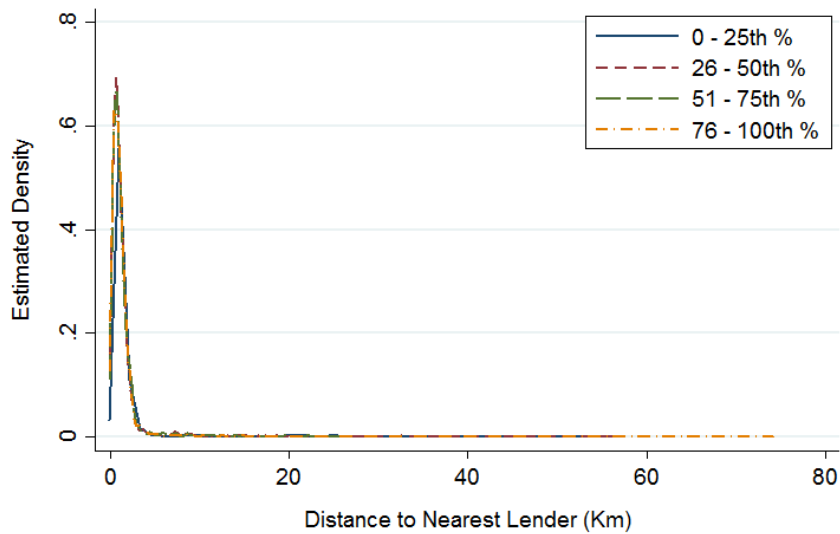


**Figure A19. No Relation Between Lottery-Winning Amounts and DA Proportion Widowed**



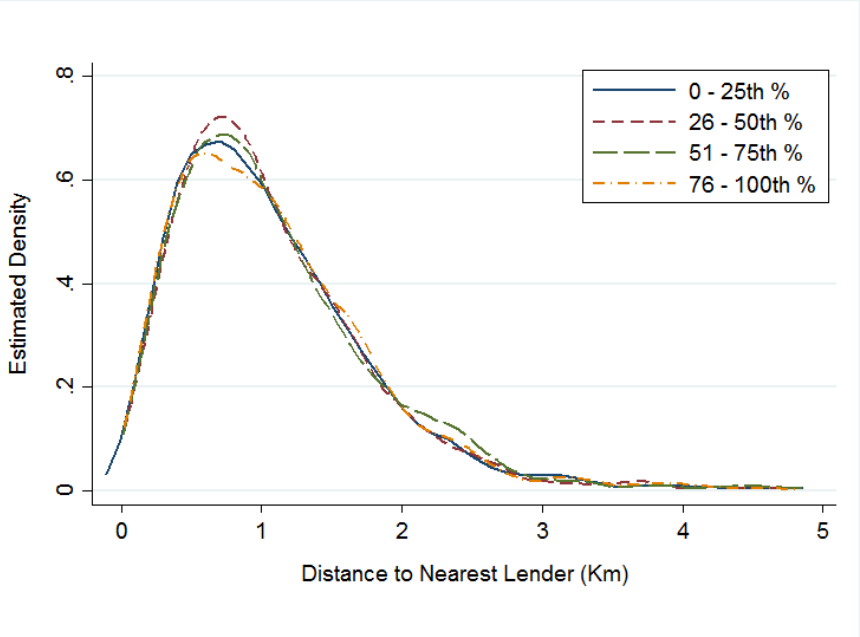
*Notes:* This figure shows distributions of the DA proportion widowed for the four ranges of the lottery-winning amount. There is no relation between the proportion of widowed and lottery amounts.

**Figure A20. No Relation Between Lottery-Winning Amounts and Distance to Closest Lender**



*Notes:* This figure shows the distributions of distance to the nearest bank for the four ranges of the lottery-winning amount. There is no relation between the distance to the nearest bank and lottery amounts.

**Figure A21. No Relation Between Lottery-Winning Amounts and Distance to Closest Lender for Postal Codes Within 5 Km of a Lender**



*Notes:* This figure shows distributions of the distance to the nearest bank for the four ranges of the lottery-winning amount. There is no relation between the distance to the nearest bank and lottery amounts.

