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ABSTRACT

We provide new causal evidence that keeping up with the Joneses behavior causes financial distress by examining whether lottery prizes of random dollar magnitudes increase bankruptcy filings of very close neighbors of the winner. We find that a 1% increase in the lottery prize causes a 0.04% rise in subsequent bankruptcies among the winners' close neighbors. We also provide evidence on conspicuous consumption as a mechanism for this causal relationship. The size of lottery prizes increases the value of visible assets (e.g., houses, cars) but not invisible assets (e.g., cash, financial assets), appearing on the bankruptcy balance sheets of neighboring bankruptcy filers.

Keywords: financial distress, bankruptcy, peer effects in consumption

JEL Codes: G02, D14, D31, K35

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1. Introduction

The keeping up with the Joneses hypothesis (originating with authors such as Veblen, 1899, and Duesenberry, 1949) states that an individual's utility is related to matching the consumption of the individual's social reference or peer groups. A mechanism for keeping up with the Joneses is conspicuous consumption, where individuals attempt to signal increased status by consuming specific goods that are more visible to their social reference or peer groups.

While the keeping up with the Joneses literature is very large, recent research by Georgarakos, Haliassos, and Pasini (2014); Bertrand and Morse (2016); and others has focused on the specific argument that if keeping up with the Joneses behavior is financed by the unsustainable accumulation of debt, then this can lead to increased financial distress for the peers. The aim of this paper is to provide new causal evidence on the hypothesis that peer effects in consumption can lead to financial distress for the peers.

While issues of peer effects have long been of considerable interest across many areas of finance and economics, providing empirical evidence of peer effects faces significant empirical identification challenges such as the reflection problem (Manski, 1993). In brief, when examining how peers influence the choices of each other, 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). We argue 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 and consumption affects other peers' financial distress and bankruptcies.

Our identification strategy (lottery winners within small neighborhoods) is similar to Kuhn, Kooreman, Soetevent, and Kapteyn (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. While our identification strategy (lottery wins in neighborhoods) is similar to Kuhn et al. (2011), we examine a very different question; specifically, whether the keeping up with the Joneses type behavior of the neighbors of lottery winners can result in financial distress because the increased consumption is financed by unsustainable debt.

As emphasized by Georgarakos, Haliassos, and Pasini (2014), the issue of whether peer effects generate an increase in consumption is conceptually very distinct from the issue addressed in this paper, of whether that increase in peers' consumption generates subsequent financial distress. It is indeed possible that keeping up with the Joneses behavior 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). In a related paper, Coibion, Gorodnichenko, Kudlyak, and Mondragon (2014) argue that banks may be reluctant to extend credit to poorer individuals in higher inequality neighborhoods, which would curtail both keeping up with the Joneses behavior and related financial distress.

Our contribution is to provide new causal evidence on the link between keeping up with the Joneses behavior and financial distress by using the identification strategy that exploits randomly sized income shocks of lottery winners relative to their nonlottery-winning neighbors. We examine the hypothesis that, if an exogenous income shock from a lottery win causes an increase in conspicuous consumption by both the winner and her neighbors and if this increased consumption causes financial distress for the neighbors because it is financed by an unsustainable buildup in debt, then we should observe a subsequent increase in bankruptcy filings from the neighbors following the date of the lottery win.

Similar to other papers using lottery wins for identification (e.g., Imbens, Rubin, and Sacerdote, 2001; Hankins, Hoekstra, and Skiba, 2011), our identification strategy exploits the random dollar size of lottery wins across all lottery winners, conditional on winning the lottery. For this reason, our specifications only examine neighborhoods where there is a single lottery win and compare the impacts of large lottery wins on neighbors' bankruptcies with the impacts of small lottery wins on neighbors' bankruptcies. We test the prediction that larger lottery wins should cause more bankruptcies from nonlottery winning neighbors compared with smaller wins.

Our data match the universe of lottery winners to the universe of bankruptcy filers in a specific Canadian province. We are able to match these two databases because we can observe the name and six-digit postal code (median of 13 households) of every individual in both databases. We define neighborhoods as the six-digit postal code (containing a median of 13 households), where we can observe both the universe of lottery prizes won and the universe of

bankruptcy filings. Because we can observe both the dollar magnitude of every lottery prize and the universe of every bankruptcy filing in that specific postal code, we can examine whether the random magnitude of the lottery prize has a causal impact on future bankruptcies of the very close neighbors of the lottery winner.

Our main finding is that a larger lottery win will lead to more bankruptcy filings from close neighbors of the lottery winner compared with bankruptcy filings of close neighbors of a smaller lottery winner. In terms of economic magnitudes, our main finding is that an increase in the dollar amount of a lottery prize significantly increases the number of subsequent bankruptcy filings by very close neighbors of the lottery winner (i.e., within the same six-digit postal code). In the 0- to 2-year event window after the date of the lottery win, where there is a base of 0.46 bankruptcies per postal code over the three-year window, a 1% increase in the lottery prize in the postal code significantly increases the number of subsequent bankruptcy filings in that postal code by 0.04%.

We also find that there is heterogeneity in the strength of our estimated peer effects across neighborhoods of different types. Our estimates indicate that peer effects are stronger in poorer neighborhoods relative to richer neighborhoods. This is consistent with evidence from the social psychology literature (e.g., Bianchi and Vohs, 2016) that poorer individuals are more likely to cultivate relationships with close neighbors. Similarly, we find that our estimated peer effects emerge faster in urban neighborhoods relative to rural neighborhoods. This is consistent with the argument of Han and Hirshleifer (2016) that it is easier to observe the consumption of close neighbors in urban neighborhoods relative to rural neighborhoods. The size of the economic effects in these subsamples is substantially larger than the effects in the whole sample.

While our main evidence shows that keeping up with the Joneses causes financial distress, using a similar identification strategy, we are also able to provide additional evidence that conspicuous consumption serves as a mechanism for the relationship between keeping up with the Joneses and financial distress. The conspicuous consumption hypothesis focuses on the specific products that are purchased and predicts that individuals who attempt to keep up with the Joneses will be more likely to purchase conspicuous or visible products that can be observed by their relevant social reference or peer groups. The previous literature on the conspicuous consumption hypothesis (e.g., Charles, Hurst, and Roussanov, 2009; Agarwal, Qian, and Zou, 2016; Bertrand and Morse, 2016) has empirically distinguished between products that are more

visible to peers (e.g., houses, cars, jewelry) or less visible to peers (e.g., cash, financial assets). The testable implication of the conspicuous consumption argument, when examining issues of financial distress, is that it should be the consumption of goods that are visible to close neighbors, rather than the consumption of goods that are invisible to close neighbors, that leads to financial distress.

We are able to provide new evidence on this argument by exploiting the full balance sheets of bankruptcy filers in our data. We find that bankruptcy filers who filed for bankruptcy following a larger lottery win of a close neighbor have significantly larger holdings of visible assets (e.g., houses, cars, motorcycles) relative to the holdings of these same visible assets by bankruptcy filers who filed for bankruptcy following smaller lottery wins of a close neighbor. On the other hand, we find no significant relationship between the size of a neighbor's lottery win and bankruptcy filers holding of invisible assets. In terms of economic magnitudes, in the 0- to 2-year event window 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, as reported on the bankruptcy balance sheet on the date of the bankruptcy filing. In similar tests examining differences in the value of invisible assets (e.g., cash, financial assets), we find no significant differences between bankruptcy filers whose neighbors had relatively larger or smaller lottery wins.

