Bankruptcy Spillovers:

Distance, Public Disclosure and Opaque Information

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We examine whether individuals are influenced to file for bankruptcy by the past bankruptcies of their close neighbors. We find that a one standard deviation increase in neighborhood bankruptcies increases the probability of defaulting via bankruptcy rather than via credit card charge-off by 6%. Our individual level data on defaulters is from detailed credit card accounts, which is matched to neighborhood level data on the geographic location of every bankruptcy filer. Our data allows us to employ two separate identification strategies based on neighborhood geography and bankruptcy/default law. Our results are consistent with the bankruptcy policy arguments of pro-creditor lobbyists.

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An increasingly important issue in finance is how the transmission of opaque information within social or geographic networks, impacts financial decisions. In this regard, Wurgler (2012) highlights the importance of the “broader synthesis of social sciences concepts and financial economics” (p. 227), where two of the social sciences concepts he lists include “social communication” and “geographic accident”. This paper examines these issues in the specific context of the financial decision to file for personal bankruptcy.

The existing personal bankruptcy and default literature (e.g. Fay, Hurst and White, 2002, Gross and Souleles, 2002, Dick, Lehnert and Topa, 2008, Cohen-Cole and Duygan-Bump, 2009, White, 2011, Agarwal, Chomsisengphet, Liu, 2011, Guiso, Sapienza and Zingales, 2012) has argued that past bankruptcies in an individual’s neighborhood can influence the individual to file for bankruptcy. It is usually argued that these effects can operate either through stigma (where an awareness of other individuals in a social network who have previously filed for bankruptcy lowers stigma), or learning (where interactions with previous bankruptcy filers leads to learning about the legal process involved in a bankruptcy filing). The justification for this relationship is provided by Fay, Hurst and White (2002) who argue that “if households live in a district with a higher bankruptcy filing rate, then they are more likely to hear firsthand about bankruptcy from friends or relatives because the latter are more likely to have filed...This information will tend to make households more comfortable with the idea of bankruptcy, so the level of bankruptcy stigma falls” (p. 710). Similarly Gross and Souleles (2002) argue that "social stigma and information about bankruptcy might change, with the number of people in one's community, appropriately defined, that have already filed for bankruptcy." (p. 339).

The key issue in any test of the influence of neighbors on individuals is the issue of endogeneity. Endogeneity can arise because individuals with certain preferences or characteristics can self-select to locate in the same neighborhood, which in turn influences the choices of individuals in the neighborhood. Similarly, an unobservable neighborhood specific shock (e.g. a local plant closure) can also influence choices by both individual’s and their close neighbors.

In this paper, we simultaneously employ two different identification strategies to control for endogeneity; the first based on geography and the second based on legal institutions. We are able to undertake these identification strategies because we use a high quality new Canadian database that matches individual level data taken from individual credit card accounts, with neighbourhood level data on the geographic location, and date, of every Canadian bankruptcy filer, provided by the Canadian bankruptcy regulator. We can thus observe detailed bank account level data of individual bankrupts, as well as all past bankruptcies in the individual's neighbourhood, where the neighbourhood is a geographic area as small as a single city block.

Our first identification strategy exploits our very fine grained geographic data using a methodology proposed by Grinblatt, Keloharju and Ikaheimo (2008), in their study of how neighbors influence individual consumption. Grinblatt, Keloharju and Ikaheimo (2008) define near neighbors (or inner-rings), and distant neighbors (or outer-rings) for each individual. For example, near neighbors can be thought of as those living in the same city block as the individual, while distant neighbors can be thought of as those living in the same suburb as the individual. The Grinblatt, Keloharju and Ikaheimo (2008) methodology involves creating a new independent variable defined as the number of (in our case) bankruptcies in the near neighborhood (inner-ring) minus the number of bankruptcies in the distant neighborhood (outer-ring), weighted by the number of households in each ring. This variable is regressed on the individual's choice of filing for bankruptcy, and the estimated coefficient captures the neighborhood spillover effect of the near inner-ring neighbors on the individual.

The idea behind this approach is that the near neighborhood spillover effect will be larger the greater the impact of near inner-ring neighbors on the individual, relative to the size of the impact of distant outer-ring neighbors on the individual. The econometric intuition behind this approach is that the subtraction of outer-ring neighbor effects from inner-ring neighbor effects controls for omitted common attributes that are shared by residents of *both* the inner- and outer-rings. This allows us to control for

possible endogeneity that could arise from the self-selection of individuals with common attributes into neighborhoods, at the level of the outer-ring neighborhood.

This general approach of controlling for omitted common neighborhood level attributes by comparing outer-ring neighbors with inner-ring neighbors has become increasingly common in studies, like ours, with access to very fine grained geographic data. In addition to Grinblatt, Keloharju and Ikaheimo (2008), other recent examples include Linden and Rockoff, (2008), who examine the impact of sex offenders within neighborhoods, Campbell, Giglio and Pathak (2011), who examine the impact of forced house sales within neighborhoods, and Pope and Pope (2012) who examine the arrival of Walmart stores into neighborhoods. These studies are methodologically similar to ours in that they all use specifications that compare inner- and outer ring-neighbors. Our study, however, is the first to use this methodology in the bankruptcy context.

This paper extends this literature by simultaneously implementing a second identification strategy based on legal institutions, in addition to the inner- and outer-ring neighborhood strategy. The main insight behind this second, institutional, approach is that there are two separate legal mechanisms by which an individual can choose to default on credit card debt; (1) bankruptcy and (2) credit card charge-off. We describe these legal institutions in full below, but in brief, both bankruptcy and charge-off provide both benefits and costs to individual defaulters, which will impact the choice between them. Bankruptcy, by law, is publically disclosed through the court systems, whereas credit card charge-off is not publically disclosed, thus affording more privacy to the defaulter. On the other hand, an advantage of bankruptcy is that the bankrupt is no longer liable for unsecured debt (e.g. credit card debt), whereas charge-off still leaves the defaulter liable for that debt. In summary; both bankruptcy and charge-off constitute default, but charge-off affords the defaulter more privacy, while bankruptcy allows non-payment of credit card debt.

We argue that an unobservable neighborhood level income shock (e.g. plant closure) will lead to an increase in financial distress and thus default in the neighborhood. Once the distressed individual decides to default, however, the individual must then *choose* to default either via bankruptcy or via charge-off based on the trade-offs between them (charge-off affords more privacy, while bankruptcy allows non-payment of credit card debt). We argue that while an unobservable shock can cause financial distress and thus default, there is no reason for why the unobservable shock (plant closure) should impact the *choice* that defaulters have to make as to the legal mechanism for default (between bankruptcy and charge-off). Our strategy, therefore, is to limit our main sample *only* to individuals who have defaulted (bankrupts plus charge-offs), and test which defaulters choose to default via bankruptcy and which choose to default via charge-off. To the best of our knowledge, this paper is the first in the bankruptcy literature to implement this strategy. While this paper examines which *defaulters* choose bankruptcy, previous papers in the bankruptcy spillover literature (e.g. Fay, Hurst and White, 2002, Gross and Souleles, 2002) have examined which individuals in the *population at large* choose bankruptcy (i.e. examining the choice between defaulting via bankruptcy and not defaulting at all). Under such a specification, it is indeed possible that unobserved local shocks could impact this choice.

Because we are uniquely able to observe both individual bankruptcies as well as individual charge-offs, we can develop and test new hypotheses as to the determinants of the bankruptcy/charge-off choice by defaulters. Our main test examines the impact of past neighborhood level bankruptcies on the defaulter's choice between bankruptcies and charge-offs. Our argument links the public/private distinction between bankruptcy and charge-off and information flows between neighbors.

We follow Fay, Hurst and White (2002) and Gross and Souleles (2002) in arguing that the greater the number of past bankruptcy filings in an individual's neighborhood, the greater will be the probability of a financially distressed individual defaulting via bankruptcy, either because of increased learning about bankruptcy or because of lower neighborhood stigma effects. Our data allows us to extend this argument by proposing and testing the new hypothesis that the lower the number of past bankruptcy filings in an

individual's neighborhood, the greater the probability of a financially distressed individual choosing to default via charge-off, which is not publically disclosed through the courts. Our argument is that financially distressed individuals for whom avoiding future stigma is important (because they live in a neighborhood with low levels of bankruptcy) will be more likely to choose a mechanism for default (charge-off) where there will be less future public disclosure about their default. In other words, we hypothesize that an individual who has few neighbors who have filed for bankruptcy will choose to default via charge-off rather than bankruptcy. This will lower the probability of the neighbors subsequently learning about the default, in spite of the disadvantages of charge-off (the fact that they are still liable for credit card debt).

Our results are consistent with these hypotheses. We find that a one standard deviation increase in neighborhood bankruptcies increases the probability of defaulting via bankruptcy rather than via charge-off by 6%. Our results are consistent with Fay, Hurst and White (2002), Gross and Souleles (2002) and Guiso, Sapienza and Zingales (2012) in that we find significant neighborhood spillovers when the default is publically disclosed (e.g. bankruptcy or mortgage foreclosure). Our main new contribution, however, is to show for the first time that neighborhood spillover effects also impact the *choice* the individual defaulter makes as to whether to default using a procedure that is publically disclosed through the courts (in our case bankruptcy), or not publically disclosed (in our case charge-off).

Our data also allows us to compare bankruptcy spillovers across neighborhoods with different characteristics. The bankruptcy spillover hypothesis predicts that spillovers should be greater in neighborhoods with greater social interactions between individual neighbors. We find that bankruptcy spillover effects are larger in (1) rural compared to urban neighborhoods; (2) lower income compared to higher income neighborhoods; and (3) lower income dispersion compared to higher income dispersion neighborhoods. In all of these cases it is possible to provide arguments for why spillovers are being driven by the extent of social interactions among neighbors.

