Do Slot Machines Cause Financial Distress?

A Regulatory Natural Experiment with Exogenous Changes to Slot Locations

Vyacheslav Mikhed, Federal Reserve Bank of Philadelphia

Barry Scholnick, University of Alberta*

Hyungsuk Byun, Government of Alberta

December 2016

Abstract

We provide causal evidence that slot machines cause financial distress. We exploit a regulatory natural experiment, where gambling regulators exogenously moved slots from individual bars to larger mini-casinos, over an extended period of time. The dates at which regulators removed slots from bars were plausibly exogenous because they depended on the construction timing and opening dates of new mini-casinos, in other, geographically unrelated, locations. We find that the removal of slots from bars significantly reduced the number of neighboring bankruptcies. These effects increase with the dollar amount of gambling removed from the neighborhood, and decrease with distance from each bar.

JEL Codes: D12, K35, L83

Keywords: slot machines, bankruptcy, neighborhood disamenities

1. Introduction

This paper provides causal evidence on the link between slot machine availability and financial distress, as measured by bankruptcy filings. [1] We exploit a unique regulatory natural experiment, where gambling regulators exogenously removed slot machines from specific bars and pubs, and examine how this exogenous removal of slot machines affected subsequent bankruptcy filings in the immediate neighborhood of the removed machines. [2]

The potential costs of slot machines to their users have recently been highlighted by a number of authors. Ackerlof and Shiller, in their book "*Phishing for Phools*" (2015), for example, make the controversial argument that markets can create incentives for firms to take advantage of consumer behavior through manipulation and deception. In the preface to their book, a motivating example of such behavior is the slot machine. They argue that "Free markets…create an economic equilibrium that is highly suitable for economic enterprises that manipulate or distort our judgment…*The slot machine is a blunt example*. It is no coincidence that before they were regulated and outlawed, slot machines were so common that they were unavoidable" (Ackerlof and Shiller, 2015, p. x, italics added).

Other authors in the popular press have also described slot machines in terms of consumer manipulation. For example, Tim Harford, the "Undercover Economist" of the Financial Times (2014) in an article titled "Casinos worrying knack for consumer manipulation" has argued that "it is hard for a free-market enthusiast like me to look unblinkingly at ... (slot) machines, designed by an elite and needing little human

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[1]   In this paper, we use the US term "slot machines" or "slots" even though these machines are referred to by various alternative names around the world. They are also known in the United States as "video poker", in Canada as "video lottery terminals" (VLTs), in Britain as "fruit machines" and in Australia and New Zealand as "pokies". They are also known by the more generic term Electronic Gaming Machines (EGMs) (Smith and Campbell, 2007).

[2]   There is a large literature examining how exogenous shocks of various kinds impact bankruptcy filings (e.g., Fay, Hurst, and White, 2002; Gross and Souleles, 2002; White, 2011; Gross and Notowidigdo, 2011; Hankins, Hoekstra, and Skiba, 2011; Gross, Notowidigdo, and Wang, 2014; Dobbie and Song, 2015, Li, White and Zhu, 2011, White, 2007, White, 2011). Using the same Canadian bankruptcy data as used in this paper, Mikhed and Scholnick (2015) examine an exogenous government cash payment as an exogenous shock, and Agarwal, Mikhed and Scholnick (2015) examine lottery winnings as an exogenous shock.

intervention, drawing in consumers, soothing them, entertaining them and eating their money – and not to feel that the invisible hand has slipped."

In spite of the concerns about the possible addictive dangers of slot machines, governments in many jurisdictions around the world have recently chosen to allow a rapid increase in the availability of slot machines, largely because of the significant revenues that slot machines typically generate for governments (Dow Schull, 2012, 2013). In the USA for example, while 31 states permitted slots in 2000, this number had increased to 41 states by 2012. In 2013, slots accounted for approximately three quarters of all gambling revenue in the USA (Dow Schull 2012, 2013).

Even though the existing literature has examined the possibly negative impacts of slot machines in a variety of other disciplines (e.g. psychology, neuroscience, geography, public health, etc.), much of this research is based on non-random samples (e.g. self-identified problem gamblers or neuroscience laboratory participants).[3] There is a particular scarcity of studies in the economics literature examining the consequences of slot machines using rigorous causal methodologies and large administrative datasets. In this paper, we aim to provide causal econometric evidence on the financial consequences of slot machines by exploiting a natural experiment design, and using very detailed administrative data on the universe of slot machine locations,[4] and the universe of individual bankruptcy filers,[5] in the Canadian province of Alberta.

Our natural experiment identification strategy exploits a particular slot machine reduction policy, implemented by the gambling regulator in the province of Alberta, the Alberta Gaming and Liquor Commission (AGLC). The AGLC has the regulatory power to determine the exact location of every slot machine in the province of Alberta. While the total number of slot machines in the province remained fixed by legislation, in the

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[3] While the economics based literature on the harms caused by slot machines is very scarce, a large literature has examined the harm caused by slot machines in other fields. These include Blaszczynski (2013), Afifi et al, (2010), Buchanan et al (2009), Rintoul et al (2013), Grifiths (1999), Hodgins et al (2012), Lund (2009), Mishra et al (2010), Wheeler et al (2011), Young and Tayler (2008).

[4] A variety of studies have examined gambling issues in the Geography literature including, Doran et al (2007), Markham et al (2014), McMillen and Doran (2006), Storer et al (2009), Wheeler et al (2006), Young et al (2009), Young, Markham and Doran (2012 A and B).

[5] Studies that have specifically linked gambling to bankruptcy include Barron et al (2002), Daraban and Thies (2011), Garrett and Nichols (2008), Grote and Metheson (2013), Komoto (2014), Thalheimer (2004).

period from 2001 to date, the AGLC implemented a long term and deliberate policy of reducing the total number of slot machine locations, by removing slot machines from neighborhood bars and concentrating its machines in a few lager facilities (mini-casinos). Our natural experiment concerns the very small neighborhoods (of fractions of a kilometer) surrounding those neighborhood bars which had their slot machines exogenously removed by the gambling regulator under this policy. Using a difference-in-differences methodology, we examine bankruptcies in the neighborhood before and after the exogenous removal of the slot machines from the specific bar, in order to provide causal evidence on the effect of the removal of slot machines on bankruptcies in the immediate neighborhood.