2. Relationship to the Literature

This paper provides empirical evidence on two hypotheses. The first examines whether keeping up with the Joneses causes financial distress, while the second examines whether conspicuous consumption of visible (rather than invisible) goods is a mechanism by which this process operates. We discuss each in turn and how it relates to previous research.

2.1. Does Keeping Up with the Joneses Cause Financial Distress?

The closest paper to ours in terms of our main hypothesis is Georgarakos, Haliassos, and Pasini (2014), who also directly examine whether keeping up with the Joneses behavior is linked to financial distress. The data and methodology that Georgarakos et al. (2014) uses, however, is very different from ours. Their main source of data 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. is that such perceptions of low income relative to the social circle are related to measures of possible future financial distress (e.g., debt service 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 actual financial distress as measured by individual bankruptcy filings.

Bertrand and Morse (2016) provide "indirect" (p. 3) rather than causal evidence to address the main hypothesis of this paper — that keeping up with the Joneses is related to increased debt and increased financial distress. They use data from the Consumer Sentiment Survey to document that the nonrich report to be "worse off financially" (p. 3) when exposed to the rich. 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 very 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 (Manski, 1993) inherent in testing for peer effects. We examine neighborhoods with a single exogenous income shock to one neighbor and no income shocks to any other neighbors. The randomization in our identification strategy comes from the fact that conditional on winning the lottery, the dollar magnitude of the lottery win will be random. This allows us to compare the impact of large lottery winners with the impact of small lottery winners, where the magnitude of the lottery win is random.

While our paper is the first to examine the effect of exogenous income shocks (from lottery winnings) on the *bankruptcy* filings of very close neighbors, a variety of papers have examined the impact of exogenous income shocks on the *consumption* choices of very close neighbors. As described earlier, Kuhn et al. (2011) explore the impact of lottery wins on the consumption choices of very close neighbors of the lottery winners, where their neighborhoods are defined by Dutch postal codes (with an average of 19 households). They find that a neighbor winning the lottery has a significant effect on the consumption of nonlottery winning neighbors. Similarly, Angelucci and De Giorgi (2009) test the effect of exogenous government cash

transfers on the consumption and debt choices of close neighbors. They also 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.[1]

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. Han and Hirshleifer (2016) contrast their *visibility bias* hypothesis with the standard, Veblen-type, keeping up with the Joneses hypothesis tested here. In their model, the authors show that even individuals with the same wealth can overconsume because of the visibility bias, while in the Veblen models, overconsumption will only occur with an actual variation in wealth between peers. Because our context examines actual increases in income from lottery wins, we are able to provide evidence on the Veblen-type keeping up with the Joneses hypothesis but not the Han and Hirshleifer (2016) visibility bias hypothesis.

Our study is closely 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 focus only on the impact of a lottery win on the bankruptcy of the lottery winner herself (in the Appendix to this paper, 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.[2]

[1] 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.

[2] For example, Fay, Hurst, and White (2002); Gross and Souleles (2002); Agarwal and Song (2015); and many others.

While our study examines the impact of peer effects in consumption on a specific financial outcome (e.g., household bankruptcy, financial distress), a large volume of literature in finance has examined peer effects in the context of various financial choices, including choices relating to stock market participation and financial asset allocation,[3] the market for loans,[4] choices relating to retirement plans,[5] and foreclosure and house prices.[6]

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 keeping up with the Joneses behavior, thus leading to financial distress. Much of this 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.[7] The link between income inequality and financial distress has been more formally examined in various macroeconomics papers.[8]

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 of other contexts.[9] 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, 2015). In this paper, while

[3] 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).

[4] See Duarte, Siegel, and Young (2012).

[5] See Duflo and Saez (2003).

[6] See Campbell, Giglio, and Pathak (2011).

[7] See Rajan (2010); Acemoglu (2011); Becker (2011); Krugman (2013); Stiglitz (2013); and Cochrane (2014).

[8] See Krueger and Perri (2006); Iacoviello (2008); Bordo and Meissner (2012); and Kumhof, Ranciere, and Winant (2015).

[9] 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(2015) on household labor supply.

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.

2.2. Conspicuous Consumption as a Mechanism for the Relationship Between Keeping Up with the Joneses and Financial Distress

The second hypothesis we empirically examine in this paper is whether conspicuous consumption serves as a mechanism for financial distress following keeping up with the Joneses behavior. 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. Research on conspicuous consumption is typically based on evidence of the consumption of specific, more or less conspicuous, products. We test this hypothesis by exploiting our ability to observe the full balance sheets of all bankruptcy filers including both visible assets (e.g., houses, cars, motorcycles) and invisible assets (e.g., cash and financial assets).

Our focus on specific products follows the previous literature examining conspicuous consumption. Charles, Hurst, and Roussanov (2009) provide evidence that individuals from racial minority groups in the U.S. own a higher proportion of visible goods in their consumption bundles (specifically clothing, jewelry, and cars), which they ascribe to status seeking using costly signaling. Bertrand and Morse (2016) provide evidence that poorer individuals in high-inequality states (and who may be more prone to keeping up with the Joneses behavior) will be more likely to purchase more visible products. Examples of more visible products in their study (based on Heffetz, 2011) include shelter, cars, and tobacco products, while examples of less visible expenses include health insurance, business services, and interest paid.

Kuhn et al. (2011) also provide evidence that keeping up the Joneses leads to relatively poorer individuals purchasing products that are more visible. They find that a close neighbor winning the lottery has a very large and significant impact on the consumption of cars among the nonwinning neighbors and a less robust but still significant impact on the consumption of exterior home renovations. As noted by Kuhn et al. (2011), cars and exterior home renovations are two of the most visible products that can be purchased.

3. Data

We use three main data sources for data on (1) lotteries, (2) bankruptcies, and (3) neighborhoods, which we discuss in turn. Full summary statistics of all data we use are reported in Table 1 for bankruptcy count data and Table 6 for balance sheet data.

3.1. Lottery Data

All of the individual provincial governments in Canada have monopolies over official lotteries run in their jurisdictions. 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.

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 to us by the provincial lottery organization (which, under the terms of our nondisclosure agreement, we are not able to divulge).[10] 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. 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, with a maximum of C$150,000. As described earlier, these dollar magnitudes of lottery wins provide the key exogenous variation for our tests.

3.2. Bankruptcy 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.

[10] We do not use any personal identifiable information (PII) in our analysis.

There are two separate bankruptcy databases, which provide the dependent variables for the two main empirical sections of 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 impacts 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.

Although our bankruptcy count data allow us to examine how many individuals in a neighborhood filed for bankruptcy after neighboring lottery wins (i.e., the extensive margin), the OSB has also provided us with the full balance sheet of individual bankruptcies filed electronically.[11] 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. These individual bankruptcy balance sheet data form the second of our two main bankruptcy databases. 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. We use these individual-level balance sheet data to run intensive margin regressions examining how the size of a lottery win affects the individual balance sheet characteristics of neighboring bankruptcy filers.

3.3. Neighborhood Data

Our main geographic unit of analysis and our definition of "neighborhoods" are Canadian six-digit postal codes, which contain a median of 13 households. These areas are extremely small, often smaller than a city block in size. Single apartment buildings, for example, can have multiple six-digit postal codes. Both our individual lottery winner data and our individual bankruptcy filer data contain postal code data for all individuals; thus, our primary unit of

[11] 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. These results are available upon request.

analysis is defined by bankruptcy filers living in the lottery winner's six-digit postal code but exclude the winner herself.