It is important to note that our paper follows much of the spillover literature (Grinblatt et al, 2008, Fay, Hurst and White, 2002 Gross and Souleles, 2002, and Guiso, Sapienza and Zingales, 2012) in not attempting to determine the exact reason for spillovers between neighbors and individuals (e.g. differentiating between stigma effects, information flows, learning etc.). Rather we only document that such spillovers occur.

This paper forms part of a rapidly growing literature which examines a variety of possible causes of bankruptcy, including (but not limited to) Domowitz and Sartain (1999), Dick and Lehnhert (2010), Livshits, MacGee and Tertilt, (2010), Hankins, Hoekstra and Skiba (2011) and Gross and Notowidigdo (2011) as well as the papers cited above. The focus in this paper on the relationship between the transmission of opaque information and geographic distance is also related to a series of papers, in various contexts. In the context of bank lending, these papers include Petersen and Rajan (2002), Degryse and Ongena (2005), Hauswald and Marquez (2006), and Agarwal and Hauswald (2010). Distance has also been related to opaque information by Hau (2001) and Coval and Moscowitz (1999 and 2001) in the context assert trading, by Butler (2008) in the context of municipal bonds, and by Garmaise and Moskowitz (2004) in the context of commercial real-estate.

1. POLICY MOTIVATION

An important motivation for this paper is that the issue of bankruptcy spillovers through stigma and information effects has played a key role in policy debates over bankruptcy regulation, including the debate over bankruptcy reform in the US in 2005. Broadly speaking, pro-creditor lobbyists (e.g. the credit card industry) have tried to make the case that bankruptcy has become more prevalent because of spillovers (caused by lower stigma and increased information about the process of bankruptcy), and thus argue for greater restrictions on the ability of debtors to file. On the other side, pro-debtor lobbyists (e.g. consumer rights groups) have tried to make the case that bankruptcy filing should be made easier, because

negative shocks to individuals (unemployment, divorce, health shocks) rather than bankruptcy spillovers are the main reason for increased individual bankruptcy.

The importance of stigma effects and spillovers between individuals in these policy debates can be seen by the comments of various US Senators, of both parties, in 2005. These include: "Bankruptcy should be difficult, and the moral stigma that used to be associated with bankruptcy ought to be resurrected." (Senator Grassley); "The explosion in bankruptcy filings has less to do with causes and more to do with motivations. The stigma of bankruptcy is all but gone." (Senator Hatch); "There has been a decline in the stigma of filing for bankruptcy and appropriate changes are necessary to ensure that bankruptcy is no longer considered a lifestyle choice." (Senator Kerry); "The social stigma of bankruptcy is gone" (Senator Dodd)). At the same time, then Federal Reserve Chair Greenspan commented that "Personal bankruptcies are soaring because Americans have lost their sense of shame." (all citations from Efrat (2006, p. 486)).

Much of this policy discussion on bankruptcy stigma and spillover effects was based on evidence provided by Gross and Souleles (2002) and Fay, Hurst and White (2002). Both these papers found that higher levels of bankruptcies in an individual's social network resulted in the higher probability of a bankruptcy filing, and both attributed these spillovers to either stigma or information effects. However, an important methodological concern in these two papers is the large size of the individual's "social network" usually because of data limitations. For example, Gross and Souleles (2002) capture social networks by using data on aggregate bankruptcies as measured at the level of US States, while Fay, Hurst and White (2002) capture social networks by using aggregate bankruptcies as measured at the level of US Bankruptcy court districts, of which there are 94 in the US. Both of these geographic measures of social networks thus include many millions of individuals. Gross and Souleles (2002) acknowledge this concern, and comment, that "of course, the relevant community within which stigma operates and information flows might not be one's state, so these results can be considered a lower bound for their effect" (p. 340).

They thus conclude that their finding is "*suggestive* of a decline in social stigma or information costs, *but is not conclusive*" (italics added p. 345).

This methodological concern played an important role in the 2005 policy debates, and was specifically addressed by Sullivan, Warren[1] and Westbrook (2006. p. 217). These "pro-debtor" authors strongly criticized the spillover findings of Fay, Hurst and White, (2002) and Gross and Souleles, (2002), based on their defining of "social networks" as US states or court districts. It is for this reason that our, more rigorous, tests of the bankruptcy spillover hypothesis have important policy implications. Our data, allows us to specifically address the criticism of Sullivan, Warren and Westbrook, 2006 by testing the bankruptcy spillover hypothesis, where the individual's social network is defined as near neighbors in areas as small as city blocks[2].

2. CANADIAN INSTITUTIONAL BACKGROUND

In order to examine the relationship between individual defaulters and previous bankruptcies in the individual's neighborhood we require individual level data matched to neighborhood level data. To do this we use data derived from the matching of two unique databases, both of which are provided to us confidentially. First, we use individual level monthly credit card account data provided to us by an individual Canadian bank. The data contain information on a large number of individual credit card account holders, a small fraction of whom have either filed for bankruptcy or who have had their credit cards charged-off. We can identify the date of each bankruptcy and charge-off in the data. This data also include the Canadian six digit postal code of each individual. These postal codes are extremely small geographic areas containing approximately 13 households on average, and often consisting of less than a single city block. These individual credit card account level bankruptcy and/or charge-off data are our dependent variable(s).

[1] Warren subsequently played a key role in the creation of the US Consumer Financial Protection Bureau.
[2] Cohen-Cole and Duygan-Bump, 2009, also attempt to examine bankruptcy spillovers in very small neighbourhoods, but their credit bureau data is subject to other concerns, such as only being able to determine that a bankruptcy filing occurred at some stage in the previous seven years.

Our second data base contains the counts of every past insolvency filing in every Canadian six-digit postal code (neighborhood) in every year. This second, neighborhood count database, was provided to us uniquely by the Office of the Superintendent of Bankruptcy (OSB), the Canadian bankruptcy regulator, and is a result of special runs of the OSB data extraction system, conducted specifically for this research project. These neighborhood level counts are used to construct our main independent variable of interest. We are able to match these two databases using the six digit post code.

The remainder of this section provides institutional background in Canada, while the following section describes these data in more depth.

2.1. Bankruptcy and Credit Card Charge-off

Legally, bankruptcy and charge-off are the two mechanisms whereby a credit card contract can be terminated with unpaid balances remaining, thus both constitute formal default. Default is different from delinquency, which occurs when there are late payments, because under delinquency the card contract is not legally terminated and the individual is still able to use the card. The specific choice between default via bankruptcy, and alternatively, default without bankruptcy is discussed by White (2011) in her survey of the institutional details surrounding default. She writes that "the main punishments for bankruptcy are making filers' names public ..which...stigmatize the bankruptcy filers"[3]. On the other hand, the "punishments for debtors who default but do not file for bankruptcy, include(s) credit collectors calling them, suing them, and garnishing their wages."(White, 2011, p.2). Defaulters thus face a trade-off when choosing between bankruptcy and charge-off. Bankruptcy entails increased public disclosure, but under bankruptcy all outstanding unsecured debts (e.g. credit card debt) can be written off, and all recovery actions by creditors are stayed (stopped). Charge-off entails reduced public disclosure, but under charge-off creditors are able to continue actions to recover debt through wage garnishment and other actions.

[3] Sullivan, Warren and Westbrook (2006, p. 242) argue that "most people want to conceal the fact of their bankruptcy filings from at least some of their families, coworkers, friends, and neighbors." They cite a 2001 survey showing that 84.3% of bankruptcy filers "would be ‘embarrassed’ or ‘very embarrassed’ if their families, friends, or neighbors learned of their bankruptcy."

Public disclosure of every bankruptcy filing in Canada is provided through the court system, and in addition is provided on a single Government of Canada web page. A simple web search can thus reveal the name of every Canadian bankruptcy filer. This is not true for credit card charge-offs, where there is no legal requirement that information on this kind of default be publicly disclosed, either through the courts or on a web page. While information about both bankruptcy and credit-card charge-off appear on the defaulter's credit rating (e.g. FICO score), the distinction we exploit here concerns the public disclosure of the default to those without access to credit ratings, i.e. the defaulter's broader social network (e.g. neighbors), from where opaque neighborhood information effects are assumed to flow.

We argue in this paper that an individual defaulter has the choice as to whether to default via bankruptcy or via credit card charge-off. Legally, the individual debtor always has the choice as to whether to file for bankruptcy. In addition, we use specific institutional details about credit card charge-off procedures used by the bank that provided us with the data to argue that credit card charge-off at this specific bank is also in effect a choice made by the individual debtor. The procedure used by this bank is that every credit card account that reaches 120 days delinquent is sent a formal letter informing the debtor that the account will be charged-off and declared in default at 180 days delinquency. The key institutional detail is that these actions by the bank are automatic and not discretionary. In other words, because the individual is made aware at 120 days that the account will be charged-off at 180 days, we argue that by not taking alternative action before 180 days (either paying off the outstanding amount or alternatively filing for bankruptcy) the individual is in effect choosing to default via credit card charge-off.

2.2. Neighborhoods and Households in Canada

A key advantage of both the neighborhood and individual level data we use is that it can be matched to the Canadian postal system. Every Canadian postcode consists of six digits.[4] There are more

[4] The Canadian six digit postcode system is similar in many respects to the Dutch six digit postcode system, which formed the basis of the empirical research in Kuhn, Kooreman, Soetevent and Kapteyn (2011).

than 840 thousand individual six digit postcodes in Canada, and the median number of households in each six digit postcode is approximately 13. In this paper we designate these six digit postcodes as the "near neighbors" or "inner-ring neighbors" in terms of the methodology proposed by Grinblatt, Keloharju and Ikaheimo (2008).

Under the Canadian post code system, each six digit post code can be aggregated geographically to a larger area reflected by its first three digits. These three digit postcode areas are called Forward Sortation Areas (FSA). All six digit postcodes with the same first three digits form a single FSA. This is not, however, true for other levels of aggregation such as the first four or first five digits of the six digit postcode. There are approximately 1500 FSAs in Canada, with a median of approximately 7800 households. Intuitively, these FSAs can be thought of as a suburb. In this paper we designate these three digit FSAs as "distant neighbors" or "outer-ring neighbors" in terms of the methodology proposed by Grinblatt, Keloharju and Ikaheimo(2008).