**Table A1. The Heterogeneous Effect of Lottery Prize on the Bankruptcies of Winners' Neighbors (Split Samples)**

Event Window (years)	-3 to -5	-1 to -2	0 to 2	3 to 5
Panel A. Low-income neighborhoods				
Log of winning amount	0.0016 (0.0137)	0.0135 (0.0114)	0.0391** (0.0152)	0.0634*** (0.0197)
Number of observations	3,648	3,648	2,666	1,307
Panel B. Higher-income neighborhoods				
Log of winning amount	-0.0089 (0.0105)	0.0081 (0.0092)	0.0078 (0.0135)	0.0012 (0.0203)
Number of observations	3,729	3,729	2,686	1,279
Panel C. High-income inequality neighborhoods				
Log of winning amount	-0.0153 (0.0106)	0.0130 (0.0085)	0.0227* (0.0119)	0.0455*** (0.0166)
Number of observations	5,025	5,025	3,668	1,802
Panel D. Low-income inequality neighborhoods				
Log of winning amount	0.0160 (0.0150)	0.0033 (0.0136)	0.0182 (0.0191)	-0.0121 (0.0264)
Number of observations	2,352	2,352	1,684	784
Panel E. Urban, high-density neighborhoods				
Log of winning amount	-0.0052 (0.0095)	0.0090 (0.0082)	0.0288*** (0.0111)	0.0185 (0.0154)
Number of observations	5,567	5,567	4,054	1,929
Panel F. Not urban, low-density neighborhoods				
Log of winning amount	-0.0015 (0.0198)	0.0142 (0.0158)	-0.0048 (0.0234)	0.0706** (0.0334)
Number of observations	1,810	1,810	1,298	657

*Notes:* This table reports the marginal effect of the log of the lottery prize on the number of bankruptcies in the winners' closest neighborhood (postal code), excluding winners' own bankruptcy filings in four event windows. This effect is estimated using a Poisson model. Subsamples are defined based on the medians of census variables. Urban versus not urban definitions are based on metropolitan statistical areas. All specifications include the lottery product and winning-year fixed effects. Control variables are described in the text. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table A2. The Heterogeneous Effect of a Lottery Win on the Number of Bankrupt Neighbors (Interaction Terms)**

Event Window (years)	-1 to -5	-3 to -5	-1 to -2	0 to 2	3 to 5	0 to 5
Panel A. The effect of DA average age on bankruptcies after lottery wins						
Log of winning amount	-0.0027 (0.0019)	-0.0009 (0.0015)	-0.0018 (0.0012)	-0.0019 (0.0018)	-0.0056** (0.0026)	-0.0096*** (0.0035)
Panel B. The effect of DA average marriage rate on bankruptcies after lottery wins						
Log of winning amount	0.0007 (0.0008)	0.0005 (0.0006)	0.0001 (0.0005)	0.0009 (0.0007)	-0.0002 (0.0010)	0.0004 (0.0013)
Panel C. The effect of DA income inequality on bankruptcies after lottery wins						
Log of winning amount	-0.1961 (0.2254)	-0.0724 (0.1731)	-0.1326 (0.1448)	0.1075 (0.2054)	0.2183 (0.2782)	-0.0970 (0.3787)
Panel D. The effect of DA prevalence of low income on bankruptcies after lottery wins						
Log of winning amount	0.0003 (0.0012)	0.0004 (0.0009)	0.0001 (0.0008)	-0.0003 (0.0012)	0.0014 (0.0015)	0.0020 (0.0020)
Panel E. The effect of DA share of immigrants on bankruptcies after lottery wins						
	0.4167 (0.3410)	0.3013 (0.2354)	0.0663 (0.2592)	-0.5527 (0.5167)	0.0211 (0.6763)	-0.6072 (0.9172)
Panel F. The effect of DA population density on bankruptcies after lottery wins						
	0.0037 (0.0038)	0.0048* (0.0026)	-0.0018 (0.0028)	-0.0060 (0.0038)	-0.0027 (0.0045)	-0.0113* (0.0064)
Number of observations	7,377	7,377	7,377	5,352	2,586	2,586

*Notes:* This table reports the marginal effect of the log of the lottery prize interacted with a DA observable (as indicated in each panel's title) on the number of bankruptcies among the winner's closest neighbors (postal code). This effect is estimated using a Poisson model. Panel A shows that the effect of neighbor's lottery win on other neighbors' bankruptcy is declining in age of the neighborhood. This finding may suggest that young people may be more susceptible to peer effects in relative income. All other panels show no significant difference in the effect of lottery wins on bankruptcies depending on neighborhood's income inequality, prevalence of low income, share of immigrants within 1 year, and population density. Population density is measured in 1000s of persons per square km. All specifications include lottery product and winning-year fixed effects. Control variables are described in the text. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table A3. Summary Statistics for Individual Lottery Winners and Winners' Own Bankruptcy**