When evaluating such neighborhood level government policies (treatments) on residents of neighborhood, there are various issues regarding selection of neighborhoods on unobservables that need to be controlled for by the econometrician. The first is the possibility that *individuals* have selected to locate in the neighborhood based on unobservables, such that individuals with certain unobservable characteristics cluster within specific neighborhoods. These unobservable characteristics could be correlated with the outcome of interest (in our case bankruptcies), resulting in omitted variable bias.

In order to address this type of selection on unobservables, we follow the large literature which has examined the effect of various neighborhood (dis)amenities on various outcomes.[6] The central methodological element of this literature is to control for household level unobservables by using very close neighbors of the (dis)amenity as the treated group (inner rings), and neighbors slightly further away as the control group (outer rings). Typically these rings have radii of fractions of a kilometer. In brief, the argument used by this literature is that correlated household unobservables within a neighborhood (e.g. selection into the neighborhood by individuals with similar characteristics, or unobservable neighborhood shocks that affect all households in the

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[6] Studies using the inner ring minus the outer ring methodology include: Linden and Rockoff (2008) (sex offender location on neighborhood house prices), Currie, DellaVigna, Moretti, and Pathania (2010) (fast food restaurants on neighborhood obesity), Campbell, Giglio and Pathak (2011) (foreclosure on neighborhood house prices), Currie, Greenstone, and Moretti (2011) (Superfund Cleanup and neighboring infant health), Currie and Walker (2011) (traffic congestion and infant health), Pope and Pope (2014) (Walmart openings on neighbourhood house prices), Currie and Tekin (2015) (foreclosure on neighbourhood hospital visits) and Currie, Davis, Greenstone and Walker (2015) (toxic plants on neighborhood infant health).

neighborhood) will exist across both the inner as well as outer rings, because of the very small geographic sizes of the rings. Thus by "differencing out" inner and outer rings, it is possible to control for correlated household level unobservables within a neighborhood.

The second type of neighborhood unobservable we need to control for concerns government selection of which neighborhood to treat based on unobservables. For example, it may be possible that when a government can implement a policy treatment (e.g. the removal of slot machines from bars) in some but not all neighborhoods, that it could select which neighborhood to treat based on unobservable political considerations (e.g. neighborhoods where it would like to win more votes in the next election). An important element of our context is that slot machines where removed by the government regulator (AGLC) from some but not all neighborhoods, and where removed over a long period of time, rather than all at once. Thus we have a case where the decision by the regulator of *which* neighborhood bar to have its slot machines removed, as well as the decision as to *when* the machines would be removed, could possibly be biased based on selection by the government regulator on politically motivated unobservables.[7] In order to address this possible government selection on unobservables, we exploit the specific institutional nature of the selection process implemented by the AGLC regulator, which we argue can be considered a natural experiment.

In brief, the AGLC regulator has a profit (or government revenue) maximizing mandate, where the 85% of all revenues from slot machines accrue to the provincial government (the remainder accrues to the bar owner). This revenue maximizing motivation of the government regulator is a central element of the natural experiment we use in our identification strategy. Specifically, the AGLC decision rule to determine exactly which bar would face the removal of its slot machines, is that bars providing the lowest revenue per slot machine to the AGLC will have their slot machines removed. This follows directly from the revenue maximizing mandate of the AGLC, and implies that the selection of bars to face the removal of their slot machines is unlikely to be

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[7] The issue of politically related unobservables impacting the choice of which neighborhood governments select for treatment is the main reason why governments sometimes use explicit randomization in the choice of neighborhood to treat. An example is the "Moving to Opportunity" study where governments moved poor households to randomly assigned new neighborhoods (see e.g. Kling, Liebman and Katz (2007), Kling, Ludwig and Katz, (2007), Chetty, Hendren and Katz (2016)). Algan, Hémet , and Laitin (2016) also exploit randomization in the allocation of neighborhoods.

driven by unobservable political considerations, and can thus be considered plausibly exogenous with respect to the regulator.

In addition, we argue that the *timing* of the removal of slot machines from a bar can also be considered plausibly exogenous with respect to the regulator. This is because of the AGLC's revenue (profit) maximizing mandate to ensure that all slot machines, allowed under the law, are continuously accessible at all times in order to maximize revenue. In order to ensure that all slots machines where continuously operating, the timing of the removal of slots from a bar by the AGLC depended on the unrelated timing of the construction and the opening date of a new mini-casino location at some other, unrelated, geographic location somewhere else in Alberta.

An additional element of our empirical specification is that because we can observe the dollar amount of gambling for each operating slot machine in the province for each month, our data allow us to observe the dollar magnitude of the gambling removed from each neighborhood following the removal of slot machines from the neighborhood bar. This allows us to include in our identification strategy variation in the *intensity* of treatment (i.e. variation of the dollar amount of gambling removed from each neighborhood). Such data are very rare in the literature on neighborhood disamenities (cited in footnote 6).

Our main finding is that the removal of slot machines from a bar significantly reduces the number of bankruptcies in the inner ring relative to the outer ring, two years after the removal of the slots. In terms of economic magnitudes, we find that a removal of 1% in total dollars gambled on slots in the year prior to the closure, causes a reduction of approximately 0.0015 bankruptcies per postal code per year, from a base of 0.159 bankruptcies per postal code per year, which is a 1% reduction. These effects are largest (between 2 and 3%) when neighbors are very close to the bar with slots removed (within a quarter kilometer), but decrease in size to below 1% when neighbors are slightly further away (within a half-kilometer or within three-quarters of a kilometer).

2. Institutional Background and Data Sources

2.1. Slot Machine Design

A large literature has argued that various elements of slot machine design could generate possibly harmful outcomes through gambling addiction (for surveys of this literature see Dow Schull, 2012; Australian Government, 2010). Slot machines are specifically designed to have a very high "event frequency" (a new gamble every 3 seconds is typical), which has been argued to be correlated with addictive outcomes (see Griffiths 1999, Smith and Wynne 2004, and other surveys of this behavioral psychology literature above). The behavioral psychology literature has also identified various other mechanisms by which slot machine design could facilitate gambling addiction, including (1) "false wins" (where the gambler is congratulated after "winning" a smaller amount than was gambled), (2) "close misses" (where the gambler is shown how close they were to winning a large prize, in spite of winning no prize), and (3) creation of the "illusion of skill" (where the gambler is led to believe they can control outcomes).