In addition, we use a second, slightly larger measure of neighborhood geography, called a dissemination area (DA), which contain approximately 200 households (i.e., the size a few city blocks) with an average size of approximately 0.2 square kilometers. Using a conversion tool known as the Postal Code Conversion File (PCCF), we can accurately match Statistics Canada DA-level geographies to the much smaller Canada Post 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. Relative Magnitude of Lottery Shocks to Income

An important element of all lottery-based studies is whether the magnitudes of the lottery prizes are salient relative to individual income levels. In Figure 1, we provide a histogram of the dollar magnitudes of all lottery wins in our sample, and, in Figure 3, Panel A, we provide kernel densities of median DA income at the DA level taken from Canada census data. These figures report data from the 7,377 lottery wins and matched DAs in our main sample, described later. 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, Panel A, shows that the mean of the median DA income data across the DAs in our study is C$31,000. In other words, the larger lottery prizes are clearly very salient relative to median DA income, but the smaller prizes are somewhat inconsequential (later, we describe the quartiles shown in Figure 3, Panel A).

4. Research Design

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., 2016; 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 non-lottery winners, who may be systematically different because non-lottery winners may be nonlottery players.

4.1. Inclusion of Postal Codes with Only a Single Lottery Win

Our research design restricts our sample to neighborhoods where there is only a single lottery win over the period of our data. The primary reason for this is based on the very long event windows in our study (five years after the lottery win and five years before the lottery win for the placebo tests) and the difficulties in interpreting the impact of multiple events (i.e., multiple lottery wins in the postal code) within the same event window. By restricting our sample to only postal codes with a single lottery win over the period of our data, 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 et al., 2011, and many others) that the lags between an exogenous shock and a bankruptcy filing are long and variable. Our event window lengths are exactly the same as in Hankins et al. (2011).

4.2. 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 following section.

4.3. Removal of Very Large Winners

We also exclude from our sample all very large lottery wins of more than C$150,000. Both Imbens et al. (2001) and Hankins et al. (2011) also exclude extremely large lottery winners from their samples (which can be many million dollars in magnitude) to reduce the possible impact of very large outliers. Our choice of C$150,000 as the cutoff is exactly that of Hankins et al. (2011). There are only 105 winners with prizes above C$150,000.

4.4. 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 themselves filed for bankruptcy. Hence, we identify and exclude lottery winners in a postal code who also filed for bankruptcy.[12] There are 824 lottery winners in our sample who filed for bankruptcy at some stage in our sample (either before or after the lottery win), all of whom we exclude from our main 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 the 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

[12] We delete any personal identifiable information (PII) after this exercise. No PII is used in any analysis.

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.

4.5. 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 compared with small lottery winners. As is the case with Hankins et al. (2011), our 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 in this regard relative to most studies in the lotteries literature, which 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. So, if there was 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 later 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 will 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 subsequent to the date of their lottery win.

This indicates that for the larger lottery winners in their sample (maximum of U.S. $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 to the magnitude of lottery wins in the Hankins et al. (2011) study.

5. 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., 2016; 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. To test this assumption for neighborhood observables, we run essentially the same test as those authors 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 (the full list of these census variables 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 (conditional on there being a single lottery win in that neighborhood).

Figure 3, which shows the median income in each DA and the Gini coefficient for each DA, also provides additional graphic evidence of this lack of a relationship between DA observables and lottery win size. In both panels of Figure 3, we plot the distribution of these variables across all DAs in our sample for each of four lottery prize quartiles based on the size of the lottery win. As can be seen in both panels, the distributions of DA median income and Gini coefficient across the four quartiles are essentially indistinguishable from each other, thus confirming visually the statistical finding that there is no relationship between lottery win size and observable DA characteristics.

6. Extensive Margin Tests: Neighborhood Bankruptcy Counts

As described earlier, we have two main hypotheses in this paper. The first examines whether exogenous lottery shocks affect the total counts of bankruptcies in that postal code neighborhood (labeled *extensive margin tests*). The second examines the role of conspicuous consumption by testing the effect of exogenous lottery shocks on more or less visible balance sheet assets of individual bankruptcy filers (*intensive margin tests*). We describe each in turn.

6.1. Extensive Margin Model

All of our tests exploit the exogenous variation in the size of the lottery win, conditional on there being a single lottery win in the postal code over the course of our sample. For this reason, the basic structure of our tests is an event study-type cross-sectional specification in which the event date of interest (the date of the lottery win) is set equal to *t = 0*. This cross-sectional specification, with event dates set relative to time *t = 0*, is essentially the same as used by Hankins et al. (2011). As in a standard cross-sectional event study-type specification, we use various event windows before and after the lottery win to examine how the coefficient of interest changes over various time periods after the win.

Our basic model, which only includes neighbors from within the same postal code as the lottery winner, is as follows:

(1) $$Neighbor\_Bankruptcy\_Count_p = \beta_1 \ln\left(lottery\ win\ size\right)_p + \beta_2 (Controls)_d + \delta_p + \alpha_p + \varepsilon_p,$$

where subscript *p* represents postal code of the winner and subscript *d* represents the DA of the winner. Because, by design, there is only a single lottery win in each postal code, the subscript *p* captures each separate lottery win. As in the literature (e.g., Imbens et al., 2001; Hankins et al., 2011; Cesarini et al., 2016), this is an event study-type specification, where all events (lottery wins) are set to occur at time *t = 0*.

The dependent variable represents the number of bankruptcies in postal code *p* for a variety of different event windows relative to *t = 0*. Because our dependent variable is a count variable, we use the Poisson specification. The key independent variable is the log of the lottery win size, occurring at time *t = 0*. Given that this is a neighborhood-level regression (our dependent variable is the number of bankruptcies coming from winner's neighbors within the postal code), we only include neighborhood-level rather than individual-level controls. 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, δ, capturing the year of the lottery win (to capture business cycle variation), and a fixed effect, α, for each of the different types of lottery game (product) provided by the provincial lottery corporation. In terms of event window length, we provide results for both groups of years (Table 3) and individual years (Table 4). These long event windows, which examine event windows from five years before to five years after the event date 0, 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 3 uses similar event windows as Hankins et al. (2011): 0 to 2 years and 3 to 5 years. We also examine event windows before the winning (-1 to -2 years and -3 to -5 years) to test if they are statistically insignificant and consistent with parallel trends in postal codes receiving various shocks from lotteries. In Table 3, the smaller number of observations in the 3- to 5-year sample compared with the 0- to 2-year sample 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.

6.2. Full Sample Results

The results in Table 3, Panel A, report results for our full sample, with multiple years in each event window. We find significant and positive coefficients in the event windows after the event date, indicating that the dollar magnitude of a lottery win will increase subsequent bankruptcies in the individual postal code. No coefficients are statistically significant in the event windows before the lottery win, which supports our identifying assumptions. The coefficient in the 0- to 2-year event window in Panel 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. In terms of economic magnitudes, our estimated coefficient for the 0- to 2-year event window implies that a 1% increase in the dollar magnitude of the lottery win across all postal codes with a single lottery win will cause a 0.04% increase in subsequent bankruptcy filings in that postal code. As can be seen in the summary statistics (Table 1), this increase in bankruptcies is from a base of 0.455 bankruptcies per postal code over the 0- to 2-year event window.

Table 4 presents results for the effect of lotteries on winner's neighbors' bankruptcies for each individual year from *t = -5* to *t = 5* (where 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 significant coefficient is year 3, which is equal to 0.0129 and is significant at the 5% level. This coefficient implies that a 1% increase in the lottery win will cause 0.087% increase in neighbors' bankruptcies in year 3 after winning. On average, there are 0.148 bankruptcies per postal code in year 3.