<b>Variable</b>	<b>Obs.</b>	<b>Mean</b>	<b>Std. Dev.</b>
Log of winning amount	18,012	7.879	1.015
Winning year	18,012	2,009	3
Own bankruptcy rate relative to the lottery date			
Years			
-3 to -5	18,012	0.005	0.069
-1 to -2	17,451	0.005	0.071
0 to 2	10,749	0.006	0.078
3 to 5	4,412	0.005	0.067
Neighborhood characteristics:			
DA Gini coefficient	18,012	0.424	0.048
Median income (\$)	18,012	30532	8079
Population density (persons per sq. km)	18,012	2342	3005
Region type (1 to 8 score)	18,012	2.116	1.788
Unemployment rate (%)	18,012	4.177	3.797
Numerical literacy score (between 100 and 500)	18,012	276	11
Divorced (proportion of DA population)	18,012	0.078	0.032
Separated (proportion of DA population)	18,012	0.028	0.015
Widowed (proportion of DA population)	18,012	0.05	0.049
High school (proportion of DA population)	18,012	0.238	0.066
Apprenticeship (proportion of DA population)	18,012	0.13	0.06
College (DA) (proportion of DA population)	18,012	0.203	0.065
University (DA) (proportion of DA population)	18,012	0.169	0.101
Graduate (DA) (proportion of DA population)	18,012	0.053	0.056
Homeownership (proportion of DA population)	18,012	0.387	0.082
Male (proportion of DA population)	18,012	0.5	0.03
Age between 20 and 39 years (proportion of DA population)	18,012	0.295	0.098
Age between 40 and 64 years (proportion of DA population)	18,012	0.333	0.068
Age over 65 years (proportion of DA population)	18,012	0.113	0.094

**Table A4. The Effect of Lottery Winning on Winners' Own Bankruptcy**

<b>Event Window (Years)</b>	<b>-3 to -5</b>	<b>-1 to -2</b>	<b>0 to 2</b>	<b>3 to 5</b>
Panel A. Single-winning postal codes, controls included				
Log of winning amount	-0.0020 (0.0014)	0.0008 (0.0011)	-0.0029 (0.0031)	0.0036 (0.0025)
Number of observations	6,661	6,323	3,842	949
Panel B. All winners, controls included				
Log of winning amount	-0.0003 (0.0005)	-0.0009 (0.0007)	-0.0011 (0.0011)	0.0017 (0.0011)
Number of observations	18,012	17,078	10,579	3,699
Panel C. All winners, controls excluded				
Log of winning amount	-0.0003 (0.0005)	-0.0009 (0.0007)	-0.0011 (0.0011)	0.0016* (0.0010)
Number of observations	18,012	17,447	10,658	4,019

*Notes:* This table reports the effect of the lottery prize size on winners' probability to file for bankruptcy. This effect is estimated using a logit model with a binary filing indicator as the dependent variable. The sample consists of all individual winners in single winning postal codes (panel A), and all postal codes (panels B and C) with random prize lotteries and prizes between C\$1,000 and C\$150,000 won between 2004 and 2014. All specifications include lottery product and winning-year fixed effects. When included, the control variables consist of DA-level Gini coefficient, median income, population density, region's influence on urban core, DA numerical literacy, unemployment rate, family breakdowns, homeownership, age and gender distributions, and education levels. We consider individual winners with randomly sized prizes of more than C\$1,000 and less than C\$150,000. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table A5. The Effect of Lottery Prize on the Count of Neighborhood Bankruptcies (Single-Year Event Windows)**

Years Relative to Winning	(1)	(2)
	DA – Postal Code Bankruptcies	se
-5	-0.0038	(0.0042)
-4	-0.0047	(0.0044)
-3	-0.0043	(0.0046)
-2	-0.0018	(0.0044)
-1	-0.0057	(0.0048)
0	-0.0021	(0.0046)
1	-0.0033	(0.0052)
2	-0.0033	(0.0055)
3	0.0015	(0.0058)
4	-0.0003	(0.0062)
5	0.0020	(0.0072)

*Notes:* This table reports the marginal effect of the log of the lottery prize on the count of bankruptcy in the winners' outer neighborhood (DA, excluding winner's postal code). This count excludes winners' own bankruptcy filings. An OLS model is used because of noninteger values of bankruptcies per postal code in these data. All specifications include lottery product and winning-year fixed effects. Control variables are described in the text. Standard errors (se) are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table A6. The Effect of Lottery Prize on the Balance Sheets of Neighboring Bankruptcy Filers (Single-Year Event Windows, After Winning)**

Event Window (Years)	1	2	3	4	5
Cars	0.2114 -0.1639	0.2384 -0.1704	0.0417 -0.1875	-0.247 -0.195	0.1805 -0.2728
Houses	0.0687 -0.2337	0.5508** -0.2464	0.0265 -0.269	-0.2953 -0.2865	0.1727 -0.4012
Motorcycles	-0.0092 -0.0544	0.1577** -0.064	0.0041 -0.0639	0.0031 -0.0419	0.0712 -0.0919
Recreational equipment	-0.0435 -0.0836	0.0676 -0.093	-0.0948 -0.1137	-0.0846 -0.1028	0.0781 -0.1676
Furniture	0.0074 -0.0994	-0.0164 -0.0908	0.1414 -0.1172	-0.0305 -0.118	0.2731 -0.1758
Cash	-0.0151 -0.0522	0.0157 -0.0454	0.0248 -0.0592	-0.0566 -0.0604	0.0709 -0.0723
Securities	0.0271 (0.0602)	0.1731** (0.0732)	0.0274 (0.0804)	0.1820** (0.0860)	0.1481 (0.1201)
Insurance and pensions	-0.0851 (0.1529)	-0.1840 (0.1668)	-0.0275 (0.1835)	-0.2878 (0.1915)	-0.2402 (0.2907)
Number of observations	878	700	563	436	260