2.2. Slot Machines in Alberta

The Alberta Gaming and Liquor Commission (AGLC) an arms-length agency of the Government of Alberta, regulates all slot machines in Alberta.[8] The AGLC is both the *regulator* of gambling across the province (with a mandate to implement government legislation and policies), as well as gambling *operator* on behalf of the province (with a mandate to maximizing government revenue from gambling, within the relevant legislation and policies). The AGLC license to operate slot machines is provided to private slot machine operators (owners of bars) and can be withdrawn at any moment.

The AGLC regulator has provided us with monthly data for the universe of retail slot locations in the province. These data include six-digit postal code for each slots location and the exact dates that slots where introduced or withdrawn from a location. We

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[8] A variety of studies have also examined the political context of slot machines, and the possible role for regulation including Grinols and Mustard (2006), Dow Schull (2012), Livingstone and Woolley (2007), Doughney (2006), Buchanan and Johnson, (2009). Examinations of regulatory issues in the Canadian and Alberta context include Smith and Campbell (2007), Smith and Wynne (2004), Williams, Belanger and Arthur (2011) and Smith et al (2012), El Guebaly et al (2008), Williams and Wood (2004), Williams et al (2012).

can also observe monthly data on the total dollars gambled and the number of slot machines at each location. All of these data are measured without error, because every slot machine transaction in the province is processed from the central AGLC computer, which is the source of all our data. Summary statistics of these data are shown in Table 1, and Figures 1, 2 and 3.

The AGLC can place slot machines in various categories of locations. During the entire period of our study, Alberta provincial legislation mandated that the total number of slots in bars and mini-casinos could not exceed 6,000. While slots located in bars and mini-casinos together cannot exceed 6,000 machines, slots can also be located in large city-wide casinos and race tracks (which are governed by other legislation to that examined here). Because our identification strategy exploits very small neighborhoods within fractions of a kilometer of the bar with removed slots, our tests only focus on individual bars, which will have a neighborhood level catchment area of fractions of a kilometer. It may not be appropriate to use our methodology to examine mini-casinos, city wide casinos or race tracks because their customer catchment areas are very much larger than the very small neighborhoods that are central to our identification strategy. In order to provide the cleanest test, the focus of this paper is only on the effect of the removal of slot machines from specific bars on bankruptcy filings of very close neighbors of that bar (within fractions of a kilometer), where there are no other slot machines of any kind operating in that neighborhood.

Every physical retail slot machine terminal across Alberta is essentially identical in terms of the games offered as well as the payout ratio provided. This is because the machines are purchased in bulk by the AGLC from slot machine manufacturers. They are all provided directly by the AGLC to individual locations, and are updated at regular intervals. The location owner receives 15% of all net revenue (after prizes) from the slots, with the other 85% being returned to the AGLC as general revenue for the Government of Alberta. Because the payout ratio is fixed across the province, any revenue generated per slot machine is thus a direct function of the extent of gambling per machine at each location. The implication of this is that slot locations cannot compete based on the actual characteristics of the slot machines, which are essentially the same across all slot machines in the province.

2.3. The Natural Experiment: Exogenous Removal of Slot Machines from Bars

Our natural experiment is described in the AGLC 2012 Annual Report (p. 33) as follows: *"Based on the recommendations from the 2001 Gaming Licensing Policy Review... the AGLC continues to reduce accessibility to [slots] by reducing the number of locations providing [slots] to Albertans. Since 2001, the number of [slot] locations in Alberta have been reduced by over 23 per cent."* The aim of this policy was to reduce the possible harm from slot machines, by locating an increasing fraction of slots in locations where individuals had to intentionally travel to in order to gamble (e.g. mini-casinos), rather than in more accessible locations such as neighborhood bars. Empirical evidence on this natural experiment is displayed in Figures 1, 2 and 3.

We argue that the evidence displayed in Figures 1 and 2 provide empirical support for our argument that the *timing* of the removal of a slot machine from a bar can be considered plausibly exogenous. Figure 1 shows that during the course of our study (2003-2013), the AGLC concentrated its slot machines in a smaller number of locations, by significantly reducing the number of bars providing slot machines, while moving those removed slots to larger mini-casinos.

Because of the strict, legally mandated, ceiling on the total number of bar and mini-casino slot machines in the province, a key motivation of the revenue maximizing AGLC regulator was to ensure that all 6,000 slot machines approved by the Province where continuously operating at any given time, rather than, in the words of an AGLC official, "gathering dust in a warehouse". Figure 2 shows that over this period, while the number of slot locations was significantly reduced (Figure 1), the total number of slot machines in operation each month remained constant at the 6,000 ceiling imposed by provincial legislation.[9] This is because the AGLC had an incentive to only remove slots from bar locations at the unrelated point of time when a new mini-casino facility, at some unrelated geographic location, had been constructed and was able to start operating. Thus because the timing and location of the new mini-casino opening can be considered exogenous with respect to the bar losing its slot machines, the exact date at which a specific bar had its slot machines removed, can be considered plausibly exogenous.

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[9] Figure 2 shows that in a few months the total number of slot machines in the province was slightly above the legislated maximum of 6,000 machines. The reason for this is double counting in our data, when slot machines where moved from one location to another within the month.