An important element of the peer effects literature (as summarized by De Giorgi, Frederiksen, and Pistaferri, 2016) is the choice of an appropriate peer or social reference group. While some peer groups in the literature have been defined based on racial characteristics (e.g., Charles et al., 2009) or workplace colleagues (e.g., De Giorgi et al., 2016), many papers have used geographically close neighbors as the relevant peer group (e.g., Angelucci and De Giorgi, 2009; Kuhn et al., 2009). When using close neighbors as the relevant peer group, the implicit assumption in the literature is that the social interactions between very close neighbors should be stronger than the social interactions between neighbors who are farther away.

We can empirically test the prediction that peer effects among close neighbors are larger than peer effects among neighbors slightly farther away. Our main test in Table 3, Panel A, uses as a dependent variable bankruptcy counts within the winner's postal code (median of 13 households) but excludes the winner's bankruptcy. In Table 3, Panel B, we use as the dependent variable bankruptcy counts per postal code of neighbors who are slightly farther away, which we define to be bankruptcies occurring within the winners' DA area (outlined in red in Figure 2) but excluding the winner's own postal code. We define these slightly more distant neighbors as outer rings. 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. Our predication is that we should not find significant coefficients in the outer rings examining the bankruptcies of more distant neighbors.

Table 3, Panel B, provides the results of these tests for outer ring neighbors for event windows of multiple years, while Table 4, columns 3 and 4, present results of these tests for single years. As predicted, we find no statistically significant results for the slightly more distant neighbors in the outer rings. These test results are consistent with our argument that very close

neighbors are a more relevant peer group compared with neighbors slightly farther away and that the effect of peer comparisons is very local and quickly dissipates with distance.

6.3. Neighborhood Heterogeneity

To examine how neighborhood heterogeneity affects our results, in this section, we provide results for various subsamples defined based on the observable neighborhood characteristics from census data. Statistics Canada provides a large amount of observable census-level data for each DA in Canada (approximately 200 households on average), and we are able to match each DA to each postal code in Canada (median of 13 households) as displayed in Figure 2. Table 5 reports results for the previously given specification, in which we categorize postal codes based on observable measures of low income, income inequality, and urbanization level. We discuss each in turn.

6.3.1. Neighborhood Heterogeneity: Income

The central element of our empirical design in this paper is the assumption that close neighbors are an appropriate social reference group. One important result documented in the social psychology literature, however, is that there is heterogeneity in the extent to which close neighbors are an important reference group across neighborhoods of different types. For example, Bianchi and Vohs (2016) document that neighbors are more important as peer groups in poor neighborhoods compared with wealthier neighborhoods. In particular, Bianchi and Vohs (2016) provide evidence that poorer individuals spend more time socializing with close neighbors compared with richer individuals, using data from the General Social Survey and the American Time Use Survey. Bianchi and Vohs (2016, p. 2) suggest that one possible reason for this finding is that poorer individuals are more likely to cultivate relationships with close neighbors because those neighbors could be used to provide "sporadic instrumental support" (e.g., unexpected child-care needs). Because richer individuals have more resources, they will typically require less of this kind of sporadic instrumental support from neighbors; thus, the rich will be less likely to cultivate close neighbors as members of their social network.

This heterogeneity in the importance of neighborhood relationships across neighborhoods of different income levels, documented by Bianchi and Vohs (2016), provides us with an additional test with which to corroborate our full sample results. If, as documented by the social

psychology literature, close neighbors are more important as peers in poor neighborhoods relative to rich neighborhoods, then we can test the hypothesis that our main results should be stronger in poor neighborhoods relative to rich neighborhoods. We conduct such a test in this section.

Table 5, Panels A and B, 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 results in which the proportion of low-income individuals in a DA is above the median level across DAs, and Panel B shows results for DAs with below the median level of low-income individuals.

The coefficients in Table 5, Panel A, are larger and different from zero at higher levels of significance (5% level for the 0- to 2-year event window and 1% level for the 3- to 5-year event window) than the full sample results reported in Table 3, Panel A. Table 5, Panel B, shows that no coefficients are significant for postal codes with below-median levels of low-income individuals. These results are, therefore, consistent with the argument that the specific peer effects between close neighbors documented in this study (lottery winners causing neighboring bankruptcy filings) are stronger in poorer neighborhoods.

6.3.2. Neighborhood Heterogeneity: Income Inequality

The second form of heterogeneity across neighborhoods we examine is neighborhood income inequality. The basic premise of our identification strategy in this paper is that a large lottery win in a neighborhood will increase keeping up with the Joneses behavior among close neighbors of the winner, leading to more consumption peer effects and more financial distress. The previous literature has shown that income inequality can lead to keeping up with the Joneses behavior and conspicuous consumption (e.g., Bertrand and Morse, 2016). An extension of this argument is that a lottery win in a neighborhood that already has high *preexisting* levels of income inequality before the lottery win will even further exacerbate these consumption peer effect processes, leading to larger impacts on subsequent neighborhood bankruptcies, compared with similar lottery wins in neighborhoods with lower levels of preexisting income inequality. The testable prediction of this argument is that the peer effects results in our main tests should be

greater in higher income inequality neighborhoods compared with lower income inequality neighborhoods.

Table 5, Panels C and D, report coefficient estimates based on splitting the sample at the median level of DA Gini coefficient. 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. Table 5, Panel D, shows that no coefficients are significant for the sample of below-median income inequality DAs. These results are consistent with the prediction that a large lottery win will have a greater impact in high-income inequality neighborhoods relative to low-income inequality neighborhoods because the lottery win will further exacerbate keeping up with the Joneses behavior within the neighborhood.

6.3.3. Neighborhood Heterogeneity: Urban vs. Rural

In Table 5, Panels E and F, we examine a different form of heterogeneity based on urban or rural character of neighborhoods. This heterogeneity is based on the observation of Han and Hirshleifer (2016) that "urbanization is associated with a higher intensity of social interaction and observation of the consumption of others" (p.4). Han and Hirshleifer (2016), thus, predict that urbanization will be correlated with the strength of consumption-based peer effects between close neighbors.

Table 5, Panel E, examines only 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 5, Panel C, being a relatively large fraction of the total sample. The results in Panel E show that in the 0- to 2-year event window, the estimated coefficient is both larger and estimated with greater precision compared with the full sample estimate in Table 3, Panel A, (significant at 1%, whereas the full sample estimate was only significant at 5%).

Table 5, Panel F, shows results for postal codes 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 keeping up with the Joneses may take longer

to appear in rural neighborhoods compared with urban neighborhoods examined in Table 5, Panel E. This finding is roughly consistent with the Han and Hirshleifer (2016) argument that it is easier for urban individuals relative to rural individuals to observe the consumption patterns of neighbors.

7. Intensive Margin Tests: Visible or Invisible Assets

The intensive margin tests in this section are all at the level of the individual bankruptcy filer rather than at the aggregate neighborhood level, as in the extensive margin tests presented earlier. Thus, while the dependent variable in our extensive margin tests is the count of neighboring bankruptcies, the dependent variables in our intensive margin tests are balance sheet characteristics of those individual neighbors who do file for bankruptcy following a lottery win of a neighbor. In particular, the characteristics we examine relate to various visible and invisible assets as reported by bankruptcy filers to the Canadian bankruptcy regulator, OSB, on the date of their bankruptcy filing. Thus, our intensive margin tests examine all individual bankruptcy filers in the neighborhoods of lottery winners (excluding the winners) and compare whether bankruptcy balance sheet characteristics of these individual bankruptcy filers differ between larger or smaller lottery wins in their postal codes.