*Notes:* This table reports the effect of the log of the lottery prize on the log of asset value. All coefficients are from separate OLS regressions with log of assets value + 1 as the dependent variable. All specifications include lottery product and winning-year fixed effects. Control variables are described in Table 6 and the text. These coefficients may imply that the value of conspicuous consumption assets in bankruptcy increases in lottery size for filers after the neighbor's lottery winning. Lottery size has no effect on the ownership of invisible consumption assets. Lottery size also increases the value of risky financial assets (securities) but not less risky assets (insurance and pensions). Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.



**Table A7. The Effect of Lottery Prize on the Balance Sheets of Neighboring Bankruptcy Filers (Single-Year Event Windows, Before Winning)**

Event Window (Years)	-5	-4	-3	-2	-1	0
Cars	-0.0760 (0.2937)	-0.0512 (0.2179)	-0.0683 (0.2166)	0.0783 (0.1588)	0.2737 (0.1736)	0.1810 (0.1528)
Houses	0.0374 (0.2950)	0.3361 (0.2368)	0.1665 (0.2733)	0.0597 (0.2018)	0.3229 (0.2342)	0.1615 (0.2117)
Motorcycles	-0.0834 (0.0686)	-0.0312 (0.0623)	-0.0404 (0.0699)	-0.0104 (0.0439)	-0.0210 (0.0649)	0.0376 (0.0414)
Recreational equipment	-0.1472 (0.1063)	0.0309 (0.1055)	0.0088 (0.1159)	0.0456 (0.0686)	0.1129 (0.0839)	0.0665 (0.0708)
Furniture	0.1029 (0.1663)	-0.0089 (0.1380)	-0.1320 (0.1453)	0.0768 (0.0882)	0.1307 (0.1049)	-0.0574 (0.0912)
Cash	0.1264 (0.1026)	0.0416 (0.0569)	-0.0363 (0.0637)	-0.0089 (0.0454)	0.0670 (0.0503)	-0.0030 (0.0437)
Securities	0.0248 (0.1467)	-0.0065 (0.1175)	-0.0992 (0.1103)	-0.0709 (0.0650)	-0.0844 (0.0712)	0.0601 (0.0652)
Insurance and pensions	-0.3804* (0.2231)	-0.1217 (0.1896)	0.5579*** (0.1867)	-0.0110 (0.1358)	0.1628 (0.1599)	-0.1231 (0.1402)
Number of observations	329	502	646	878	863	1,039

*Notes:* This table reports the effect of the log of the lottery prize on the log of asset value. All coefficients are from separate OLS regressions with log of assets value + 1 as the dependent variable. All specifications include lottery product and winning-year fixed effects. Control variables are described in Table 6 and the text. These coefficients may imply that the asset values of bankrupts are not related to lottery size before the neighbor's lottery winning. Standard errors are in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% levels, respectively.

**Table A8. Lottery Prize Effect on Credit Outcomes Within Winners' Postal Codes with a Lender Within 1 Km**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.007 (0.017)	0.015 (0.020)	0.011 (0.009)	0.030 (0.019)	0.006 (0.017)
year -4	0.001 (0.007)	-0.038 (0.036)	0.005 (0.012)	-0.002 (0.015)	0.009 (0.006)	-0.009 (0.014)	0.014 (0.013)
year -3	-0.001 (0.004)	-0.001 (0.024)	-0.014* (0.008)	-0.009 (0.011)	0.001 (0.004)	-0.005 (0.011)	-0.011 (0.010)
year -2	-0.002 (0.002)	-0.006 (0.015)	-0.006 (0.005)	-0.002 (0.008)	0.002 (0.002)	0.004 (0.008)	-0.001 (0.006)
year 0	0.002 (0.002)	0.002 (0.011)	0.014*** (0.004)	0.001 (0.006)	0.004** (0.002)	0.004 (0.006)	0.004 (0.005)
year 1	0.005** (0.002)	0.027* (0.014)	0.022*** (0.006)	0.007 (0.008)	0.008*** (0.003)	0.014* (0.008)	0.015** (0.007)
year 2	0.008*** (0.003)	0.039** (0.017)	0.036*** (0.008)	0.019** (0.009)	0.012*** (0.004)	0.025*** (0.009)	0.021*** (0.008)
year 3	0.011*** (0.003)	0.042** (0.018)	0.043*** (0.010)	0.016 (0.010)	0.013*** (0.005)	0.016 (0.010)	0.014 (0.009)
year 4	0.014*** (0.003)	0.044** (0.021)	0.039*** (0.012)	0.015 (0.012)	0.009 (0.006)	0.015 (0.012)	0.017 (0.010)
year 5	0.018*** (0.004)	0.054** (0.024)	0.040*** (0.015)	0.020 (0.013)	0.007 (0.007)	0.017 (0.014)	0.019 (0.012)
Observations	750,371	750,371	966,073	966,073	966,073	966,073	966,073
# of individuals	153,018	153,018	172,044	172,044	172,044	172,044	172,044
R-squared within	0.123	0.0857	0.0953	0.000869	0.114	0.00142	0.00372
R-squared overall	0.0157	0.0155	0.00387	4.45e-05	0.00527	0.000391	2.65e-06