While Figures 1 and 2 provide evidence concerning the plausible exogeneity of *when* a bar had its slot machines removed, Figure 3 provides evidence that *which* bar had its slot machines removed, was not driven by unobservable political motivations of the government regulator. As described above, AGLC policy in terms of which bar to remove slots from was driven by its revenue maximizing mandate. Thus its decision rule for removing slot machines from a bar is that bars with the lowest revenue per machine, relative to all other bars in Alberta, would have their slots removed. Evidence for this is displayed in Figure 3, which compares monthly revenue per slot machine for locations that had their slot machines removed by the AGLC, compared to monthly revenue per slot machine that continued to operate through our data period, without any changes imposed by the AGLC. As can be seen in Figure 3, the revenue per slot machine of closed locations was significantly lower than the revenue per machine that was allowed to operate continuously.[10]

An additional element of the AGLC decision criteria, as to which bar to remove slots from, is that individual bar owners are not able to manipulate their slots revenues in order to avoid the AGLC removing slot machines from their bar. All slot machines in the province are essentially identical, because all are provided by the AGLC, thus it is not possible for a bar owner to increase slot revenue by changing the characteristics of their machines. Furthermore, all bar owners can be assumed to be revenue maximizers in all periods (because of the 85% - 15% revenue split with the provincial government), whether or not they are facing an imminent threat to that the AGLC will remove their slot machines. It is difficult, therefore, for any bar owner to specifically attempt to increase revenues in response to an impending threat by the AGLC to remove slot machines from that bar.[11] In addition, the mechanics of this decision rule are transparent to bar owners. Bar owners are aware of revenues generated by competing bars, because average slot machine revenues are reported each year in the AGLC Annual Report.

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[10] Data in Figure 3 reflects all slot machines in Alberta. We describe below the sample selection criteria we use to select locations to examine in our study (e.g. restricting our sample to locations with no other proximate slot machine locations).

[11] Similar decision rules occur in various sports leagues (e.g. English Premiership Soccer) where the worst performing teams are automatically removed from the league each year. As in our context, the key issue in this decision rule is that no poorly performing team is able to manipulate the decision rule in order to avoid removal from the league.

Another important institutional detail is the existence of a very large waiting list of bar owners wanting to install slot machines, but who were not able to because of the ceiling of 6,000 slot machines imposed by the Province. This large waiting list is an indication of the strong incentives for bar owners to have slot machines in their locations, both because of the direct revenue from the slot machine they receive, but possibly more importantly because of the increased alcohol sales from users of the slot machines. It can be argued that this situation of strong excess demand by bar owners for slot machines (as indicated by the large waiting list) is consistent with our argument that the decision of the AGLC to remove the slot machine from an individual bar, can be considered plausibly exogenous with respect to that individual bar owner, because such a removal will be detrimental to the bar owner.

It is possible that a bar owner could unilaterally decide to close down her bar leading to the removal of slot machines from that location. However, based on discussions with AGLC managers it is much more common that a bar owner would sell her bar as a going concern rather than shutting it down completely. This is because such a bar would have two very valuable assets: (1) a liquor license, as well as (2) an allocation of slot machines, that would both lose all of their value if the bar was shut down, but would both retain their value if the bar was sold as a going concern.

2.4. Measuring the Intensity of Treatment

An important element of our research is our focus on measures of the continuous intensity of treatment. In our model, treatment is defined as the removal of slots from a bar, while the *intensity* of treatment is defined as the dollar magnitude of gambling removed from each bar. Recall that our data allow us to observe the dollar amount of gambling at each bar for each month that the slot machines are operating. Because we are able to observe the dollar magnitudes of gambling across bars, we are able to provide evidence on the extent of bankruptcy caused by each extra dollar of gambling removed from a neighborhood bar (as measured in the year prior to the removal of the slots). An important contribution of our study to the neighborhood disamenity literature, therefore, is that we are able to quantify the size of the disamenity across locations ($ amount of gambling).

This kind of data is often unavailable in other studies examining various other disamenities, thus those studies are restricted to using a binary (treatment/no treatment) variable in their specifications, rather than the continuous intensity of treatment (dollars of gambling removed from the neighborhood) that we can use in our study. In some studies in the neighborhood disamenity literature, the disamenity cannot, by definition, be measured with a continuous variable (e.g. sex offender locations (Linden and Rockoff, 2008) or foreclosure locations (Campbell, Giglio and Pathak, 2011). In other studies, researchers do not have access to data on the revenue or profitability of specific neighborhood branches of the businesses being considered as disamenities (e.g. fast food locations (Currie, DellaVigna, Moretti, and Pathania, 2010), or Walmart Stores (Pope and Pope, 2014), thus they are required to use binary rather than continuous measures of treatment. In their neighborhood pollution based studies, Currie et al (2011) and Currie et al (2015) have noted the importance of controlling for variations in the intensity of treatment across locations (in their case, the amount of pollution across different neighborhoods), when using specifications that are similar to ours, although they note that such data are often unavailable.[12]

2.5. Consumer Bankruptcy in Canada

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 US), our data include the complete universe of every bankruptcy filing in Canada. This OSB database includes 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. See Table 1 for summary statistics on these data.

2.6. Geographic Data

We can match our bankruptcy and slot location data because we can observe the exact six digit postal code of every location in both databases. Postal codes are extremely

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[12] Currie et al (2011) are able to use a more limited binary, rather than continuous measure, to distinguish between more and less dangerous Superfund cleanup sites, as measured before the site cleanup. Their key finding is that when splitting the sample into two subsamples, more dangerous sites cause more harmful effects compared to less dangerous sites, implying that variations in the intensities of treatments across locations are important determinants of neighborhood outcomes.

small geographic areas, with a median of 16 households. There are often multiple postal codes within a single city block or within a single apartment building. We can observe the exact longitude and latitude of the centroid of each postal code, thus we can accurately calculate the distance from any postal code to any other postal code.

We can also match every postal code to a larger geographic area, called a dissemination area (DA), which contains 200 households on average, with an average area of 0.2 square kilometers. A large amount of neighborhood level census data is available at the DA level. Because we can observe the exact postal code of every bar as well as every bankruptcy filer we can include a large amount of neighborhood level data in our models to control for observable confounding factors. Census data available to us includes all the standard census demographics, including income, unemployment, age, education, homeownership, gender, as measured at the neighborhood DA level.

No observable neighborhood data exist at the postal code level, largely because of their extremely small size. We are however able to observe the total amount of postal codes in each DA, as well as the total population per DA. We can thus determine an average population size per postal code in each DA, based on the assumption that all postal codes in a DA have the same population. This assumption is motivated by the fact that DAs are designed by Census Canada to be homogeneous in terms of population density and other neighborhood characteristics. We include the average population size per post code as a control variable in all regressions.