3. DATA

Below we provide details on the different databases we use. Summary statistics are provided in Table 1.

3.1. Bank Account Data on Bankruptcies and Charge-Offs (Dependent Variable(s))

The first database we consider is individual monthly credit card account data, provided to us confidentially by an individual Canadian bank. The credit card account level data are measured monthly from Dec 2004 to June 2006. This credit card data is similar in structure to previous bankruptcy stigma research conducted by Gross and Souleles (2002), with the important difference that our data flags two separate kinds of individual default - bankruptcy and credit card-charge-off [5]. These individual

[5]Gross and Souleles (2002) do examine both bankruptcy as well as three month credit card delinquency (which is not a default because the card contract is not terminated) as dependent variables , but, they do not include the lagged

bankruptcies and charge-offs are the main binary dependent variables in our logit specifications below. The full database contains credit card account details for approximately 93 000 individuals. In this data we can observe 119 individual bankruptcies and 362 individual credit card charge-offs[6].

We argue that bankruptcy spillovers because of stigma and information flows between neighbors are low frequency phenomena that are likely to build up over multiple years, rather than over few months. It is for this reason that we define an individual as choosing bankruptcy or charge-off if that individual has made that choice at any time during the 19 month period of our credit card account database. For this same reason (low frequency of spillovers between neighbors) we define neighborhood bankruptcies as those occurring in the five year period before the individual defaults (as described below).

The credit card database contains various bank determined measures of the individual's risk of default including the FICO score, the credit card credit limit and the APR of the credit card. It also contains details from the monthly credit card statement, including monthly balance outstanding. We divide balance outstanding by the card credit limit to determine the utilization rate. A particular concern with the FICO score data (which is also faced by Gross and Souleles (2002) is that the FICO data is missing for many individuals in the database who have defaulted or are close to default. This is particular for those who are in the charge-off sample. The summary statistics in Table 1 show that out of 362 charge-offs in our data we only have FICO scores on 203 individuals. In this paper we run our main tests without including FICO scores, but as a robustness test we rerun all our tests including FICO scores. Our main results are robust across theses specifications.

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geographic bankruptcy rate (US State level data) as an independent variable in their three month delinquency models.

[6] This implies that we can observe a bankruptcy rate of 1.28 bankruptcies per 1000 credit card account holders in our sample. This compares with a total Canadian bankruptcy rate of 2.7 bankruptcies per 1000 total population in 2004 (as measured by the Office of the Superintendent of Bankruptcy). Our sample only includes credit card holders rather than a count of the total population , which can explain why the bankruptcy rate in our credit card sample is smaller (but of similar orders of magnitude) to the bankruptcy rate in the entire Canadian population. As a comparison the bankruptcy rate in the United States in 2004 was 7.7 bankruptcies per 1000 population.

Our credit card account level database also contains data on whether mortgage debt is owed by those individuals to that specific bank. This linked credit card account and mortgage account data is particularly valuable to us because it allows us to control for the possibility that issues relating to mortgage debt could be impacting the choice between defaulting via bankruptcy or via credit card charge-off.

The structure of this database is similar to the data used by Gross and Souleles (2002) whose monthly data "are followed ...until they *first* default" (italics added p. 326). Similarly, in our data, in the months prior to a bankruptcy/charge-off the data show the individual's monthly credit card activity; in the actual month of the individual's default the data show either a bankruptcy flag or a charge-off flag; and in subsequent months all the credit card data for that individual is empty, because the credit card contract has been terminated. Our dependent variable thus reflects the choice by the defaulter as to which of bankruptcy or charge-off occurred first.

The actions the bank is legally allowed to take following a bankruptcy filing are very different from the actions the bank is legally allowed to take following a charge-off. After charge-off the bank typically sells the outstanding credit card debt to a collection agency. On the other hand, under Canadian bankruptcy law, once bankruptcy is filed any attempts by any creditor (e.g. the bank) to claim on any unsecured (e.g. credit card) debt have to be stayed (stopped). The implication of this sharp distinction is that the bank has a strong incentive to accurately capture in its records, which of bankruptcy or charge-off occurred first. This is what is captured in our data.

In this paper we follow essentially the same procedure of Gross and Souleles (2002) which is to measure all of these monthly bank account variables only at the first month in the data set[7]. The main reason for this is to control for possible endogeneity between default (e.g. bankruptcy) and monthly credit

[7] While Gross and Souleles (2002) only use the first observed month of this data in their baseline specifications, they allow this to change in subsequent specifications.

card behavior in the months leading up to the default. For example, individuals who are planning to declare bankruptcy may have an incentive to max out their credit card prior to bankruptcy. Because we only include data for these credit card control variables from the first observable month, we do not explore the monthly dynamics of these bank account variables. However, the main focus of this paper is on capturing low frequency spillovers between neighbors (through stigma or information effects) using five yearly summations of OSB neighborhood count data.

We argue that the timing convention of our data (i.e. being able to observe the choice of the defaulter as to whether to *initially* default via bankruptcy or charge-off) is advantageous to us, in that it allows us to control for issues that may impact strategic interactions between defaulters and creditors that occur in subsequent periods *after* the initial default. The period after the initial default often involves the use of various negotiating and legal strategies between debtors and creditors, attempting to maximize their advantage. These strategic interactions are discussed and modeled by authors such as White (1998a), White (1998b), Dawsey and Ausubel (2004), Dawsey, Hynes and Ausbel (2009), Chatterjee, (2011) and Benjamin and Mateos-Planas (2011). We argue, however, that because our credit card account level data all reflect the choice of the defaulter as to whether to *initially* default via bankruptcy or charge-off, issues related to possible strategic interactions between debtors and creditors in the subsequent time periods after default, will not be captured in our data. Our data thus enables us to tests the specific hypothesis that opaque neighborhood spillovers impacts the initial choice of whether to default via bankruptcy or charge-off.

3.2. Neighborhood Level Bankruptcy Count Data (Independent Variables)

The main independent variables in our tests below are counts of annual consumer bankruptcies in each Canadian six-digit postal code provided to us uniquely by the Office of the Superintendent of Bankruptcy Canada (OSB). We are able to aggregate up these counts to three digit FSA level, thus we can examine both near neighbor (inner ring or city block) as well as the distant neighbor (outer ring or suburb)

level bankruptcy counts, measured annually. In addition to the advantages described above, there are at least three additional advantages of our aggregate geographic data relative to the data used in the literature.

First, an important concern with measuring social interactions using aggregate bankruptcy data from US states (as in Gross and Souleles, 2002) or bankruptcy court districts (as in Fay, Hurst and White, 2002) is that it is difficult to disentangle differences in legal and/or administrative processes across these jurisdictions from differences in neighborhood effects across these jurisdictions. Fay, Hurst and White (2002) acknowledge this issue by arguing that "a significant coefficient on the lagged bankruptcy filing rate in the (US bankruptcy court) district could reflect local differences in the level of bankruptcy stigma *or* local differences in the administration of bankruptcy law... *or* could reflect the influence of information cascades" (italics added p. 710).In our paper we are able to disentangle state or court district legal/administrative procedures from neighborhood effects (caused by stigma or information flows) because our data are all from a single large Canadian province. We are able to do this because our primary unit of geographic space is the six-digit postal code, of which there are many thousands in this particular province. There are indeed legal differences in the administration of bankruptcy *across* the Canadian provinces, but there are no legal or administrative differences *within* a province. Because our basic unit of geographic analysis is the six-digit (or three digit) postal code within a single Canadian province, we argue that all individuals in our study face the same legal and administrative environment when filing for bankruptcy.

Second, a common issue facing studies relating individual behavior to social networks (neighborhoods) is the so called "reflection problem" of Manski (1993). This relates to the possible simultaneous interrelationship between the individual and the neighborhood. Our data allows us to address this issue in the same manner as Gringlatt et al (2008). Grinblatt, Keloharju and Ikaheimo (2008) emphasize the importance of examining the *lagged* actions of neighbors relative to the actions of the individuals. This is because "*lagged* actions (of neighbors) are not plausibly affected" (p. 736) by

individual behavior (in our case, bankruptcy filing). Our credit card data provides us with the exact month of each individual's delinquency[8], and our neighborhood count OSB data provides us with annual data on bankruptcy filings per neighborhood. We are thus able to ensure that all our neighborhood level data (independent variables) are lagged relative to the individual's default date (dependent variable).

Third, our data provides an exact count of aggregate bankruptcy filings in each postal code in each year (from our OSB data). The issue of measurement error in neighborhood bankruptcy counts is of particular importance because we are dealing with very small neighborhood areas, where there are typically very few bankruptcy filings in a given year. Because annual bankruptcy totals in each neighborhood are so small, any inaccuracies in this count can have large implications on subsequent empirical models. Our OSB data on aggregate insolvency filings per Canadian six-digit postal code (neighborhood) is a complete count of every insolvency in Canada[9] and is thus not subject to this measurement error[10].

3.3. Neighborhood Level Controls

We are also able to match our post code data with Canadian Census data, measured at a geographical area known as the dissemination area (DA). The DA is bigger than a six digit post code and smaller than a three digit FSA[11]. Matching postal code level and DA level geographic areas is quite common in research using Canadian data, and is undertaken using a conversion file developed by both Statistics Canada and Canada Post. This allows us to include a large variety of DA level census data as

[8] This is different from the credit report data used by Cohen-Cole and Duygan-Bump (2009) which show whether an individual has filed for bankruptcy at some stage in the previous 7 years, rather than showing the exact timing of the bankruptcy filing.

[9] Our OSB data by design only include primary filers rather than secondary estates (for example joint filings by separated spouses or other related individuals who could live in separate postal codes). In other words, each filing is allocated to the postal code of the primary filer, and each filing in the data is only counted once.