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All dollar denominated variables are transformed by taking logs and adding 1 to the original dollar value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. These coefficients imply that when a lender is 1 km away from the winning postal code, credit increases in the postal code. This finding may suggest that 1 km is far enough for lenders to not being able to collect soft information on very local shocks such as lottery prizes. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table A9. The Lottery Prize Effect on Credit Outcomes Within Winners' Postal Codes Without a Lender Within 1 Km**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.019 (0.019)	-0.053** (0.021)	-0.014 (0.010)	-0.030 (0.020)	-0.002 (0.018)
year -4	-0.004 (0.007)	-0.021 (0.042)	0.024* (0.013)	-0.027* (0.015)	0.000 (0.006)	-0.014 (0.015)	0.009 (0.013)
year -3	0.003 (0.004)	0.058** (0.027)	0.020** (0.009)	-0.006 (0.012)	-0.001 (0.005)	-0.011 (0.011)	-0.009 (0.010)
year -2	0.006** (0.003)	0.046*** (0.017)	0.009 (0.005)	0.017* (0.009)	-0.001 (0.003)	0.009 (0.009)	-0.008 (0.007)
year 0	0.002 (0.002)	0.010 (0.013)	0.010** (0.004)	-0.002 (0.007)	0.005** (0.002)	0.005 (0.007)	0.011** (0.005)
year 1	-0.000 (0.002)	-0.010 (0.016)	0.015** (0.006)	-0.017** (0.009)	0.007** (0.003)	0.008 (0.008)	0.009 (0.007)
year 2	-0.000 (0.003)	-0.007 (0.019)	0.016* (0.008)	-0.015 (0.010)	0.005 (0.004)	-0.007 (0.010)	0.002 (0.008)
year 3	0.002 (0.003)	0.012 (0.021)	0.024** (0.010)	-0.010 (0.011)	0.008* (0.005)	-0.008 (0.011)	-0.007 (0.009)
year 4	0.005 (0.004)	0.053** (0.024)	0.028** (0.013)	-0.015 (0.013)	0.011* (0.006)	-0.012 (0.013)	-0.012 (0.011)
year 5	0.009** (0.004)	0.075*** (0.027)	0.044*** (0.016)	-0.011 (0.014)	0.017** (0.007)	-0.022 (0.015)	-0.001 (0.013)
Observations	736,724	736,724	940,051	940,051	940,051	940,051	940,051
# of individuals	144,378	144,378	160,603	160,603	160,603	160,603	160,603
R-squared within	0.147	0.104	0.125	0.00237	0.136	0.00394	0.00690
R-squared overall	0.0127	0.0174	0.00268	0.000758	0.00570	0.00114	0.00118

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. These coefficients imply that credit in postal codes without a lender within 1 km increases after large lottery wins in these postal codes. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table A10. Lottery Prize Effect on Credit Outcomes of Prime Borrowers (Risk Score Based on One Year Prior to Lottery Win)**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.024* (0.013)	-0.019 (0.016)	-0.002 (0.007)	0.007 (0.014)	0.011 (0.012)
year -4	-0.007 (0.005)	-0.057* (0.030)	0.010 (0.009)	-0.010 (0.012)	0.003 (0.005)	-0.011 (0.011)	0.010 (0.009)
year -3	-0.001 (0.003)	0.017 (0.019)	-0.000 (0.006)	-0.007 (0.009)	-0.001 (0.003)	-0.006 (0.008)	-0.007 (0.007)
year -2	0.000 (0.002)	0.011 (0.012)	-0.004 (0.004)	0.009 (0.006)	-0.001 (0.002)	0.008 (0.006)	-0.006 (0.004)
year 0	0.003** (0.001)	0.011 (0.009)	0.013*** (0.003)	0.002 (0.005)	0.004** (0.002)	0.005 (0.005)	0.004 (0.004)
year 1	0.004** (0.002)	0.011 (0.012)	0.022*** (0.005)	0.002 (0.006)	0.008*** (0.002)	0.011* (0.006)	0.010** (0.005)
year 2	0.005** (0.002)	0.019 (0.014)	0.031*** (0.006)	0.011 (0.007)	0.010*** (0.003)	0.013* (0.007)	0.011* (0.006)
year 3	0.009*** (0.003)	0.033** (0.016)	0.038*** (0.008)	0.012 (0.008)	0.013*** (0.004)	0.008 (0.008)	0.007 (0.006)
year 4	0.011*** (0.003)	0.055*** (0.018)	0.036*** (0.010)	0.014 (0.009)	0.012*** (0.004)	0.008 (0.009)	0.009 (0.008)
year 5	0.014*** (0.003)	0.060*** (0.019)	0.046*** (0.012)	0.023** (0.010)	0.015*** (0.005)	0.009 (0.010)	0.014 (0.009)
Observations	1,242,087	1,242,087	1,598,704	1,598,704	1,598,704	1,598,704	1,598,704
# of individuals	241,337	241,337	264,644	264,644	264,644	264,644	264,644
R-squared within	0.137	0.0955	0.114	0.00199	0.143	0.00458	0.00952
R-squared overall	0.0179	0.0192	0.00573	0.00544	0.00789	0.00181	0.00205