3. Research Design

3.1. Sample Selection

We define the event in our distributed lag model as the date of the exogenous removal of all slot machines from a specific bar. Our aim in our sample selection design is to provide the cleanest test of the effect of the removal of slot machines on neighborhood bankruptcies. The cleanest experiment in our context would occur if there is only a single slot removal event in a specific neighborhood, and no other sources of supply of slot machines over the course of the sample in that neighborhood.

In order to create this clean natural experiment, we define our main sample of slot machine closure events to only include bars where there is a single slot closure event

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during the sample, with no other slot openings or slot closures at that bar. This is to avoid the problem of interpreting multiple events within a single event window. As we describe below, our event windows are long (from 4 years before to 4 years after the event date), to reflect findings in the bankruptcy literature that the lags between an exogenous shock and a bankruptcy filing can be long and variable (e.g. Hankins, Hoekstra, and Skiba, 2011). Because of the difficulties of interpreting such closure and subsequent reopening type events in our econometric framework with very long event windows, we remove all such closing and subsequent reopening events from our sample.

Some opening and closing events in our data reflect episodes of a bar owner selling her bar (including slot license) as a going concern. AGLC regulations require a new owner of a bar providing slot machines to undergo a criminal background check, which takes some time. When this transition occurs we can observe a gap in slot machines operating at that specific location. Including such locations (with time periods with no slot machines operating) would complicate interpretation, thus in our sample we remove all locations where such events occurred. In other words, our sample consists of locations where there was a single closing event over the sample period.

Another restriction in our sample of slot closure events is that we only include locations where there are no other proximate slot locations of any sort, operating at any stage during the course of our sample. We define a proximate slot location as any slot location (bar, mini-casino, etc.) that is situated within the radius of our control group (1.5 km, 2.0 km or 2.5 km). The reason for this restriction is that if there was an alternative slot location operating close by to the bar with removed slots this could result in geographic substitution between neighboring locations, and thus impact our main results of interest. The possibility of geographic substitution between locations is particularly relevant, given that all slot machines across locations in the province are essentially identical, because all are provided by the AGLC regulator to individual location owners. This restriction removes a significant number of bar locations from our sample, because many bars agglomerate in specific areas.

A related data restriction we impose is that we only include as events those instances where the AGLC removed all slot machines from a bar at a specific date. In some instances, the AGLC removed some, but not all, slot machines from a bar. Such

episodes are also difficult to interpret in the context of our tests, because gamblers could use the remaining slot machines more intensively, thus we also do not include such instances as events in our tests.

Based on these sample selection criteria, we are able to define 39 events of the AGLC exogenously removing all slot machines from a bar. Our unit of analysis is postal codes in the areas surrounding those 39 slots closures. We define our treatment and control groups based on a radius from the slot closure. In total, we observe 689 specific postal codes within 1.5 km radius of a closure. Similarly, for the 2.0 km radius, we include 776 postcodes; and we observe 864 postal codes in the 2.5 km radius. All these geographic units are observed for the total of nine years (four before, year of closure, and four after).

3.2. Identification Using Inner and Outer Rings

Methodologically, our main specification uses spatial distributed lag models to identify causal impacts by possibly negative disamenities on very close neighbors. Our identification strategy is similar to a number of recent studies that examine the impact of neighborhood disamenities on very close neighbors. Other papers that have used this approach include Linden and Rockoff (2008) (sex offender location on neighborhood house prices), Currie, DellaVigna, Moretti, and Pathania (2010) (fast food restaurants on neighborhood obesity), Campbell, Giglio and Pathak (2011) (foreclosure on neighborhood house prices), Currie, Greenstone, and Moretti (2011) (Superfund Cleanup and neighboring infant health), Pope and Pope (2014) (Walmart openings on neighbourhood house prices), Currie and Tekin (2015) (foreclosure on neighbourhood hospital visits) and Currie, Davis, Greenstone and Walker (2015) (toxic plants on neighborhood infant health).

The key to the identification strategies used in these papers, is the definition of neighbors who are very close (typically fractions of a kilometer away) to the disamenity as the treated group (labeled inner rings), and the definition of neighbors who are slightly further away as the control group (labeled outer rings). The intuition behind this identification strategy is that using very close inner ring neighbors as treatment groups, and outer ring neighbors as control groups can help to control for any unobserved

common attributes (e.g. unobservable shocks) that are shared by residents of *both* the inner rings and outer rings. The key assumption in this literature is that because both inner rings and outer rings are very small, the unobserved local shocks should affect them similarly. Campbell, Giglio and Pathak (2011), for example, argue that "if there is a common shock in the neighborhood which generates an overall … trend within this micro geography, it will be captured by the difference between these two groups." (p. 2125).

A second possible source of endogeneity is non-random selection into neighborhoods, either by individuals (e.g. gamblers) or by facilities (e.g. owners of bars providing slots). Selection issues can arise if gamblers select to locate close to slots, or alternatively if slot operators select to locate close to gamblers. In both of these cases, our methodology exploits the fact that at the very small distances involved (fractions of a kilometer) the supply of locations available at the time individuals or facilities owners decide to move into the neighborhood will be very limited. For example, Linden and Rockoff (2008) argue that "individuals may choose neighborhoods with specific characteristics, but, within a fraction of a mile, the exact locations available at the time individuals seek to move into a neighborhood are arguably exogenous" (p. 1110). Similarly, Currie et al (2010) in their study of fast food restaurants and obesity in schools argue that "we only require that, within a quarter of a mile from a school, the exact location of a new restaurant opening is determined by *idiosyncratic* factors such as where suitable locations become available" (p. 34, italics added).

4. Econometric Methodology

Using a distributed lag model that is similar to that of Gallagher (2014), Agarwal, Liu, Souleles (2007), and Agarwal and Qian (2014), our main specification is the following distributed lag model interacted with continuous intensity of treatment:

$$Y_{ijt} = \beta_0 + \sum_{s=-4}^{4} \beta_{1s} Near_{ij} \times T_s \times \ln(Gambling)_j + \beta_3 \ln(Gambling)_j \times$$
$$Near_{ij} + \sum_{s=-4}^{4} \beta_{4s} Near_{ij} \times T_s + \sum_{s=-4}^{4} \beta_{5s} \ln(Gambling)_j \times T_s +$$
$$\beta_6 \ln(Gambling)_j + \beta_7 Near_{ij} + \alpha_t + \beta_9 (Controls)_{da} + \delta_j +$$
$$\varepsilon_{ijt,} \tag{1}$$

16

where Y_{ijt} is the number of bankruptcies in postal code i near bar j in year t. Our sample includes all postal codes that are within the outer and inner rings for each event (the removal of slots from bar j), for each year t. Our sample thus consists of observations for postal code-years. Our dependent variable captures the number of bankruptcy filings in each postal code per year of the study.