7.1. Intensive Margin Model

Our intensive margin specification is as follows:

$$(2) \qquad Bankrupt\_Asset_i = \beta_1 \ln\left(lottery\ win\ size\right)_i + \beta_2 (Controls)_i + \delta_i + \alpha_i + \varepsilon_i.$$

Our dependent variable is the value of a particular asset appearing on the balance sheet of a bankruptcy-filing neighbor of a lottery winner. The key independent variable is the log of lottery win size, which comes from the database of lottery winners (one per postal code) used in the extensive margin tests. Because the intensive margin specification is at the level of the individual (nonlottery winning, bankruptcy-filing neighbor), we can also include individual-level controls of the bankruptcy filing neighbor (*i*) in addition to the neighborhood controls used earlier. Table 6 provides details of these controls. Similarly, because these tests are at the level of the individual bankruptcy filer (*i*), we can also include dummies for each of the 17 different "reasons for financial distress" given by filers when they file, as reported by the OSB. Table 6

also describes these reasons. As in the extensive margin specification, lottery-specific fixed effects (i.e., lottery product and lottery winning year fixed effects) are also included.

We examine a variety of different specifications to categorize the various balance sheet measures of the different assets in bankruptcy of winners' neighboring bankruptcy filers. 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 log of these asset values and use them as dependent variables. We also add 1 to all values to make log of 0 value equal 0, not missing. As an additional robustness check, instead of adding 1 to all 0 observations, we transform all asset values using the inverse hyperbolic sine transformation (Burbidge, Magee, and Robb, 1988; and MacKinnon and Magee, 1990). Our results using the inverse hyperbolic sine transformation are essentially identical to our results using the transformation adding 1 to zero asset values; they are available upon request.

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 assets that we define as visible (e.g., cars, houses, motorcycles) also have another important feature, which is that they are all durable assets. All of these assets provide a stream of consumption services over time, and all can be disposed of at any time. Thus, the fact that a bankruptcy filer owns a particular visible durable asset at the date of the bankruptcy filing and did not dispose of it prior to 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 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 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 date of the

neighbor's bankruptcy filing), although in our case, we can observe the actual dollar value of the car (or house or motorcycle) 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 housing values of winners. This visible consumption mechanism could explain how 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 and 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.

7.2. Intensive Margin Results

We report results for our intensive margin balance sheet tests for multiple years in Table 7 and for single years in Tables 8 (years after the event date) and 9 (years before the event date). In each cell of these tables, we report the effect of the lottery prize size in that neighborhood on various balance sheet amounts reported by nonwinning bankruptcy filers in that neighborhood. In these tables, we only report a single coefficient (on the log of lottery win size) from each regression. Full results are available upon request.

Our main result in Tables 7 and 8 is that we find statistically significant coefficients for cars, houses, and motorcycles. These coefficients are significant in the 0- to 2-year event window in Table 7 and year 2 in Table 8. In other words, these results are consistent with the hypothesis that the larger the lottery prize of a neighbor, the greater the value of visible assets on the balance sheets of the winner's neighbors who file for bankruptcy after winning. When examining invisible assets such as cash and financial assets, we do not find any significant coefficients. We generally do not find significant coefficients for the tests examining balance sheets of bankruptcy filers before the lottery win for either groups of years (Table 7) or single years (Table 9). The results in Table 7 imply that, in the 0- to 2-year event window 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.

Our findings regarding statistically significant coefficients for visible assets (specifically cars, houses, and motorcycles) can be compared with the lack of significant coefficients for

invisible assets (specifically cash and financial assets) reported in Tables 7 and 8. In other words, our results indicate that, while a large (relative to a small) lottery win by a neighbor will increase the value of visible assets reported on bankruptcy balance sheets, such a relationship is not evident for invisible assets on the bankruptcy balance sheets of the neighbors. For individuals who file for bankruptcy after the lottery win of a neighbor, therefore, the size of the lottery win of their neighbor will affect their holding of visible but not invisible assets. We argue that these results are consistent with the hypothesis that conspicuous consumption is a mechanism through which keeping up with the Joneses behavior causes subsequent financial distress for peers.

8. Conclusion

This paper provides new causal evidence that keeping up with the Joneses behavior leads to financial distress. Our identification strategy uses lottery prizes of random magnitudes as exogenous shocks and measures how these shocks affect bankruptcy filings by very close neighbors of the lottery winner.

Using Canadian administrative data, we can observe the universe of lottery winners and the universe of bankruptcy filers within Canadian six-digit postal codes containing a median of 13 households. We find that the magnitude of a lottery win causes a significant increase in bankruptcy filings within the winner's postal code in the 0- to 2-year event window. A 1% increase in the lottery prize causes a 0.04% increase in subsequent neighborhood bankruptcies.

Using unique data on bankruptcy balance sheets, we also provide evidence that conspicuous consumption plays a role in this causal relationship. We find that the larger the magnitude of a lottery prize, the larger the value of visible assets (e.g., houses, cars, motorcycles) on the balance sheets of close neighbors of the winner who file for bankruptcy after the lottery winning. On the other hand, we find no such relationship for invisible assets (e.g., cash, financial assets).

Our results are of importance because they provide causal evidence on the hypothesis, recently proposed by Georgarakos et al. (2014) and Bertrand and Morse (2016) that keeping up with the Joneses behavior can result in financial distress for the peers if that increased consumption by the peers is financed by unsustainable debt, resulting in subsequent financial distress. Our main contribution is to provide new evidence on this hypothesis, by using a

methodology (lottery winners within small neighborhoods) that specifically allows us to overcome challenges inherent in examining peer effects, such as the reflection problem.