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. These results are qualitatively similar to the results in Table 16 (prime borrowers in all years prior to the lottery win). Prime borrowers in this sample are defined as those with risk scores above 660 in the year prior to the lottery win. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table A11. Lottery Prize Effect on Credit Outcomes of Subprime Borrowers (Risk Score Based on One Year Prior to Lottery Win)**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.007 (0.044)	-0.024 (0.038)	-0.009 (0.020)	-0.074* (0.043)	-0.081 (0.051)
year -4	0.017 (0.011)	0.048 (0.070)	-0.005 (0.028)	-0.038 (0.028)	-0.001 (0.013)	-0.028 (0.030)	-0.002 (0.037)
year -3	0.008 (0.007)	0.029 (0.045)	-0.003 (0.019)	-0.008 (0.021)	0.004 (0.009)	-0.023 (0.023)	-0.049* (0.027)
year -2	0.004 (0.004)	0.024 (0.025)	0.011 (0.011)	-0.005 (0.014)	0.001 (0.005)	-0.007 (0.015)	-0.018 (0.017)
year 0	-0.003 (0.002)	-0.003 (0.016)	0.015** (0.007)	-0.009 (0.011)	0.007* (0.003)	0.008 (0.011)	0.025** (0.013)
year 1	0.001 (0.003)	0.029 (0.023)	0.007 (0.011)	-0.023* (0.014)	0.003 (0.005)	0.023 (0.014)	0.023 (0.016)
year 2	0.002 (0.004)	0.030 (0.028)	0.012 (0.015)	-0.025 (0.017)	0.006 (0.007)	0.008 (0.018)	0.021 (0.019)
year 3	0.004 (0.005)	0.031 (0.031)	0.023 (0.020)	-0.030 (0.020)	0.005 (0.009)	-0.001 (0.022)	-0.010 (0.022)
year 4	0.006 (0.006)	0.042 (0.038)	0.036 (0.027)	-0.053** (0.025)	0.003 (0.012)	-0.011 (0.027)	-0.023 (0.027)
year 5	0.018** (0.007)	0.119*** (0.046)	0.023 (0.034)	-0.091*** (0.031)	-0.005 (0.015)	-0.048 (0.035)	-0.009 (0.033)
Observations	245,008	245,008	307,420	307,420	307,420	307,420	307,420
# of individuals	54,728	54,728	66,088	66,088	66,088	66,088	66,088
R-squared within	0.129	0.0938	0.0988	0.00650	0.0492	0.00692	0.0118
R-squared overall	0.0179	0.0152	0.00149	0.000142	0.000985	0.000274	0.00176

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. These results are qualitatively similar to the results in Table 17 (subprime borrowers in any year prior to the lottery win). In this sample, subprime borrowers are defined as those with risk scores less than or equal 660 in the year prior to the lottery win. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table A12. Lottery Prize Effect on Credit Outcomes of Young Prime Borrowers**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			0.001 (0.023)	0.022 (0.026)	-0.001 (0.012)	0.041* (0.025)	0.027 (0.019)
year -4	-0.002 (0.010)	0.010 (0.059)	-0.002 (0.016)	0.021 (0.020)	-0.005 (0.008)	-0.013 (0.019)	0.007 (0.015)
year -3	0.002 (0.006)	0.047 (0.037)	-0.005 (0.011)	0.002 (0.015)	-0.004 (0.006)	-0.014 (0.014)	-0.009 (0.011)
year -2	-0.003 (0.004)	0.004 (0.024)	-0.009 (0.006)	0.020* (0.011)	-0.000 (0.003)	0.005 (0.011)	0.002 (0.007)
year 0	0.003 (0.003)	0.012 (0.018)	0.009 (0.005)	0.007 (0.009)	0.002 (0.003)	0.014 (0.009)	0.006 (0.006)
year 1	0.002 (0.003)	0.002 (0.023)	0.017** (0.007)	0.007 (0.011)	0.006* (0.003)	0.018* (0.010)	0.008 (0.007)
year 2	0.002 (0.004)	0.012 (0.027)	0.027*** (0.010)	0.022* (0.012)	0.008* (0.005)	0.029** (0.012)	0.008 (0.009)
year 3	0.006 (0.004)	0.029 (0.029)	0.034*** (0.012)	0.016 (0.013)	0.012** (0.006)	0.013 (0.013)	0.008 (0.010)
year 4	0.011** (0.005)	0.061* (0.032)	0.027* (0.015)	0.015 (0.015)	0.010 (0.007)	0.007 (0.015)	0.000 (0.012)
year 5	0.017*** (0.006)	0.074** (0.036)	0.040** (0.018)	0.015 (0.016)	0.011 (0.008)	0.010 (0.017)	0.006 (0.014)
Observations	482,085	482,085	613,176	613,176	613,176	613,176	613,176
# of individuals	97,287	97,287	107,073	107,073	107,073	107,073	107,073
R-squared within	0.205	0.146	0.250	0.0114	0.193	0.0130	0.0143
R-squared overall	0.0340	0.0303	0.00797	0.00368	0.00914	0.00349	0.00163