The *Near* variable is an indicator variable equal to 1 for the postal codes in the inner rings and 0 for the postal codes in the outer rings. In terms of ring classification, inner rings are either 0.25 km or 0.5 km or 0.75 km from the slot location, and outer rings are either 1.5 km or 2.0 km or 2.5 km from the slot location. The time invariant intensity of treatment term, *Gambling* is the log of the dollar amount of gambling in the bar with removed slots in the 12-month period before the closure. T represents a set of indicator variables equal to 1 for a particular year relative to the closure year = 0 (e.g. T_2 is equal to 1 in the second year after closure and it is zero otherwise).

Our main coefficients of interest are β_{1s} which are coefficients on the three-way interaction term of *Near,* with the indicator variables for the years relative to closure (T), and with *Gambling* (intensity of treatment measured as the log of dollars gambled in this location in the 12 months before its closure). In other words, the coefficients on this triple interaction terms β_{1s} measure the effect of a removal of the specific $ amount of gambling from the inner ring (relative to the outer ring) for each year relative to the closure year. As is standard when including a three-way interaction term, we also include all sub components of this term, including all three two-way interaction terms as well as all three terms without any interactions.

In terms of defining the *Gambling* term (dollar amount of gambling removed) we define the last full 12 months before the date of the closure as a time invariant variable of the total dollar amount of gambling at that location. Even though we do have time varying data for these variables, these data are coded as zero in all periods after the date of the removal of the slot machines. Thus if we were to use time varying measures, any interaction term including the *Gambling* variable would take a value of zero in all periods after the closure. Because we use a time invariant definition of *Gambling*, the one-way *Gambling* variable, will be perfectly correlated with the location fixed effect variable, δ_j, thus we drop the term $ln(Gambling)_j$ from our specifications.

Our event windows are very long (4 years before and 4 years after the event date) because of the long lags between an exogenous shock and a bankruptcy filing in the bankruptcy literature. In this specification we use the year prior to the slot machine removal event date (year t = -1) as the omitted year, and compare every other year in our event period (from year t = - 4 to year t = 4) to that year.

In order to control for the possible correlation of the residuals within a specific slot location we cluster standard errors by slot location. Our main specification also includes bar location fixed effects δ_j to account for any unobserved differences between the bars j. Furthermore, all specifications include calendar year fixed effects, α_t, which capture macro time trends that could impact bankruptcy decisions. We do not include event time fixed effects (T) by themselves into any regressions because they are perfectly correlated with calendar year fixed effects. Our main specification also includes a large amount of DA level observables (with subscript da), taken from Statistics Canada Census data.

If there is a causal relationship between slot location closures and bankruptcy filers (in the inner ring relative to the outer ring), we would expect significant coefficients on β_{1s} in the years after the event (s > 0), but not in the years before the event (s < 0). We can test the parallel trends assumption of the model by testing whether coefficients β_{1s} are statistically insignificant in each of the years before the slots removal event. A negative coefficient on the coefficients β_{1s} in the years after the event implies that there should be a reduction in bankruptcies following the removal of slot machines from a specific bar, when comparing (1) bankruptcies in inner rings compared to outer rings, (2) bankruptcies in various years relative to bankruptcies in year t = -1, and (3) when accounting for the magnitude of gambling withdrawn because of the closure.

5. Main Results

Our main results for this specification, and the main results of our paper, are reported in Table 2. Each column in this table is a separate regression using different sizes of inner and outer rings. We use three inner ring radii (0.25 km, 0.5 km and 0.75 km) and three outer ring radii (1.5 km, 2.0 km and 2.5 km), thus we report results for a total of nine different ring size combinations. The increasing number of observations as

the outer ring gets larger reflects the fact that there are more postal codes as the outer ring size increases. The rows in these tables report results for each individual year from t = -4 to t = 4, where year t = -1 is the omitted year.

The main results from Table 2 are the significant and negative coefficients in year 2, across all nine ring-size specifications (columns). These results imply that there is a significant reduction in inner ring bankruptcies compared to outer ring bankruptcies, two years after the removal of slots from a bar.

We can use the coefficients reported in Table 2 to derive economic magnitudes. Our main conclusion in this regard is that the economic magnitudes fall as the size of the inner ring radii increases (from 0.25 km to 0.5 km to 0.75 km). On the other hand, the economic magnitudes are somewhat similar across the various sizes of outer ring radii (1.5 km, 2.0 km and 2.5 km). For example, when the inner ring is 0.25 km, for each additional 1% of dollar gambling removed from bars (across all bars with gambling removed) we find that there is a reduction in bankruptcies by 2.3%, 2.4% or 2.9%, depending on the outer ring size. However, when the size of the inner ring increases to 0.5 km we find that a 1% of dollar gambling removed causes a reduction in bankruptcies by 1.4%, 1.4% and 1.8%. Similarly, when the size of the inner ring increases even more to 0.75 km, the corresponding decreases in bankruptcies are 1%, 1% and 1.2%.

The intuition behind these economic magnitudes is that the removal of slot machines from a specific location will have the largest effect on neighborhoods that are the closest to the location – i.e. within a quarter of a kilometer. However, as we increase the radius of the inner ring in different specifications (from a quarter kilometer, to a half kilometer to three quarters of a kilometer), so the magnitude of the effect declines.

It is also important to note that these percentage increases in bankruptcy are from a very low base. Bankruptcy is a relatively rare event, reflecting an extreme form of financial distress. In our sample of postal code years within 1.5 km of the bars with removed slots, we observe 0.173 bankruptcies per postal code per year. This implies that if (as in some specifications reported above) a 1% increase in dollars gambled on removed slots leads to a 1% reduction in bankruptcies, this represents 0.0017 fewer bankruptcies per postal code per year.