[10] Cohen-Cole and Duygan-Bump (2009), for example, calculate the sum of bankruptcies within a neighborhood by aggregating from the files of a single credit bureau, which holds credit files on approximately one ninth of all individuals with a credit history. This gives an incomplete count of total bankruptcies in the neighborhood.

[11] There are approximately 55 thousand DAs in Canada (compared to 840 thousand six digit postal codes and 1500 FSAs).

control variables in our regressions, including median family income, family income standard deviation, population without income, and unemployment rate. All this data is derived from the 2006 Canadian census

There is a large literature linking issues such as bankruptcy with levels of financial literacy (see e.g. Lusardi, 2012 and many others). Furthermore, Lusardi, (2012) argues that a central element of financial literacy is numeracy - i.e. the capacity to conduct relatively complex calculations. A unique feature of this paper is that we can employ a measure of numerical literacy, measured at the very disaggregate DA geographic level, to control for the possible influence of financial literacy on personal bankruptcy. Our numerical literacy data were developed by Murray (2011)[12]. This variable is computed using the 2003 International Adult Literacy and Skills Survey (IALSS) and the 2006 census. IALSS evaluated numerical skills for a very large sample of the Canadian population as well as collecting various demographic data. The average level of numerical literacy for each DA was estimated, based on the demographic characteristics of that DA. DA level numerical literacy scores vary from 100 to 500, where higher levels correspond to higher levels of numerical literacy.

3.4. Number of Households in Each Six Digit and Three Digit Postal Code

An important element of the Grinblatt, Keloharju and Ikaheimo (2008) methodology (described in full below) is the need to determine the number of households in both six as well as three digit postal codes (i.e. near and distant neighborhoods). A full account of the number of households in each three digit postal code (FSA) is provided by Statistics Canada. However, such a list does not exist for six digit postal codes, largely because of their very small nature. Nevertheless, household counts do exist at slightly higher levels of geographic aggregation, known as dissemination blocks (DBs) and dissemination areas (DAs). Dissemination Areas (DAs) are described above, while dissemination blocks (DBs) are a level of

[12]We are grateful to Scott Murray for providing us with these data.

aggregation that is smaller than DAs, but larger than 6 character postal codes. There are approximately 5 postal codes in each DB. For about 85% of 6 digit postal codes in Canada, we are able to match the postal code to a DB. For those codes we take the total number of households in the DB and divide by the number of postal codes in the DB to get a DB level average number of households per postcode. For the remaining 15% of postcodes, we repeat the process using the slightly larger DA level of aggregation. We can thus generate a number of households for every six digit postcode in Canada.

3.5. Census Metropolitan Areas

Data from Statistics Canada classifies each six digit postal code into whether it forms part of a Census Metropolitan Area or not. We are thus able to classify our data as whether it is urban or non-urban/rural. As an extension to our main tests therefore, we can re-estimate our neighborhood spillover models for urban and rural areas.

4. UNIVARIATE TESTS

Before conducting formal econometric tests, we explore our data by providing simple univariate tests of comparisons between different neighborhoods[13]. Our bankruptcy spillover hypothesis implies that the neighborhoods of individuals who file for bankruptcy should have higher levels of aggregate bankruptcy compared to the neighborhoods of the individuals who had their credit cards charged-off. We thus define two types of neighborhood, (1) those neighborhoods where the 119 individual credit card holders who filed for bankruptcy in 2004-2006 live (which we label "bankruptcy neighborhoods"), and (2) those neighborhoods where the 361 individual credit card holders who had their credit cards charged-off in 2004-2006 live (which we label "charge-off neighborhoods"). We test whether there are more aggregate bankruptcies in the bankruptcy neighborhoods, compared to the charge-off neighborhoods, using annual comparisons of means t tests. We run these tests over OSB neighborhood count data from each of the eight individual years from 2000 to 2007. By running these tests for all individual years in our

[13]These univariate tests obviously show correlation rather than causation.

OSB neighborhood level database (i.e. from 2000 to 2007) we can examine if there are systematic differences in these neighborhoods before, during and after the years where we can observe the individual defaulters (2004 to 2006).

The results of these differences in mean tests are provided in Table 2. Each line in Table 2 provides data on the mean OSB bankruptcy counts in the 119 "bankruptcy neighborhoods" and the 361 "charge-off" neighborhoods". For example, the first line of data in Table 2 shows that in the year 2000, there was a mean of 2.722 bankruptcies in each of the 119 bankruptcy neighborhoods and a mean of 1.2493 bankruptcies in each of the 361 charge-off neighborhoods. The t test of the difference in these two means is highly significant in every year. These results are consistent with the bankruptcy spillover hypothesis because they show that there are systematic differences in the neighborhoods where individuals who chose bankruptcy live, compared to the neighborhoods where individuals who chose charge-off live.

5. ECONOMETRIC SPECIFICATIONS

Our methodology entails two separate identification strategies based on neighborhood geography and legal institutions. We discuss details of each in turn.

Our inner- and outer-ring neighborhood geography approach follows closely to that of Grinblatt, Keloharju and Ikheimo (2008)[14]. We follow Grinblat et al (2008) in using a logit specification where the dependent variable is one of the possible choices made by the individual (e.g. the choice between bankruptcy or charge-off).

[14] We argue that it is appropriate to use this Grinblatt, Keloharju and Ikaheimo (2008) net neighborhood econometric methodology to test the neighborhood spillover hypotheses developed by Fay, Hurst and White, 2002, Gross and Souleles, 2002, and Guiso, Sapienza and Zingales (2012) because both literatures provide essentially the same two possible explanations for the transmission of opaque information between neighbors and individuals. First, both literatures emphasize the importance of individuals learning from neighbors. Second, we argue that there are strong similarities between what Grinblatt, Keloharju and Ikaheimo (2008) describe as “conformity” and what Fay, Hurst and White, 2002, Gross and Souleles, 2002 and Guiso, Sapienza and Zingales (2012) describe as “stigma”. Both “conformity” and “stigma” reflect the importance of social pressures to mimic the behavior of neighbors as a mechanism by which neighbors influence individuals.

$$(1) \quad Individual's\ Binary\ Decision = \beta_1' BK_Neighborhood + \beta_2' Individual\ Controls + \beta_3' Neighborhood\ Controls + \varepsilon$$

Grinblattet et al (2008) implement their approach by creating a new variable (p. 744) which we label "net neighborhood" and which we adapt to our bankruptcy context and define as:

Net Neighborhood = (number of bankruptcies per household in inner-ring in last five year) minus (number of bankruptcies per household in outer-ring donut in last five years)

This net neighborhood variable is thus included in our main equation (1) above as the single variable $BK_Neighborhood$. This is the main independent variable of interest that could impact the binary choice. As described above, in our case near neighbors are defined as the individual's six digit Canadian postal code (approximately a city block), and distant neighbors are defined as the individuals three digit Canadian postal code (approximately a suburb). The distant (outer-ring) neighbors can be thought of as a "donut" shape, in that the near (inner-ring) neighbors are removed. Grinblatt, Keloharju and Ikaheimo(2008) argue that "subtracting this outer ring purchase rate (in our case bankruptcies) controls for omitted common attributes shared by residents of both the inner and outer ring that influence purchases (in our case bankruptcies) of residents within the combined inner and outer-ring neighborhood, as well as purchases (bankruptcy) of the subject". They argue that "instrumenting with more distant neighbors purchases (in our case bankruptcies) also controls for other variables (observed and unobserved) that are *common* to a larger community" (italics added p. 736).

Even though this methodology controls for endogeneity and self-selection at the level of the outer-ring (suburb), a possible concern with this methodology is that the net neighborhood variable (i.e. differences between inner and outer rings) may still be systematically related to unobservable characteristics of individuals. Grinblatt, Keloharju and Ikaheimo (2008) examine if there are low correlations between *observable* individual characteristics and the net neighborhood variable. They argue

that if there are low correlations with observable individual characteristics, then it "is quite reasonable to conclude that this lack of correlation extends to those characteristics that we cannot measure" (p. 739). This paper follows Grinblatt, Keloharju and Ikaheimo (2008) in this regard by examining the correlations between the net neighborhood variable, and the various observable individual characteristics in our credit card account level data. In Table 3 we report these correlations for our various samples. In no case is there an economically significant correlation between the net neighborhood variable and the observable individual level credit card variables.

Our second identification strategy is based on legal institutions around bankruptcy and default. As described in the introduction above, in order to control for unobserved neighborhood shocks (e.g. plant closures) we limit our main sample to only defaulters; i.e. bankrupts (abbreviated BK) plus charge-offs (abbreviated CO). The logit specification in (1) sets those choosing for bankruptcy equal to 1 and those choosing charge-off equal to 0 (we could also have defined the dependent logit equal to 1 for charge-off, but this simply results in equal but opposite signed coefficients). In other words, this main specification examines the probability of choosing bankruptcy rather than charge-off from a sample of individuals who are either bankrupts or charge-offs. The econometric advantage of this specification is that we are comparing individuals who are *all* in financial distress, thus we are able to control for the possibility that some neighborhoods may be more likely to face financial distress than others because of unobservable neighborhood shocks.

In addition to this main specification, we also examine a subsidiary specification, which widens the groups of individuals included in the sample beyond just bankrupts and charge-offs. Our second specification still includes bankrupts and charged-off individual's in the sample, but in addition also includes individuals who are three month delinquent (abbreviated as DEL) on their credit cards, but who have not yet defaulted. All the individuals in this larger sample can be considered to be in some degree of financial distress. We can thus use a similar logic as above which stated that unobserved local shocks will cause financial distress, but will not impact the choices that the individual makes between different kinds

of default/distress (e.g. between BK, CO and DEL). Our subsidiary specification including all three of BK, CO and DEL will not, however, be as tightly specified as the main specification that only includes BK and CO. This is because bankruptcy and charge-off can be considered substitutes in that they both constitute formal legal default, whereby the credit card contract is ended, whereas even after three month delinquency, the individual is not in default because the contract is not (yet) terminated. Empirically, we examine two versions of the specification that includes all three of BK, CO and DEL. The first examines those who chose bankruptcy rather than charge-off or delinquents (where bankruptcy = 1 in the logit), while the second examines those who chose charge-off rather than bankruptcy or delinquency (where charge-off = 1 in the logit).