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. This table shows that mortgage and all credit increase for a group of young prime borrowers (younger than the median age, which is 46 years in our sample). This finding suggests that the increases in credit to prime consumers are not driven by age only. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table A13. Lottery Prize Effect on Credit Outcomes of Young Subprime Borrowers**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			0.003 (0.040)	-0.023 (0.040)	0.003 (0.018)	-0.051 (0.043)	-0.046 (0.048)
year -4	0.020** (0.010)	0.073 (0.063)	0.011 (0.025)	-0.022 (0.028)	0.001 (0.011)	-0.017 (0.030)	0.009 (0.033)
year -3	0.013** (0.006)	0.058 (0.041)	-0.007 (0.016)	-0.026 (0.021)	0.000 (0.008)	-0.024 (0.022)	-0.040* (0.024)
year -2	0.005* (0.003)	0.028 (0.023)	0.001 (0.009)	0.000 (0.013)	-0.004 (0.004)	-0.014 (0.014)	-0.022 (0.015)
year 0	0.000 (0.002)	-0.007 (0.016)	0.009 (0.006)	-0.014 (0.010)	0.005* (0.003)	0.005 (0.011)	0.005 (0.011)
year 1	0.001 (0.003)	-0.000 (0.021)	0.006 (0.010)	-0.025** (0.013)	0.004 (0.004)	0.007 (0.014)	0.009 (0.014)
year 2	0.002 (0.004)	0.001 (0.026)	0.013 (0.013)	-0.008 (0.015)	0.005 (0.006)	0.008 (0.017)	0.018 (0.017)
year 3	0.003 (0.004)	-0.015 (0.029)	0.028 (0.017)	-0.010 (0.017)	0.003 (0.008)	0.020 (0.020)	-0.005 (0.019)
year 4	0.003 (0.005)	-0.003 (0.037)	0.034 (0.023)	-0.034 (0.022)	0.001 (0.010)	0.003 (0.025)	-0.003 (0.023)
year 5	0.012* (0.007)	0.055 (0.045)	0.024 (0.030)	-0.025 (0.028)	-0.001 (0.014)	0.000 (0.032)	0.012 (0.030)
Observations	307,274	307,274	363,763	363,763	363,763	363,763	363,763
# of individuals	68,087	68,087	74,521	74,521	74,521	74,521	74,521
R-squared within	0.153	0.113	0.231	0.0107	0.132	0.00629	0.0089
R-squared overall	0.0274	0.0275	0.00419	0.00100	0.00625	0.000687	0.000385

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. This table shows that credit does not increase for a group of young subprime borrowers (younger than the median age, which is 46 years in our sample). This finding suggests that the increases in credit to prime consumers are not driven by age only. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.