In Table 3 we replicate these tests, with the exception that we define the intensity of treatment as the number of slot machines removed from each slot location, rather than the total dollars gambled. As in the case of Table 2 above, in Table 3 we also find that the coefficient for year 2 is negative and significant, as predicted, across all nine inner and outer ring combinations. Based on these results, we can conclude that our main prediction holds, whether we use either total dollars gambled, or alternatively, number of machines, as our intensity of treatment variable. It should be noted, however, that the majority of coefficients in year 2 in Table 3 are only significant at 10%, while in Table 2, the majority of year 2 coefficients are significant at 5%. It is for this reason that we consider our tests using total dollar magnitudes as the intensity of treatment (Table 2) as our main result in this paper.

6. Robustness Checks: Alternative Geographic Fixed Effects

As a robustness check, we closely follow Currie et al (2010), whose main specification includes location fixed effects (in their case, toxic plants), but in a series of robustness checks replace the location fixed effects with zip code fixed effects to capture zip code level unobservable characteristics. Because the slot locations in our sample, are (by design) geographically dispersed, we cannot include both slot location fixed effects as well as other geographic fixed effects at the same time, because slot location fixed effects will be highly correlated with other geographic fixed effects. Thus, following Currie et al (2010), in these robustness tests, we exclude slot location fixed effects and include various other geographic fixed effects. These geographic fixed effects use one of three separate geographic categories (from smallest to largest): (1) the six digit postal code (less than the size of a city block, containing 13 households on average), (2) the dissemination area or DA (containing 200 households on average) and (3) Forward Sortation Area (FSA) (containing on average 8,000 households).

Tables 4, 5 and 6 report the results of these robustness tests. These tables use the same methodology as our main specification (1), with the exception that we replace the slot location fixed effects with FSA effects (Table 4), DA fixed effects (Table 5) and postal code fixed effects (Table 6). The main conclusions from these three tables are that our main results are very robust across all of these specifications. In other words, our

main results do not appear to have been driven by unobservables at the FSA, DA or postal code geographic levels.

7. Conclusion

This paper has provided causal evidence that the removal of slot machines from specific bars reduces bankruptcy filings from very close neighbors, after the removal of the slot machines. These effects are largest for individuals who are very close to the slot location (within 0.25 km of the bar with removed slots), and decline as the distance between the individual and the slot location increases (0.5 km and 0.75 km).

While a number of studies in other disciplines (e.g. psychology and neuroscience) have documented the harm caused by slot machines, this is the first study to document harm from slot machines using a natural experiment and detailed data on personal financial distress and bankruptcy. This study leveraged very large administrative databases on the universe of slots locations and the universe of bankruptcy filings.

Our identification strategy exploits a unique natural experiment where we argue that the timing of the removal of slot machines from a specific bar can be considered plausibly exogenous. The AGLC gambling regulator attempts to keep its 6,000 slot machines, allowed under the law, continuously operating, in order to maximize revenue. For this reason, the AGLC only removes slot machines from a bar once a new mini–casino, located at some unrelated geographic location, has been constructed and opened, at some date unrelated to the bar in question.

Previous research has provided causal evidence that a variety of different neighborhood disamenities (e.g. toxic plants, fast food restaurants, Walmart stores, sex offenders, foreclosed houses) cause negative outcomes for very close neighbors. Our paper contributes to this neighborhood disamenity literature by providing evidence that slot machines can be considered a neighborhood disamenity, in that they causes negative outcomes (in our case, bankruptcies) of very close neighbors.

Our paper also makes an important methodological contribution to the neighborhood disamenity literature, in that it demonstrates the importance of accounting for variation in the intensity of treatment across the specific neighborhoods. An important advantage of our data is that we can observe the exact dollar amount of gambling

withdrawn from each bar, thus we can observe variation in the intensity of treatment. In much of the existing neighborhood disamenity literature it is not possible to observe variation in the intensity of treatment across specific neighborhoods.

Our results also have important policy conclusions, in light of the policy of the Alberta gambling regulator (AGLC) to deliberately reduce the number of slot machines in bars in order to reduce accessibility to slot machines within local neighborhoods. Our results provide evidence to support the underlying premise of this policy choice by the AGLC regulator, in that they show that the removal of slot machines from neighborhood bars significantly reduces bankruptcy filings in very close neighborhoods near these bars.