Finally, for comparison purposes, we also include a specification which includes all 93 000 credit card holders (including the defaulters and delinquents discussed above), even though this specification does not exploit legal institutions to identify unobservable neighborhood shocks. Once again we examine two versions of this specification, where either bankruptcy or charge-off are set equal to 1 in the logit. The aim of including this specification, which is similar to that used in the existing literature, is to show how different the results are when we examine the choice *defaulters* make between bankruptcy and charge-off (our main specification above), and the choice all individuals in a population make between filing or not filing for bankruptcy.

6. RESULTS

The full results of all our logit tests are presented in the web appendix. For ease of comparison across the models, we summarize all of our results in Table 4, where we only report results from the main coefficient of interest, the measure of lagged neighborhood bankruptcy. The five columns of Table 4 reflect the five different specifications we run in terms of logit choice variable (either bankruptcy or charge-off) and sample for the regression (i.e. defaulters only, or defaulters plus delinquents, or all credit card holders). The first two rows show estimates for raw measures of the two neighborhood measures, the

six digit postcode (inner-ring or city block) and three digit postcode (outer- ring or suburb). All subsequent rows report various specifications using the Grinblatt, Keloharju and Ikaheimo (2008) net neighborhood methodology (i.e. inner-ring minus outer-ring). To ease interpretation, Table 4 reports the percentage impact on the probability of the individual choosing the choice logit variable (either BK or CO) of a one standard deviation change in the RHS neighborhood bankruptcy variable. The levels of significance are taken from the full logit estimates taken from the tables in the web appendix. For example, the top left cell of Table 4 indicates that a one standard deviation change in bankruptcies in a six digit postal code in the previous five years will increase the probability of an individual choosing default with bankruptcy compared to charge-off by 7.28%, which is significant at 1%.

The first two rows of this Table shows that neighborhood effects are highly significant and of the predicted sign when neighborhoods are defined at the 6 digit (city block) level, but are all insignificant at the 3 digit (suburb) level. Thus, when we implement the Grinblatt, Keloharju and Ikaheimo (2008) methodology of subtracting the outer-ring from the inner-ring, reported in row 3 of Table 4, we find that these effects are still highly significant and all have the expected signs. However, the magnitudes of these effects in row 3 are smaller than the raw inner ring effects reported in row 1. This reduction in the magnitude of the estimated effect is the result of controlling for endogeneity or self-selection issues at the level of the outer-ring.

We can also examine the various choice (logit) and alternative samples, utilizing the Grinblatt, Keloharju and Ikaheimo (2008) methodology, by examining the cells across the Table in row 3. Our main specification (Model 1) examining whether defaulters choose bankruptcy rather than charge-off shows that a one standard deviation change in the net neighborhood measure of bankruptcies in the last five years will lead to a 6 percent increase in the probability that the defaulter will choose bankruptcy rather than charge-off. As we widen the sample (Models 2 and 3), these probabilities decline substantially. These estimates are 0.85% when the sample is broadened to defaulters plus delinquents, and 0.02% when the sample is broadened to all credit card holders. Similarly, when the charge-off results (Models 4 and 5)

are broadened to the larger samples, the magnitudes also decline sharply. These results highlight the importance of examining these neighborhood effects when the sample is restricted only to defaulters, as in column 1 (i.e. bankrupts plus charge-offs). As we describe above, our paper is the first in the literature to limit our sample in this way.

Even though the magnitudes of these effects are very different across the different cells of row 3, all estimates are very significant and all have the predicted signs. The net neighborhood coefficient is positive and significant for all models where bankruptcy is the logit choice variable (Models 1, 2 and 3), and negative and significant for all models where charge-off is the logit choice variable (Models 4 and 5). This is exactly as predicted in the discussion above. This implies that lagged neighborhood bankruptcies will have a positive impact of the individual's choice to file for bankruptcy (because of neighborhood stigma and information effects) but a negative impact on the individual's choice to charge-off (because individual's from high stigma neighborhoods, with low past bankruptcies, will choose to default without public disclosure, so as to reduce the probability of their neighbors learning of their default).

Panel 2 of Table 4 splits the sample between post codes with different characteristics, all of which are measured using the Grinblatt, Keloharju and Ikaheimo (2008) net neighborhood methodology. The first two rows of Panel 2 reports results where post codes are either in census metropolitan areas (i.e. large cities),or in other areas (which for simplicity we designate rural).We use Metropolitan Influence Zone (MIZ) data from Statistics Canada to classify every post code in Canada as either being within a Census Metropolitan Area, or not. These results are very stark, showing that the impact of neighborhood effects are very highly significant, with the predicted signs, for all the rural specifications, but are insignificant for all the urban specifications. This implies that while past neighborhood bankruptcies in rural areas have a positive effect on bankruptcies and a negative effect on charge-offs as predicted, past neighborhood bankruptcies do not have a significant impact on either bankruptcies or charge-offs in urban areas.

These rural/urban results are quite similar to those found by Grinblatt, Keloharju and Ikaheimo (2008, p.743) in a very different context (the impact of neighbors on the purchase of cars in Finland).Both of these sets of results (filing for bankruptcy in Canada or buying a car in Finland) imply that individuals in rural areas are more influenced by the opinions and actions of their close neighbors compared to individuals in urban areas. One possible explanation is that urban areas have higher densities and lower communication and transport costs, thus social networks of urban individuals could be dispersed throughout the city, rather than concentrated in the individual's near geographic neighborhood (i.e. city block) examined here. On the other hand lower densities and higher communication and travel costs in rural areas could imply that the social network of rural individuals are more likely to be geographically concentrated in the near neighborhood around the individual. Because of the very detailed nature of our data, we are the first to provide empirical evidence of these rural/urban differences in neighborhood effects in the bankruptcy context.

We also split up our data based on the income per household of different areas, as measured by average family income at the DA level. We split our sample into post codes above or below the median post code measure of average family income. Our results are reported in rows 3 and 4 of Panel 2, Table 4. Once again our main findings are stark. We find that all of the coefficients for the low income neighborhoods are very significant, with the predicted signs, while all the coefficients of the high income neighborhoods are insignificant. Furthermore, the magnitudes of the estimated coefficients for the low income sample are generally larger than for the full sample reported in panel 1 above. Once again this implies that neighbors in lower income areas have a very significant impact on individual choices to default with or without bankruptcy, whereas the impact of neighbors on these individual choices in high income areas is insignificant. One possible explanation for this finding, is that individuals in higher income neighborhoods are able to afford to maintain a social network that is more widely dispersed geographically, whereas because of the costs of developing and maintaining a social network, individuals

in lower income areas may be more likely to maintain a social network that is predominately in their intermediate geographic neighborhood.

Our final two rows of Table 4 split the sample based on the standard deviation of income in the neighborhood (as measured at the DA level by the census). In other words, we are comparing the influence of neighbors on individuals in neighborhoods that are relatively homogenous in terms of family income, and other neighborhoods where there is wide dispersion in family income. Once again the comparison between high standard deviation and low standard deviation neighborhoods is stark. We find that the neighborhood coefficient in all five models has the expected sign for the low standard deviation (i.e. homogenous) neighborhoods, while none of these coefficients is significant for the high standard deviation neighborhoods. One possible explanation for this finding is that individuals are more likely to be influenced by the opinions and actions of their neighbors if those neighbors are relatively similar to them in terms of income. On the other hand, these findings imply that near geographic neighbors will have less impact on each other if there are wide dispersions in income among the neighbors.

7. ROBUSTNESS TESTS

7.1. Removing Mortgage Holders from the Sample

In order to control for the possibility that outstanding mortgage debt could impact the choice between defaulting with or without bankruptcy, we run a robustness check of the tests above by removing from our sample any current mortgage holders at the bank. These robustness tests, in other words, include only individuals who have a credit card account at the bank, but exclude individuals who have both a credit card as well as a mortgage at the bank. By doing this we can focus on the key choice in this paper which is between defaulting via bankruptcy or defaulting via credit card charge-off, while excluding the possibility that this choice may be influenced by a mortgage debt outstanding. Our results from the robustness tests are very similar to our main results. In other words, we can conclude that our main results are not being driven by issues related to outstanding mortgage debts in bankruptcy.

7.2. Including FICO Scores in the Regressions

As described above, and in Table 1, a large number of individuals in our sample do not have an updated FICO score in the data. This is particularly prevalent for individuals who are in financial distress or who have defaulted. Thus if we included FICO score as a variable we would lose a significant fraction of our sample of defaulters. Our main results do not therefore include the FICO score variable, but we rerun all our models as robustness tests with the FICO score included. Our main results are robust to this inclusion.

8. CONCLUSION

The issue of bankruptcy spillovers within geographic areas has been discussed in a large literature (e.g. Fay, Hurst and White, 2002, Gross and Souleles, 2002, Dick, Lehnert and Topa, 2008, Cohen-Cole and Duygan-Bump, 2009, Livshits, MacGee and Tertilt, 2010). This is also an issue of considerable policy importance. However, data limitations have precluded robust tests of this hypothesis.

This paper tests the bankruptcy spillover hypothesis using a high quality new Canadian database that matches credit card account level data from individual defaulters, with city block level counts of all previous bankruptcies in each neighborhood in each year. Our data allows us to use two separate identification strategies. The first exploits very fine grained geographic data to control for unobserved neighborhood level characteristics by "differencing out" inner-ring from outer-ring neighbors. The second controls for unobserved neighborhood level shocks by limiting our sample only to defaulters and examining the choice between bankruptcy and charge-off. This differs from the existing literature, which examines the choice between bankruptcy and not defaulting at all, which could be impacted by unobserved local shocks across neighborhoods. Our main finding is that neighborhood bankruptcy spillovers are significant. A one standard deviation increase in the past bankruptcies in an individual defaulter's neighborhood will increase the probability that the individual will default via bankruptcy rather than via charge-off by 6%.