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

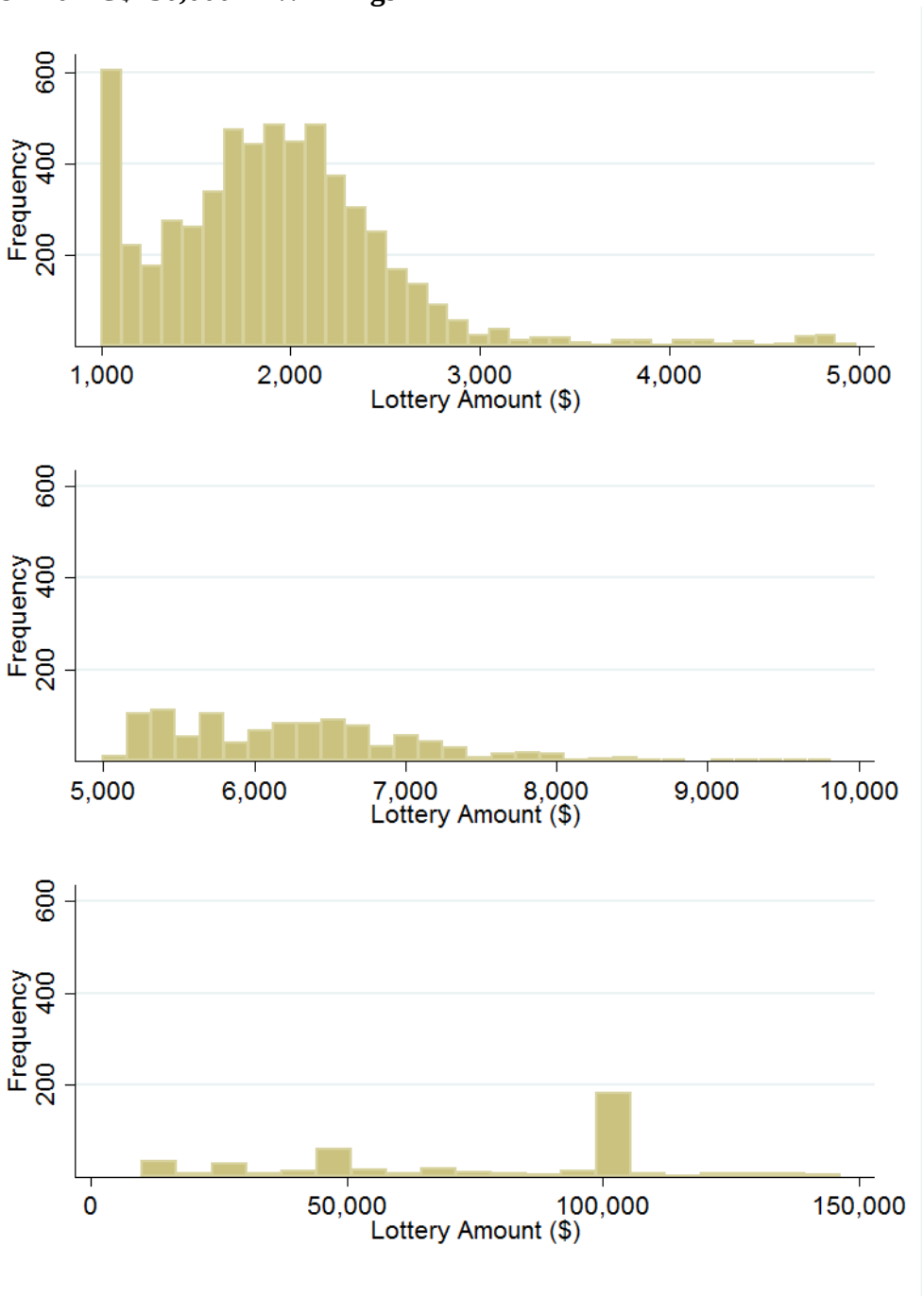
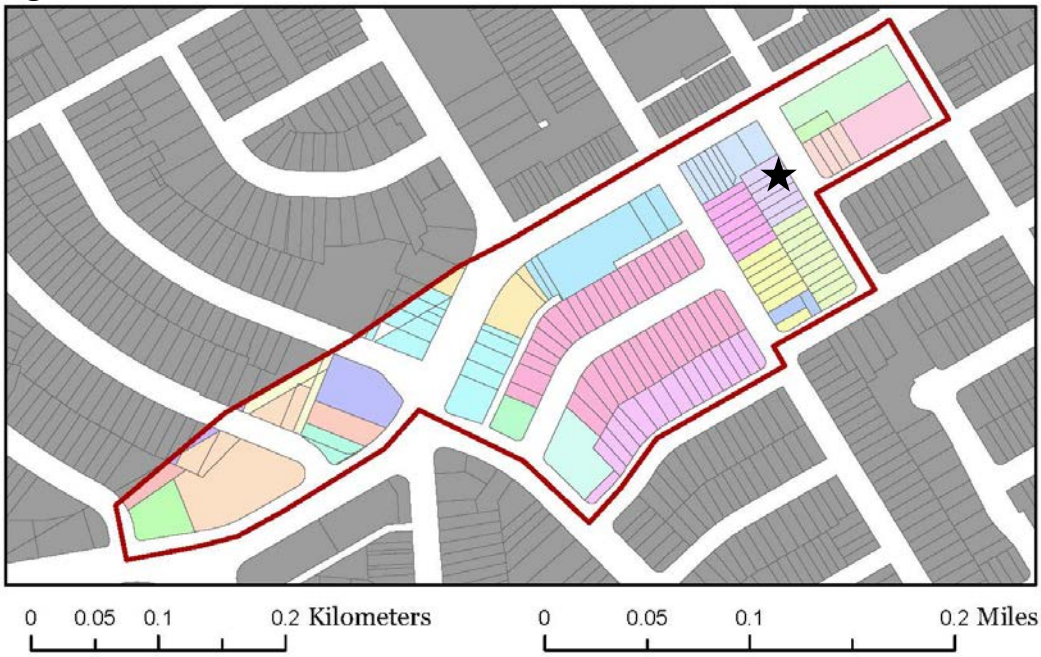


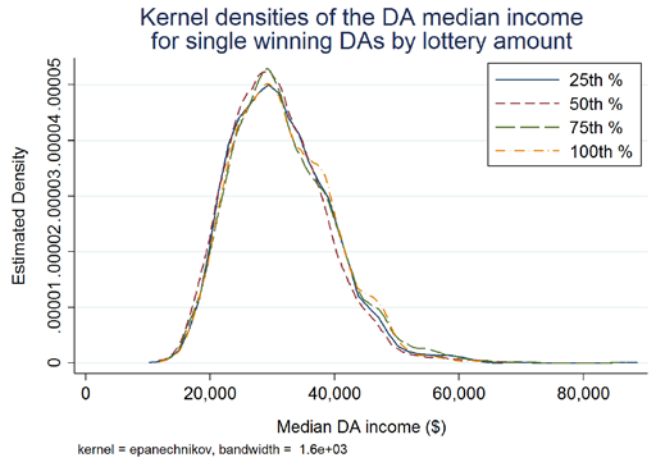
Figure 2. Postal Codes and Dissemination Areas



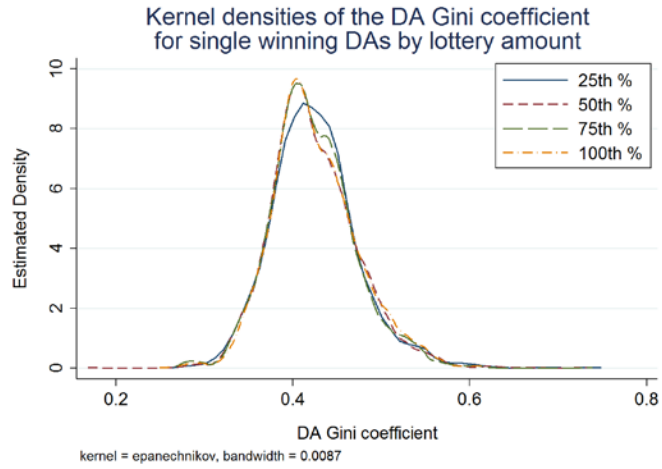
Notes: This map shows the size of postal code and DA neighborhoods used in this study. The smallest rectangular shapes are building lots. The colored sets of lots are postal codes. The red lined figure is a census DA. The star represents a lottery winner. A median postal code has 13 households (dwellings).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

Figure 3. No Relation Between Lottery Amount and Neighborhood Observables

Panel A. The Distribution of DA Median Income by Lottery Amount



Panel B. The Distribution of Preexisting Income Inequality by Lottery Amount



Notes: This figure shows DA median income and DA Gini coefficient distributions for the four quartiles of the lottery winning amount. There is no relation between the Gini coefficients or median income and lottery amounts.