**Table A14. Lottery Prize Effect on Credit Outcomes of Older Prime Borrowers**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.025 (0.016)	-0.034 (0.021)	0.001 (0.008)	-0.007 (0.018)	0.001 (0.015)
year -4	-0.009 (0.007)	-0.078** (0.035)	0.011 (0.011)	-0.035** (0.015)	0.008 (0.006)	-0.019 (0.013)	-0.001 (0.011)
year -3	-0.008* (0.004)	-0.018 (0.024)	0.006 (0.008)	-0.006 (0.012)	0.003 (0.004)	-0.000 (0.011)	-0.006 (0.009)
year -2	0.001 (0.003)	0.014 (0.015)	0.005 (0.005)	0.001 (0.009)	0.002 (0.003)	0.012 (0.008)	-0.006 (0.006)
year 0	0.003 (0.002)	0.007 (0.011)	0.010** (0.004)	-0.000 (0.007)	0.004** (0.002)	-0.003 (0.007)	0.004 (0.005)
year 1	0.005* (0.002)	0.013 (0.015)	0.018*** (0.006)	-0.007 (0.009)	0.009*** (0.003)	0.003 (0.008)	0.011* (0.006)
year 2	0.007** (0.003)	0.022 (0.017)	0.021*** (0.008)	-0.001 (0.010)	0.010** (0.004)	0.001 (0.009)	0.011 (0.008)
year 3	0.010*** (0.003)	0.032* (0.019)	0.023** (0.010)	0.003 (0.011)	0.012** (0.005)	-0.003 (0.010)	0.003 (0.009)
year 4	0.010*** (0.004)	0.043** (0.021)	0.024* (0.013)	0.014 (0.013)	0.014** (0.006)	0.003 (0.012)	0.008 (0.010)
year 5	0.008* (0.004)	0.020 (0.022)	0.017 (0.015)	0.024* (0.014)	0.015** (0.008)	0.002 (0.013)	0.011 (0.012)
Observations	582,425	582,425	782,975	782,975	782,975	782,975	782,975
# of individuals	109,258	109,258	122,776	122,776	122,776	122,776	122,776
R-squared within	0.0795	0.0531	0.0509	0.000430	0.104	0.000893	0.00354
R-squared overall	0.00142	0.00273	0.00307	0.000255	0.00219	0.00114	5.56e-06

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. This table shows that mortgage, bankcard, and all credit increase for a group of older prime borrowers (older than the median age, which is 46 years in our sample). This finding suggests that the increases in credit to prime consumers are not driven by age only. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.



**Table A15. Lottery Prize Effect on Credit Outcomes of Older Subprime Borrowers**

<b>Years relative to winning</b>	(1) Number of mortgage accounts	(2) Total mortgage balance	(3) Number of credit accounts excluding mortgages	(4) Total balance of all accounts excluding mortgages	(5) Number of bankcards	(6) Total bankcard balance	(7) Total bankcard limit
year -5			-0.012 (0.051)	-0.045 (0.049)	-0.024 (0.025)	-0.033 (0.051)	-0.025 (0.052)
year -4	0.021 (0.014)	0.033 (0.084)	0.009 (0.033)	-0.032 (0.035)	-0.002 (0.016)	-0.002 (0.037)	0.053 (0.039)
year -3	0.020** (0.009)	0.105** (0.053)	-0.006 (0.024)	-0.021 (0.029)	-0.011 (0.012)	-0.009 (0.030)	0.009 (0.029)
year -2	0.005 (0.005)	0.016 (0.034)	0.003 (0.014)	-0.003 (0.020)	-0.002 (0.007)	0.024 (0.021)	-0.002 (0.019)
year 0	0.001 (0.004)	0.034 (0.026)	0.018 (0.011)	0.001 (0.015)	0.004 (0.006)	-0.002 (0.016)	0.035** (0.015)
year 1	0.001 (0.005)	0.061* (0.034)	-0.011 (0.017)	-0.002 (0.020)	-0.004 (0.008)	0.023 (0.020)	0.024 (0.020)
year 2	-0.003 (0.006)	0.027 (0.040)	-0.001 (0.023)	-0.047* (0.025)	-0.002 (0.011)	-0.041* (0.025)	-0.000 (0.023)
year 3	0.002 (0.007)	0.054 (0.044)	0.006 (0.029)	-0.027 (0.028)	0.002 (0.014)	-0.041 (0.029)	0.003 (0.027)
year 4	0.011 (0.009)	0.088* (0.053)	0.037 (0.038)	-0.053 (0.034)	0.006 (0.018)	-0.012 (0.035)	-0.000 (0.033)
year 5	0.022** (0.011)	0.197*** (0.063)	0.069 (0.048)	-0.116*** (0.043)	0.004 (0.022)	-0.092** (0.044)	0.006 (0.038)
Observations	115,311	115,311	146,210	146,210	146,210	146,210	146,210
# of individuals	21,433	21,433	26,362	26,362	26,362	26,362	26,362
R-squared within	0.126	0.0871	0.0363	0.0154	0.0432	0.0110	0.00596
R-squared overall	0.0170	0.0211	4.39e-06	0.00674	0.000777	0.00688	0.00203

*Notes:* This table reports the effect of the log of lottery prize on credit outcomes of individuals living in winners' postal codes. Columns represent separate panel regressions with individual fixed effects. The dependent variables are described in the first row. All balance variables are transformed by taking logs and adding 1 to the original balance value. All specifications include lottery product and winning-year fixed effects. The omitted period is year -1. This table shows that credit does not increase for a group of older subprime borrowers (older than the median age, which is 46 years in our sample). This finding suggests that the increases in credit to prime consumers are not driven by age only. Standard errors are clustered at the individual level and reported in parentheses. \*, \*\*, \*\*\* denote significance at 10%, 5%, and 1% level, respectively.