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TABLE 1: SUMMARY STATISTICS

	Bankruptcies			Credit Card Charge-Offs			All Credit Card Accounts		
	Obs	**Mean**	**Std. Dev**	**Obs**	**Mean**	**Std. Dev**	**Obs**	**Mean**	**Std. Dev**
Bankruptcy (Pcode 2000-04)	119	12.13	20.35	361	6.77	11.56	93130	8.18	14.17
Utilization Rate (Balance/Limit)	119	81.41	29.94	362	87.70	28.69	93195	41.68	43.12
Card APR	119	15.99	3.93	362	16.70	3.14	93194	15.74	4.14
FICO Score	100	622.98	86.66	203	593.17	87.04	75466	729.67	72.19
Credit Line ($)	119	4360	3843	362	3900	4242	93195	5764	6162
Mortgage Balance (Dummy)	119	0.17	0.38	362	0.13	0.33	93195	0.22	0.42
Family Median Income (DA)	115	67899	23123	344	70428	24259	87152	73159	25895
Family Income Dist (DA)	115	7979	7028	344	8840	8242	87152	8788	8545
Pop Without Income (DA)	115	25.57	33.69	344	25.48	27.96	87152	27.76	37.96
Financial Literacy	115	0.30	0.03	344	0.30	0.03	87152	0.30	0.03
Unemployment Rate (DA)	115	4.57	3.58	344	4.59	5.08	87152	4.03	3.69

TABLE 2: UNIVARIATE TESTS OF NEIGHBORHOOD COMPARISONS
OSB Neighborhood Measure: Annual Consumer Bankruptcies per six-digit Post Code

This Table uses univariate comparison of mean tests to compare the number of bankruptcies in the 119 bankruptcy neighborhoods (where the 119 Individual credit card bankrupts live) with the number of bankruptcies in the 361 charge-off neighborhoods (where the 361 individual credit card charge-offs live). Tests are conducted for 8 individual years and show that there are significantly more bankruptcies in the bankruptcy neighborhoods compared to the charge-off neighborhoods.

Year	**Bankruptcy Neighborhoods**			**Charge-Off Neighborhoods**			**T test**	**signif**
	obs	**Mean**	**Std Error**	**obs**	**Mean**	**Std Error**		
2000	119	2.72268	0.561415	361	1.249307	0.122918	3.8125	***
2001	119	2.11764	0.341786	361	1.257618	0.122714	2.9658	***
2002	119	2.05042	0.315495	361	1.216066	0.126606	2.925	***
2003	119	2.71428	0.414133	361	1.448753	0.155025	3.5194	***
2004	119	2.52100	0.387978	361	1.595568	0.148992	2.7073	***
2005	119	2.47058	0.334552	361	1.393352	0.143392	3.4202	***
2006	119	1.81512	0.320893	361	0.980609	0.101499	3.2708	***
2007	119	1.38655	0.225212	361	0.839335	0.087044	2.7476	***

TABLE 3: CORRELATION BETWEEN OBSEVABLE INDIVIDUAL DATA AND NET NEIGHBORHOOD MEASURE

In order to ensure that there are not systematic differences between inner-ring and outer-ring neighbors (i.e. the net neighborhood variables), we follow Grinblatt, Keloharju and Ikaheimo (2008) in examining the correlations between the net neighborhood variable, and the various observable individual characteristics in our credit card account level data. In no case is there an economically significant correlation between the net neighborhood variable and the observable individual level credit card variables.

Sample:	**Defaulters Only (BK + CO)**	**Defaulters & Delinquents (BK + CO + DEL)**	**All Card Holders**
Observable Data (From Credit Card Account)		Correlations	
Card Utilization (%)	0.0219	0.0002	0.0096
p value	0.6336	0.9911	0.0033
Card Credit Line ($)	-0.0168	0.0316	-0.0028
p value	0.6555	0.1184	0.3903
Card APR (%)	-0.045	-0.0109	-0.0023
p value	0.2309	0.5918	0.4868
MortgagePayer - Dum	0.0536	-0.0067	-0.0109
p value	0.1532	0.7409	0.0009
FICO Score	-0.0019	0.004	-0.0131
p value	0.9725	0.8727	0.0003

Sample: "BK & CO Only" = Only Defaulters (bankrupts and charge-offs ; "BK & CO & DEL" = Defaulters + 3 month credit card delinquents

TABLE 4: RESULTS SUMMARY

This table reports the percentage impact of a one standard deviation change in past neighborhood bankruptcies in the previous five years on the logit (choice) variable. This table only reports the lagged neighborhood coefficient from the full models. Each cell represents one regression and the full regression results are reported in the Web Appendix. The main specification (reported in column 1) limits the sample to only defaulters, and examines the choice between defaulting via bankruptcy or charge-off. The remaining columns widen the sample to include delinquents (columns 2 and 4) and all credit card holders (columns 3 and 5). Panel 1 separately examines neighborhood bankruptcies in inner-rings only, outer rings only and the net neighborhood measure of inner- minus outer-rings. Panel 2 splits the sample into neighborhoods based on rural/urban, neighborhood average income and neighborhood standard deviation.

Model	**1**	**2**	**3**	**4**	**5**
Logit= 1 (Choice) Variable:	**BK**	**BK**	**BK**	**CO**	**CO**
Sample:	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
PANEL 1: All Neighborhoods					
6 Digit Post Code (City Block)	7.28***	0.99***	0.03***	-2.6***	-0.05***
3 Digit Post Code FSA (Suburb)	0.32	-0.46	-0.02	-1.56	-0.08*
Net Neighborhood (Grinblatt et al (2008))	6***	0.85**	0.02***	-2.33***	-0.05**
PANEL 2: Neighborhood Split on Demographics - All Use Grinblatt et al (2008) Net Neighborhood Methodology					
1. Urban vs. Rural Neighborhoods					
Urban (CMA)	1.05	0.14	0.01	-2.07	-0.01
Rural (Not CMA)	8.47***	1.26***	0.03***	-1.99**	-0.04*
2. Low and High Average Income in Neighborhoods					
High Average Income	3.8	0.51	0.02	-0.95	0
Low Average Income	7.72***	1.04**	0.03**	-3.75***	-0.1***
3. Low and High Standard Deviation of Income in Neighborhood					
High Income Standard Deviation	-0.64	-0.14	0	-0.34	0.01
Low Income Standard Deviation	10.58***	1.34***	0.02***	-3.83***	-0.09***

Logit Variable; BK = Bankruptcy or CO = Charge-Off
Sample: "BK & CO Only" = Only Defaulters (bankrupts and charge-offs ; "BK & CO & DEL" = Defaulters + 3 month credit card delinquents; "All" = All Credit Card Holders
Estimated Impacts of One Standard Deviation Change in Neighborhood Measures taken from PRCHANGE program in STATA
Significance Levels taken from full regressions as reported in Web Appendix.

Web Appendix

Bankruptcy Spillovers:

Distance, Public Disclosure and Opaque Information

Additional Tables

TABLE A 1: LOGIT MODELS: SIX DIGIT POST CODE (CITY BLOCK) LEVEL

ALL NEIGHBORHOODS
Basis of Results in Table 4, Panel A, Line 1

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Bankruptcies in 6 Digit Post Code (2000-04)	0.0274***	0.0135***	0.0155***	-0.0149***	-0.0104**
	(0.00782)	(0.00524)	(0.00528)	(0.00537)	(0.00495)
Card Utilization (%)	-0.00751**	0.0121***	0.00350***	0.0246***	0.00493***
	(0.00366)	(0.00344)	(0.000514)	(0.00250)	(0.000481)
Card Credit Line ($)	3.91e-06	-9.73e-06	-3.47e-05	1.45e-06	-3.7e-05***
	(3.07e-05)	(2.44e-05)	(2.13e-05)	(1.61e-05)	(1.31e-05)
Card APR (%)	-0.0397	0.0218	-0.00392	0.0923***	0.0446**
	(0.0364)	(0.0299)	(0.0259)	(0.0213)	(0.0176)
Mortgage Payer – Dum	0.193	-0.244	-0.234	-0.579***	-0.499***
	(0.314)	(0.258)	(0.251)	(0.176)	(0.162)
Family Median Income (DA)	2.79e-07	-6.85e-07	-4.67e-06	7.34e-07	-3.59e-06
	(5.51e-06)	(4.50e-06)	(4.43e-06)	(2.79e-06)	(2.47e-06)
Family Income Dist (DA)	-1.01e-05	-5.77e-06	-1.37e-07	2.26e-06	6.52e-06
	(1.80e-05)	(1.42e-05)	(1.41e-05)	(7.57e-06)	(6.56e-06)
Pop Without Income (DA)	0.00198	0.000809	0.000105	-0.000919	-0.00109
	(0.00379)	(0.00288)	(0.00279)	(0.00193)	(0.00168)
Numerical Literacy (DA)	3.225	1.651	3.420	-1.242	1.725
	(3.648)	(3.183)	(3.113)	(2.003)	(1.794)
Unemployment Rate (DA)	0.00896	0.0132	0.0237	0.0207	0.0276***
	(0.0246)	(0.0228)	(0.0192)	(0.0148)	(0.0107)
Number of H'holds in Pcode	-0.00331*	-0.00199	-0.00236*	0.000666	-0.000156
	(0.00180)	(0.00144)	(0.00137)	(0.000926)	(0.000823)
Constant	-0.946	-4.540***	-7.366***	-4.620***	-6.563***
	(1.438)	(1.229)	(1.121)	(0.796)	(0.663)
Observations	459	2,084	87,151	2,084	87,151
R2	0.0424	0.0280	0.0256	0.101	0.0364