Table 1. Summary Statistics for Postal Codes with Single Lottery Win (Extensive Margins)

Variable	Obs.	Mean	Std. Dev.
Winning amount ($)	7377	6911	19086
Log of winning amount	7377	7.903	0.963
Winning year	7377	2009	3
Bankruptcy rate relative to the winning time, years:			
0 to 2	5352	0.455	0.967
3 to 5	2586	0.41	0.91
-1 to -2	7377	0.294	0.724
-3 to -5	7377	0.407	0.93
DA Gini coefficient	7377	0.424	0.049
Median income ($)	7377	31451	8059
Population density (person per sq. km.)	7377	2550	2276
Region type (1 to 8 score)	7377	1.578	1.241
Unemployment rate (%)	7377	4.1	3.356
Numerical literacy score (between 100 and 500)	7377	277	11
Divorced (proportion of DA population)	7377	0.078	0.032
Separated (proportion of DA population)	7377	0.028	0.015
Widowed (proportion of DA population)	7377	0.046	0.044
High school (proportion of DA population)	7377	0.234	0.069
Apprenticeship (proportion of DA population)	7377	0.122	0.058
College (DA) (proportion of DA population)	7377	0.207	0.064
University (DA) (proportion of DA population)	7377	0.189	0.105
Graduate (DA) (proportion of DA population)	7377	0.063	0.062
Homeownership	7377	0.386	0.079
Male	7377	0.498	0.028
Age between 20 and 39 years	7377	0.301	0.097
Age between 40 and 64 years	7377	0.335	0.067
Age over 65 years	7377	0.109	0.087

Table 2. Test of Randomization: Effect of Neighborhood Characteristics on Lottery Win Size

Dependent Variable: Log (Lottery Dollar Win Size)		
DA Gini coefficient	0.274	(0.279)
Median income ($)	5.33e-07	(2.23e-06)
Population density (person 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^2	0.000190	
F-test	1.061	
Prob > F	0.3822	

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

Table 3. The Effect of Lottery Winning on the Bankruptcies of Winners' Neighbors (Extensive Margin)

Event window (years)	0 to 2	3 to 5	-1 to -2	-3 to -5
Panel A. Postal codes				
Log of winning amount	0.0199**	0.0266*	0.0096	-0.0045
	(0.0101)	(0.0139)	(0.0073)	(0.0086)
Number of observations	5,352	2,586	7,377	7,377
Panel B. Outer rings (DAs – postal codes)				
Log of winning amount	-0.0088	0.0024	-0.0069	-0.0124*
	(0.0083)	(0.0112)	(0.0056)	(0.0072)
Number of observations	5,342	2,582	7,361	7,361

Notes: This table reports the marginal effect of the log of the lottery prize on the number of bankruptcies in the winners' neighborhoods excluding winners' own bankruptcies in four 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 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 10%, 5%, and 1% levels, respectively.

Table 4. Effect of Lottery Prize on Count of Neighborhood Bankruptcies (Single Year Event Windows)

Years Relative to Winning	(1) Postal Code Bankruptcies	(2) se	(3) DA – Postal Code Bankruptcies	(4) se
1	0.0064	(0.0055)	-0.0033	(0.0052)
2	0.0092	(0.0056)	-0.0033	(0.0055)
3	0.0129**	(0.0066)	0.0015	(0.0058)
4	0.0053	(0.0072)	-0.0003	(0.0062)
5	-0.0041	(0.0080)	0.0020	(0.0072)
0	-0.0037	(0.0054)	-0.0021	(0.0046)
-1	0.0019	(0.0053)	-0.0057	(0.0048)
-2	0.0075	(0.0050)	-0.0018	(0.0044)
-3	-0.0030	(0.0052)	-0.0043	(0.0046)
-4	0.0020	(0.0050)	-0.0047	(0.0044)
-5	-0.0033	(0.0047)	-0.0038	(0.0042)

Notes: This table reports the marginal effect of the log of the lottery prize on the count of bankruptcy in the winners' closest neighborhood (postal code) and outer neighborhood (DA excluding the postal code). Both bankruptcy numbers exclude winners' own bankruptcy filings. Postal code effects are estimated using Poisson models, and outer ring effects are estimated by OLS. OLS are used in outer rings 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 are in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

Table 5. The Heterogeneous Effect of Lottery Prize on the Bankruptcies of Winners' Neighbors (Extensive Margin)

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

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 medians of census variables. Urban vs. not urban definitions are based on metropolitan statistical areas. 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 6. Summary Statistics of Balance Sheet Data (Intensive Margins)

Variable	Obs.	Mean	Std. Dev.
Winning amount ($)	8747	6817	18665
Log of winning amount	8747	7.909	0.954
Winning year	8747	2010	3
Log asset value owned in bankruptcy balance sheet			
Cars	8747	5.929	3.999
Houses	8747	3.731	5.69
Motorcycles	8747	0.174	1.205
Recreational equipment	8747	0.47	2
Cash	8747	0.22	1.19
Furniture	8747	6.277	2.46
Financial assets	8747	2.411	3.777
Local and individual bankruptcy filer data			
DA Gini coefficient	8747	0.419	0.046
Population density (person per sq. km.)	8747	3016	3579
Region type (1 to 8 score)	8747	1.715	1.414
Filer's age (years)	8747	43	13
Household size (count)	8747	2.145	1.374
Divorced indicator	8747	0.143	0.35
Prior defaults indicator	8747	0.201	0.401
Filed after the 2009 reform indicator	8747	0.567	0.495
Self-employed indicator	8747	0.08	0.271
Individual reasons for bankruptcy (dummy)			
Overuse of credit (0 or 1)	8747	0.548	0.498
Marital breakdown (0 or 1)	8747	0.195	0.396
Unemployment (0 or 1)	8747	0.265	0.442
Insufficient income (0 or 1)	8747	0.332	0.471
Business failure (0 or 1)	8747	0.142	0.349
Health concerns (0 or 1)	8747	0.225	0.418
Accidents/emergencies (0 or 1)	8747	0.027	0.163
Student loans (0 or 1)	8747	0.006	0.075
Gambling (0 or 1)	8747	0.031	0.173
Tax liabilities (0 or 1)	8747	0.067	0.249
Loans cosigning (0 or 1)	8747	0.015	0.121
Bad/poor investments (0 or 1)	8747	0.033	0.178
Garnishee (0 or 1)	8747	0.012	0.109
Legal action (0 or 1)	8747	0.022	0.146
Moving/relocation (0 or 1)	8747	0.044	0.205
Substance abuse (0 or 1)	8747	0.023	0.149
Supporting relatives (0 or 1)	8747	0.079	0.269

Table 7. The Effect of Lottery Prize on the Balance Sheets of Neighboring Bankruptcy Filers (Multiple Year Event Windows)

Event Window (Years)	**0 to 2**	**3 to 5**	**-1 to -2**	**-3 to -5**
Cars	0.2142**	-0.0024	0.1291	-0.1042
	(0.0910)	(0.1149)	(0.0908)	(0.1317)
Number of observations	2,617	1,259	2,764	1,477
Houses	0.2714**	-0.0006	0.1245	0.1624
	(0.1285)	(0.1659)	(0.1204)	(0.1532)
Number of observations	2,617	1,259	2,764	1,477
Motorcycles	0.0573*	0.0031	0.0032	-0.0351
	(0.0293)	(0.0352)	(0.0286)	(0.0383)
Number of observations	2,617	1,259	2,764	1,477
Recreational equipment	0.0278	-0.0761	0.0654	-0.0357
	(0.0456)	(0.0665)	(0.0437)	(0.0636)
Number of observations	2,617	1,259	2,764	1,477
Cash	-0.0004	0.0048	0.0268	0.0307
	(0.0266)	(0.0352)	(0.0285)	(0.0388)
Number of observations	2,617	1,259	2,764	1,477
Furniture	-0.0245	0.1131	0.0998*	-0.0325
	(0.0532)	(0.0718)	(0.0548)	(0.0834)
Number of observations	2,617	1,259	2,764	1,477
Financial assets	-0.0975	-0.0599	0.0292	0.0804
	(0.0877)	(0.1160)	(0.0850)	(0.1204)
Number of observations	2,617	1,259	2,764	1,477

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 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 neighbor's lottery winning. Lottery size has no effect on the ownership of invisible consumption assets. Standard errors are in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

Table 8. 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	0.2384	0.0417	-0.2470	0.1805
	(0.1639)	(0.1704)	(0.1875)	(0.1950)	(0.2728)
Number of observations	878	700	563	436	260
Houses	0.0687	0.5508**	0.0265	-0.2953	0.1727
	(0.2337)	(0.2464)	(0.2690)	(0.2865)	(0.4012)
Number of observations	878	700	563	436	260
Motorcycles	-0.0092	0.1577**	0.0041	0.0031	0.0712
	(0.0544)	(0.0640)	(0.0639)	(0.0419)	(0.0919)
Number of observations	878	700	563	436	260
Recreational equipment	-0.0435	0.0676	-0.0948	-0.0846	0.0781
	(0.0836)	(0.0930)	(0.1137)	(0.1028)	(0.1676)
Number of observations	878	700	563	436	260
Cash	-0.0151	0.0157	0.0248	-0.0566	0.0709
	(0.0522)	(0.0454)	(0.0592)	(0.0604)	(0.0723)
Number of observations	878	700	563	436	260
Furniture	0.0074	-0.0164	0.1414	-0.0305	0.2731
	(0.0994)	(0.0908)	(0.1172)	(0.1180)	(0.1758)
Number of observations	878	700	563	436	260
Financial assets	-0.0711	-0.0332	-0.0093	-0.1193	-0.0942
	(0.1561)	(0.1724)	(0.1855)	(0.1965)	(0.2908)
Number of observations	878	700	563	436	260

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 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 neighbor's lottery winning. Lottery size has no effect on the ownership of invisible consumption assets. Standard errors are in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

Table 9. The Effect of Lottery Prize on the Balance Sheets of Neighboring Bankruptcy Filers (Single Year Event Windows, Before Winning)

Event window (years)	**0**	**-1**	**-2**	**-3**	**-4**	**-5**
Cars	0.1810	0.2737	0.0783	-0.0683	-0.0512	-0.0760
	(0.1528)	(0.1736)	(0.1588)	(0.2166)	(0.2179)	(0.2937)
Number of observations	1,039	863	878	646	502	329
Houses	0.1615	0.3229	0.0597	0.1665	0.3361	0.0374
	(0.2117)	(0.2342)	(0.2018)	(0.2733)	(0.2368)	(0.2950)
Number of observations	1,039	863	878	646	502	329
Motorcycles	0.0376	-0.0210	-0.0104	-0.0404	-0.0312	-0.0834
	(0.0414)	(0.0649)	(0.0439)	(0.0699)	(0.0623)	(0.0686)
Number of observations	1,039	863	878	646	502	329
Recreational equipment	0.0665	0.1129	0.0456	0.0088	0.0309	-0.1472
	(0.0708)	(0.0839)	(0.0686)	(0.1159)	(0.1055)	(0.1063)
Number of observations	1,039	863	878	646	502	329
Cash	-0.0030	0.0670	-0.0089	-0.0363	0.0416	0.1264
	(0.0437)	(0.0503)	(0.0454)	(0.0637)	(0.0569)	(0.1026)
Number of observations	1,039	863	878	646	502	329
Furniture	-0.0574	0.1307	0.0768	-0.1320	-0.0089	0.1029
	(0.0912)	(0.1049)	(0.0882)	(0.1453)	(0.1380)	(0.1663)
Number of observations	1,039	863	878	646	502	329
Financial assets	-0.0852	0.1151	-0.0531	0.4908**	-0.1355	-0.3247
	(0.1449)	(0.1655)	(0.1413)	(0.2020)	(0.2059)	(0.2530)
Number of observations	1,039	863	878	646	502	329

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 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 neighbor's lottery winning. Standard errors are in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

Does Keeping Up with the Joneses Cause Financial Distress?

Evidence from Lottery Winners and Neighboring Bankruptcies

Sumit Agarwal, Vyacheslav Mikhed, and Barry Scholnick*

Online Appendix

Appendix 1: Effect of Lottery Wins on the Winners' Own Bankruptcies

Even though our focus in this paper is on the effect of lottery winners on the bankruptcy filings of *neighbors*, in this Appendix, we report results from regressions examining the effect of lottery winnings on the winners' *own* bankruptcy filings, which is very similar to the tests run by Hankins et al. (2011). These models use data from all individual winners, with the log of lottery win size being the main exogenous independent variable of interest. The dependent variable is a binary indicator capturing whether or not a winner filed for bankruptcy in various event windows. 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 A1, and test results are presented in Table A2. Table A2, 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 A2 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 Panels B and 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.

Table A1. Summary Statistics for Individual Lottery Winners and Winners' Own Bankruptcy

Variable	Obs.	Mean	Std. Dev.
Log of winning amount	18012	7.879	1.015
Winning year	18012	2009	3
Own bankruptcy rate relative to the lottery date years			
0 to 2	10749	0.006	0.078
3 to 5	4412	0.005	0.067
-1 to -2	17451	0.005	0.071
-3 to -5	18012	0.005	0.069
Neighborhood characteristics:			
DA Gini coefficient	18012	0.424	0.048
Median income ($)	18012	30532	8079
Population density (person per sq. km.)	18012	2342	3005
Region type (1 to 8 score)	18012	2.116	1.788
Unemployment rate (%)	18012	4.177	3.797
Numerical literacy score (between 100 and 500)	18012	276	11
Divorced (proportion of DA population)	18012	0.078	0.032
Separated (proportion of DA population)	18012	0.028	0.015
Widowed (proportion of DA population)	18012	0.05	0.049
High school (proportion of DA population)	18012	0.238	0.066
Apprenticeship (proportion of DA population)	18012	0.13	0.06
College (DA) (proportion of DA population)	18012	0.203	0.065
University (DA) (proportion of DA population)	18012	0.169	0.101
Graduate (DA) (proportion of DA population)	18012	0.053	0.056
Homeownership	18012	0.387	0.082
Male	18012	0.5	0.03
Age between 20 and 39 years	18012	0.295	0.098
Age between 40 and 64 years	18012	0.333	0.068
Age over 65 years	18012	0.113	0.094

Table A2. The Effect of Lottery Winning on Winners' Own Bankruptcy

Event Wi**ndow (Years)**	**0 to 2**	**3 to 5**	**-1 to -2**	**-3 to -5**
Panel A. Single-winning postal codes, controls included				
Log of winning amount	-0.0028	0.0033	0.0007	-0.0018
	(0.0030)	(0.0021)	(0.0010)	(0.0013)
Number of observations	4,018	1,121	6,790	7,165
Panel B. All winners, controls included				
Log of winning amount	-0.0011	0.0016*	-0.0008	-0.0003
	(0.0011)	(0.0010)	(0.0007)	(0.0005)
Number of observations	10,658	4,019	17,447	18,012
Panel C. All winners, controls excluded				
Log of winning amount	-0.0011	0.0016*	-0.0009	-0.0003
	(0.0011)	(0.0010)	(0.0007)	(0.0005)
Number of observations	10,658	4,019	17,447	18,012

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.