TABLE A 2: LOGIT MODELS: THREE DIGIT POST CODE (FSA) SUBURB LEVEL

ALL NEIGHBORHOODS
Basis of Results in Table 4, Panel A, Line 2

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Bankruptcies in 3 Digit FSA (2000-04)	9.02e-05	-0.000506	-0.00102	-0.000730	-0.00120*
	(0.000977)	(0.000952)	(0.00105)	(0.000622)	(0.000620)
Card Utilization (%)	-0.00658*	0.0119***	0.00346***	0.0247***	0.00491***
	(0.00360)	(0.00343)	(0.000516)	(0.00251)	(0.000478)
Card Credit Line ($)	3.09e-06	-6.63e-06	-3.45e-05	-7.45e-07	-3.9e-05***
	(3.08e-05)	(2.43e-05)	(2.13e-05)	(1.61e-05)	(1.31e-05)
Card APR (%)	-0.0498	0.0217	-0.00332	0.0913***	0.0437**
	(0.0356)	(0.0299)	(0.0258)	(0.0213)	(0.0176)
MortgagePayer - Dum	0.192	-0.238	-0.216	-0.581***	-0.521***
	(0.310)	(0.258)	(0.251)	(0.177)	(0.164)
Family Median Income (DA)	-2.44e-06	-1.63e-06	-5.99e-06	1.52e-06	-3.47e-06
	(5.47e-06)	(4.49e-06)	(4.41e-06)	(2.78e-06)	(2.46e-06)
Family Income Dist (DA)	-1.09e-05	-7.32e-06	-3.20e-06	2.29e-06	5.63e-06
	(1.77e-05)	(1.46e-05)	(1.46e-05)	(7.57e-06)	(6.59e-06)
Pop Without Income (DA)	-0.000263	-0.000322	-0.00112	-0.000610	-0.00114
	(0.00382)	(0.00299)	(0.00291)	(0.00191)	(0.00167)
Numerical Literacy (DA)	1.579	1.036	4.039	-1.116	2.117
	(3.507)	(3.106)	(3.089)	(1.973)	(1.781)
Unemployment Rate (DA)	0.00116	0.0120	0.0232	0.0174	0.0245**
	(0.0239)	(0.0223)	(0.0188)	(0.0151)	(0.0111)
Number of H'holds in FSA	1.43e-05	2.45e-05	2.43e-05	1.10e-05	1.23e-05
	(2.59e-05)	(2.43e-05)	(2.63e-05)	(1.59e-05)	(1.54e-05)
Constant	-0.197	-4.372***	-7.402***	-4.711***	-6.545***
	(1.404)	(1.204)	(1.115)	(0.795)	(0.663)
Observations	458	2,081	87,137	2,081	87,137
R2	0.0205	0.0227	0.0214	0.0986	0.0367

TABLE A 3: LOGIT MODELS: GRINBLATT ET AL (2008),NET NEIGHBORHOOD METHOD

ALL NEIGHBORHOODS
Basis of Results in Table 4, Panel A, Line 3

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Net Neighborhood Bankruptcies (2000-04)	0.0235***	0.0119**	0.0142***	-0.0139***	-0.0102**
	(0.00749)	(0.00525)	(0.00538)	(0.00517)	(0.00486)
Card Utilization (%)	-0.00739**	0.0120***	0.00344***	0.0248***	0.00492***
	(0.00365)	(0.00344)	(0.000514)	(0.00251)	(0.000482)
Card Credit Line ($)	6.94e-06	-8.71e-06	-3.49e-05	8.85e-08	-3.8e-05***
	(3.06e-05)	(2.45e-05)	(2.13e-05)	(1.62e-05)	(1.31e-05)
Card APR (%)	-0.0389	0.0223	-0.00374	0.0909***	0.0436**
	(0.0362)	(0.0299)	(0.0259)	(0.0213)	(0.0176)
MortgagePayer - Dum	0.170	-0.237	-0.225	-0.598***	-0.522***
	(0.312)	(0.258)	(0.251)	(0.177)	(0.164)
Family Median Income (DA)	-3.57e-07	-7.92e-07	-4.35e-06	9.70e-07	-3.12e-06
	(5.51e-06)	(4.50e-06)	(4.44e-06)	(2.79e-06)	(2.46e-06)
Family Income Dist (DA)	-9.95e-06	-5.84e-06	-7.23e-07	2.25e-06	6.36e-06
	(1.78e-05)	(1.43e-05)	(1.42e-05)	(7.57e-06)	(6.55e-06)
Pop Without Income (DA)	0.00140	0.000444	-0.000370	-0.000798	-0.00115
	(0.00380)	(0.00292)	(0.00285)	(0.00191)	(0.00167)
Numerical Literacy (DA)	1.563	0.710	2.369	-0.983	1.620
	(3.488)	(3.081)	(3.026)	(1.971)	(1.763)
Unemployment Rate (DA)	0.00933	0.0146	0.0241	0.0173	0.0253**
	(0.0245)	(0.0227)	(0.0187)	(0.0151)	(0.0111)
Constant	-0.515	-4.312***	-7.144***	-4.682***	-6.549***
	(1.409)	(1.210)	(1.108)	(0.794)	(0.661)
Observations	458	2,081	87,137	2,081	87,137
R2	0.0374	0.0262	0.0242	0.102	0.0363

TABLE A 4: LOGIT MODELS: GRINBLATT ET AL (2008), NET NEIGHBORHOOD METHOD

RURAL NEIGHBORHOODS
Basis of Results in Table 4, Panel B, Rural

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Net Neighborhood Bankruptcies (2000-04)	0.0289***	0.0180***	0.0197***	-0.0118**	-0.00932*
	(0.00923)	(0.00636)	(0.00611)	(0.00573)	(0.00549)
Card Utilization (%)	-0.0131***	0.00684	0.00333***	0.0264***	0.00469***
	(0.00490)	(0.00436)	(0.000697)	(0.00347)	(0.000623)
Card Credit Line ($)	5.27e-05	3.96e-06	-2.73e-05	-1.06e-05	-4.9e-05***
	(4.18e-05)	(2.91e-05)	(2.78e-05)	(2.19e-05)	(1.85e-05)
Card APR (%)	0.0368	0.0661	0.0278	0.0731**	0.0330
	(0.0532)	(0.0421)	(0.0381)	(0.0284)	(0.0240)
MortgagePayer - Dum	0.452	0.0210	0.0294	-0.526**	-0.476**
	(0.415)	(0.327)	(0.318)	(0.236)	(0.220)
Family Median Income (DA)	6.26e-07	-8.44e-07	-1.60e-06	8.98e-07	4.95e-07
	(7.34e-06)	(6.19e-06)	(6.35e-06)	(3.97e-06)	(3.67e-06)
Family Income Dist (DA)	1.76e-05	2.40e-05	1.32e-05	3.46e-06	-2.17e-06
	(2.90e-05)	(2.36e-05)	(2.08e-05)	(1.74e-05)	(1.43e-05)
Pop Without Income (DA)	-0.0140*	-0.0103	-0.0113*	0.000129	-0.000615
	(0.00815)	(0.00639)	(0.00637)	(0.00255)	(0.00211)
Numerical Literacy (DA)	0.341	-0.369	0.302	-1.184	0.171
	(5.019)	(4.342)	(4.419)	(2.890)	(2.659)
Unemployment Rate (DA)	0.00120	0.000703	0.0143	0.0187	0.0422***
	(0.0324)	(0.0310)	(0.0311)	(0.0179)	(0.0159)
Constant	-1.275	-4.575***	-7.356***	-4.535***	-6.210***
	(2.016)	(1.699)	(1.622)	(1.130)	(0.981)
Observations	250	1,256	51,220	1,256	51,220
R2	0.0915	0.0356	0.0305	0.102	0.0369

TABLE A 5: LOGIT MODELS: GRINBLATT ET AL (2008), NET NEIGHBORHOOD METHOD

URBAN NEIGHBORHOODS
Basis of Results in Table 4, Panel B, Urban

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Net Neighborhood Bankruptcies (2000-04)	0.00732	0.00304	0.00742	-0.0158	-0.00317
	(0.0202)	(0.0151)	(0.0143)	(0.0142)	(0.0109)
Card Utilization (%)	-0.00248	0.0183***	0.00393***	0.0229***	0.00530***
	(0.00619)	(0.00569)	(0.000798)	(0.00363)	(0.000759)
Card Credit Line ($)	-4.85e-05	-2.53e-05	-3.26e-05	1.70e-05	-2.35e-05
	(5.02e-05)	(4.17e-05)	(3.26e-05)	(2.55e-05)	(1.86e-05)
Card APR (%)	-0.122**	-0.0250	-0.0294	0.116***	0.0553**
	(0.0566)	(0.0431)	(0.0350)	(0.0330)	(0.0260)
MortgagePayer - Dum	-0.147	-0.694	-0.610	-0.708***	-0.596**
	(0.518)	(0.435)	(0.415)	(0.269)	(0.245)
Family Median Income (DA)	-3.65e-06	-1.73e-06	-1.08e-05*	8.70e-07	-7.25e-06**
	(8.68e-06)	(6.79e-06)	(6.13e-06)	(4.05e-06)	(3.35e-06)
Family Income Dist (DA)	-2.73e-05	-1.34e-05	7.17e-07	1.21e-06	1.14e-05
	(2.48e-05)	(1.88e-05)	(1.81e-05)	(8.72e-06)	(7.24e-06)
Pop Without Income (DA)	0.00689	0.00542	0.00599*	-0.00199	-0.00120
	(0.00461)	(0.00332)	(0.00314)	(0.00292)	(0.00273)
Numerical Literacy (DA)	4.990	5.887	10.57**	0.630	4.740*
	(5.531)	(4.879)	(4.761)	(2.963)	(2.623)
Unemployment Rate (DA)	0.0451	0.0498	0.0192	0.0150	3.50e-05
	(0.0524)	(0.0440)	(0.0258)	(0.0294)	(0.0224)
Constant	-0.0786	-5.419***	-8.589***	-5.302***	-7.274***
	(2.178)	(1.836)	(1.647)	(1.182)	(0.948)
Observations	208	825	35,917	825	35,917
R2	0.0511	0.0646	0.0407	0.100	0.0397

TABLE A 6: LOGIT MODELS: GRINBLATT ET AL (2008), NET NEIGHBORHOOD METHOD

HIGHER THAN AVERAGE INCOME NEIGHBORHOODS
Basis of Results in Table 4, Panel B, High Average Income

MODEL **Logit Variable**	**1** **BK**	**2** **BK**	**3** **BK**	**4** **CO**	**5** **CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Net Neighborhood Bankruptcies (2000-04)	0.0145	0.00796	0.0121	-0.00646	0.000330
	(0.0103)	(0.00867)	(0.00861)	(0.00709)	(0.00638)
Card Utilization (%)	-0.00966*	0.0134***	0.00382***	0.0274***	0.00546***
	(0.00572)	(0.00512)	(0.000804)	(0.00380)	(0.000705)
Card Credit Line ($)	1.10e-05	1.69e-06	-1.98e-05	-5.73e-06	-3.34e-05*
	(4.13e-05)	(3.23e-05)	(2.73e-05)	(2.34e-05)	(1.77e-05)
Card APR (%)	-0.0371	0.0180	-0.0154	0.0776**	0.0238
	(0.0509)	(0.0418)	(0.0354)	(0.0303)	(0.0236)
MortgagePayer - Dum	0.279	-0.479	-0.500	-1.058***	-0.989***
	(0.490)	(0.385)	(0.373)	(0.278)	(0.262)
Family Income Dist (DA)	-9.69e-06	-5.80e-06	-2.98e-06	2.74e-06	5.89e-06
	(2.22e-05)	(1.57e-05)	(1.58e-05)	(8.21e-06)	(7.00e-06)
Pop Without Income (DA)	0.00719	0.00327	0.00238	-0.00300	-0.00313
	(0.00453)	(0.00291)	(0.00272)	(0.00264)	(0.00238)
Numerical Literacy (DA)	8.638	2.678	6.231	-3.904	-0.0237
	(5.956)	(4.867)	(4.649)	(3.016)	(2.583)
Unemployment Rate (DA)	0.116*	0.0657	0.0603	-0.0309	-0.0306
	(0.0643)	(0.0483)	(0.0445)	(0.0336)	(0.0297)
Constant	-2.987	-5.212***	-8.692***	-3.427***	-5.824***
	(2.041)	(1.747)	(1.577)	(1.126)	(0.897)
Observations	212	971	45,174	971	45,174
R2	0.0613	0.0348	0.0265	0.128	0.0416

TABLE A 7: LOGIT MODELS: GRINBLATT ET AL (2008), NET NEIGHBORHOOD METHOD

LOWER THAN AVERAGE INCOME NEIGHBORHOODS
Basis of Results in Table 4, Panel B, Low Average Income

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Net Neighborhood Bankruptcies (2000-04)	0.0324***	0.0143**	0.0156**	-0.0211***	-0.0196***
	(0.0110)	(0.00667)	(0.00684)	(0.00727)	(0.00695)
Card Utilization (%)	-0.00615	0.0114**	0.00329***	0.0224***	0.00453***
	(0.00501)	(0.00469)	(0.000684)	(0.00338)	(0.000642)
Card Credit Line ($)	-9.53e-06	-2.97e-05	-5.41e-05	4.98e-06	-4.36e-05**
	(4.77e-05)	(3.87e-05)	(3.33e-05)	(2.29e-05)	(1.94e-05)
Card APR (%)	-0.0514	0.0218	0.00815	0.108***	0.0642**
	(0.0543)	(0.0434)	(0.0380)	(0.0305)	(0.0265)
MortgagePayer - Dum	-0.00383	-0.0320	-0.00380	-0.186	-0.100
	(0.431)	(0.351)	(0.339)	(0.233)	(0.210)
Family Income Dist (DA)	-2.31e-06	-9.84e-06	-8.39e-06	-6.34e-06	-4.37e-06
	(2.82e-05)	(2.85e-05)	(2.90e-05)	(1.60e-05)	(1.48e-05)
Pop Without Income (DA)	-0.0133	-0.00894	-0.00960	0.00237	0.00135
	(0.00843)	(0.00655)	(0.00646)	(0.00317)	(0.00266)
Numerical Literacy (DA)	-1.145	0.373	0.866	1.326	2.795
	(4.466)	(3.993)	(4.048)	(2.668)	(2.426)
Unemployment Rate (DA)	-0.00290	0.00391	0.0161	0.0287*	0.0343***
	(0.0288)	(0.0275)	(0.0239)	(0.0168)	(0.0103)
Constant	0.666	-3.977**	-6.843***	-5.513***	-7.378***
	(1.895)	(1.567)	(1.445)	(1.051)	(0.892)
Observations	246	1,110	41,963	1,110	41,963
R2	0.0648	0.0323	0.0268	0.0956	0.0382

TABLE A 8: LOGIT MODELS: GRINBLATT ET AL (2008), NET NEIGHBORHOOD METHOD

LOWER STANDARD DEVIATION OF INCOME NEIGHBORHOODS
Basis of Results in Table 4, Panel B, Low Std Dev Income

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Net Neighborhood Bankruptcies (2000-04)	0.0343***	0.0141**	0.0142***	-0.0199***	-0.0178***
	(0.0103)	(0.00569)	(0.00538)	(0.00683)	(0.00665)
Card Utilization (%)	-0.00500	0.0140***	0.00344***	0.0236***	0.00437***
	(0.00499)	(0.00464)	(0.000514)	(0.00355)	(0.000625)
Card Credit Line ($)	1.16e-05	2.06e-06	-3.49e-05	7.39e-06	-3.54e-05*
	(4.19e-05)	(2.96e-05)	(2.13e-05)	(2.21e-05)	(1.88e-05)
Card APR (%)	-0.0346	0.0274	-0.00376	0.0851***	0.0518**
	(0.0510)	(0.0390)	(0.0259)	(0.0302)	(0.0254)
MortgagePayer - Dum	0.330	-0.0222	-0.224	-0.660***	-0.589**
	(0.422)	(0.317)	(0.251)	(0.256)	(0.238)
Family Med Income (DA)	1.33e-05	5.47e-06	-4.43e-06	-2.27e-06	-6.01e-06
	(8.97e-06)	(6.56e-06)	(4.11e-06)	(4.59e-06)	(4.40e-06)
Pop Without Income (DA)	-0.00109	-0.000902	-0.000357	0.000574	0.00127
	(0.00464)	(0.00384)	(0.00283)	(0.00253)	(0.00235)
Numerical Literacy (DA)	-1.783	-1.702	2.374	0.972	4.501*
	(4.682)	(4.112)	(3.023)	(2.969)	(2.733)
Unemployment Rate (DA)	0.0124	0.0132	0.0242	0.0245	0.0308***
	(0.0303)	(0.0294)	(0.0187)	(0.0189)	(0.0112)
Constant	-0.659	-4.205***	-7.146***	-4.918***	-7.365***
	(1.918)	(1.609)	(1.107)	(1.152)	(0.985)
Observations	245	1,076	87,137	1,076	43,434
R2	0.0644	0.0321	0.0242	0.101	0.0402

TABLE A 9: LOGIT MODELS: GRINBLATT ET AL (2008), NET NEIGHBORHOOD METHOD

HIGHER STANDARD DEVIATION OF INCOME NEIGHBORHOODS
Basis of Results in Table 4, Panel B, High Std Dev Income

MODEL	**1**	**2**	**3**	**4**	**5**
Logit Variable	**BK**	**BK**	**BK**	**CO**	**CO**
Sample	**BK & CO Only**	**BK & CO & DEL**	**All**	**BK & CO & DEL**	**All**
Net Neighborhood Bankruptcies (2000-04)	-0.00348	-0.00312	-8.11e-05	-0.00265	0.00329
	(0.0153)	(0.0140)	(0.0140)	(0.00819)	(0.00723)
Card Utilization (%)	-0.00975*	0.00993*	0.00368***	0.0258***	0.00565***
	(0.00561)	(0.00523)	(0.000852)	(0.00354)	(0.000711)
Card Credit Line ($)	-8.18e-06	-2.56e-05	-4.83e-05	-6.64e-06	-4.10e-05**
	(4.70e-05)	(4.18e-05)	(3.53e-05)	(2.32e-05)	(1.84e-05)
Card APR (%)	-0.0677	0.0104	-0.0219	0.0970***	0.0348
	(0.0534)	(0.0476)	(0.0403)	(0.0301)	(0.0243)
MortgagePayer - Dum	-0.272	-0.616	-0.549	-0.522**	-0.457**
	(0.521)	(0.455)	(0.446)	(0.247)	(0.226)
Family Median Income (DA)	-6.84e-06	-3.52e-06	-6.37e-06	3.12e-06	-9.94e-08
	(7.48e-06)	(6.54e-06)	(6.76e-06)	(3.47e-06)	(3.16e-06)
Pop Without Income (DA)	0.00468	0.000419	-0.00178	-0.00251	-0.00431
	(0.00757)	(0.00484)	(0.00483)	(0.00325)	(0.00292)
Numerical Literacy (DA)	6.473	3.266	5.255	-2.485	-0.512
	(5.579)	(4.728)	(4.650)	(2.697)	(2.346)
Unemployment Rate (DA)	0.0473	0.0272	0.0296	-0.00429	-0.000326
	(0.0507)	(0.0401)	(0.0385)	(0.0253)	(0.0221)
Constant	-0.991	-4.547**	-7.517***	-4.477***	-5.879***
	(2.198)	(1.872)	(1.754)	(1.100)	(0.899)
Observations	213	1,005	43,703	1,005	43,703
R2	0.0372	0.0265	0.0265	0.110	0.0374