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Figure 1: Number of Slot Locations in Alberta by Month in 2003-2013
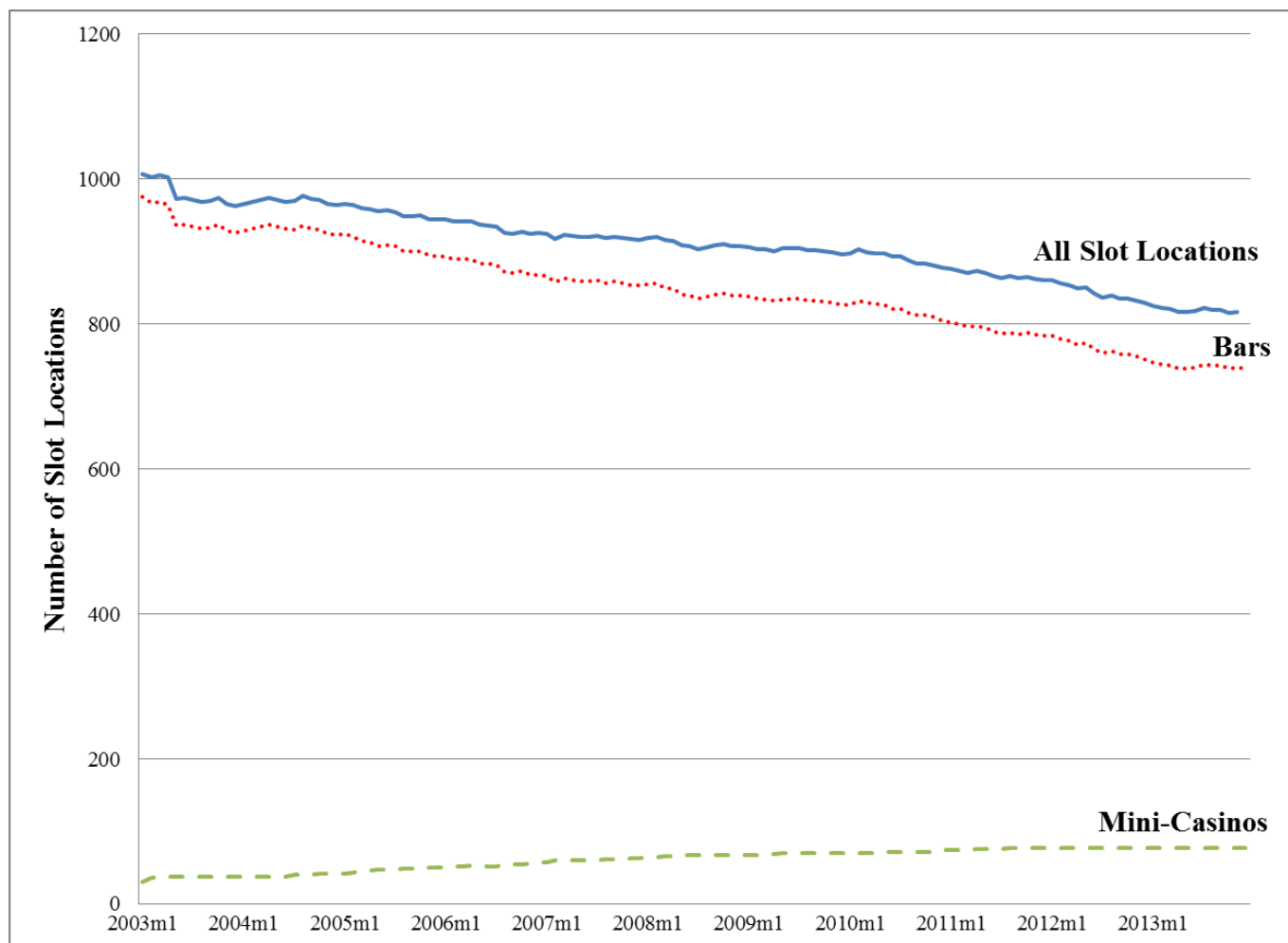
Figure 2: Number of Slot Machines in Alberta by Month in 2003-2013
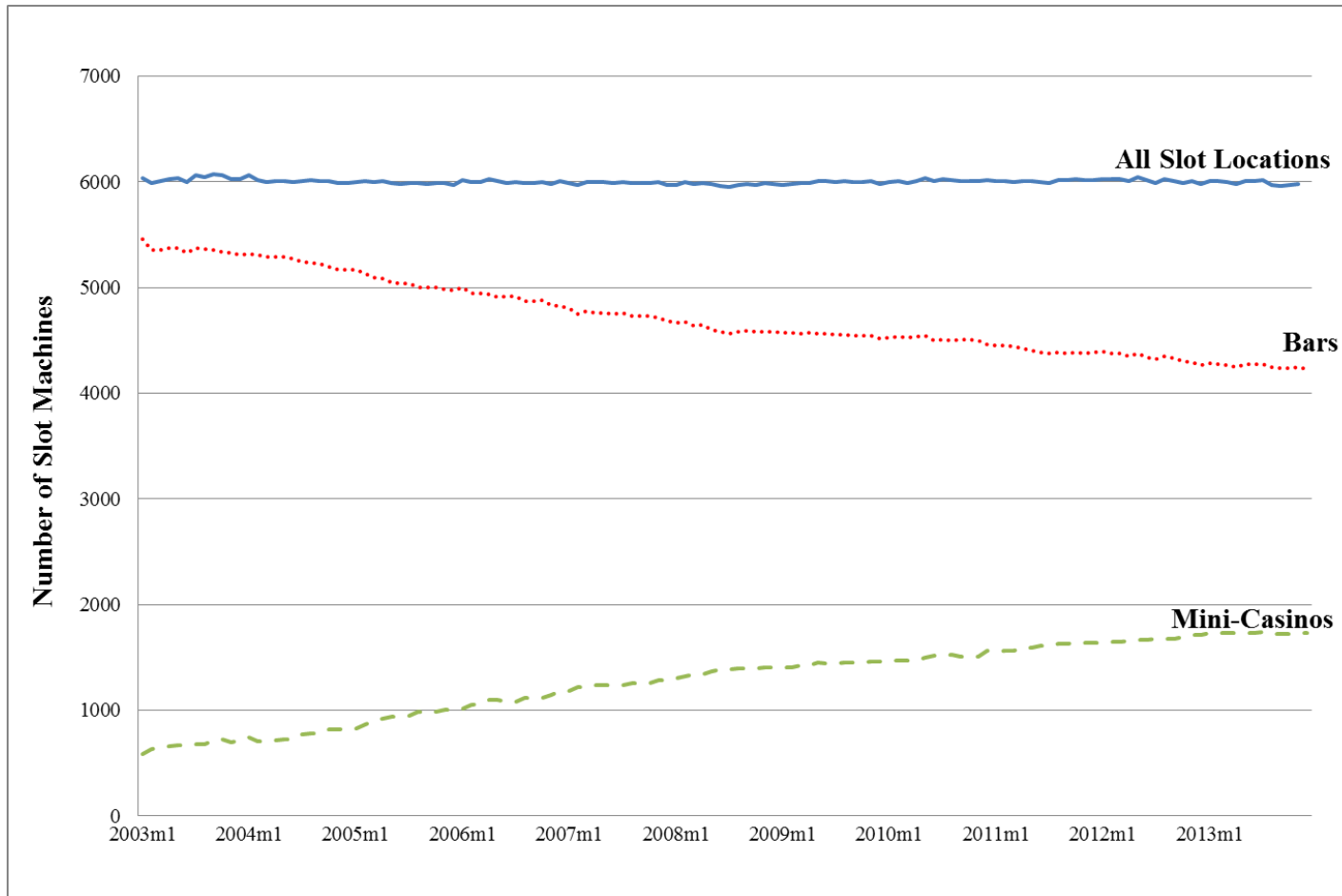
Figure 3: $ Revenue Per Slot Machine in Bars: Closed vs. Continuously Operating Machines; by Month 2003-2012
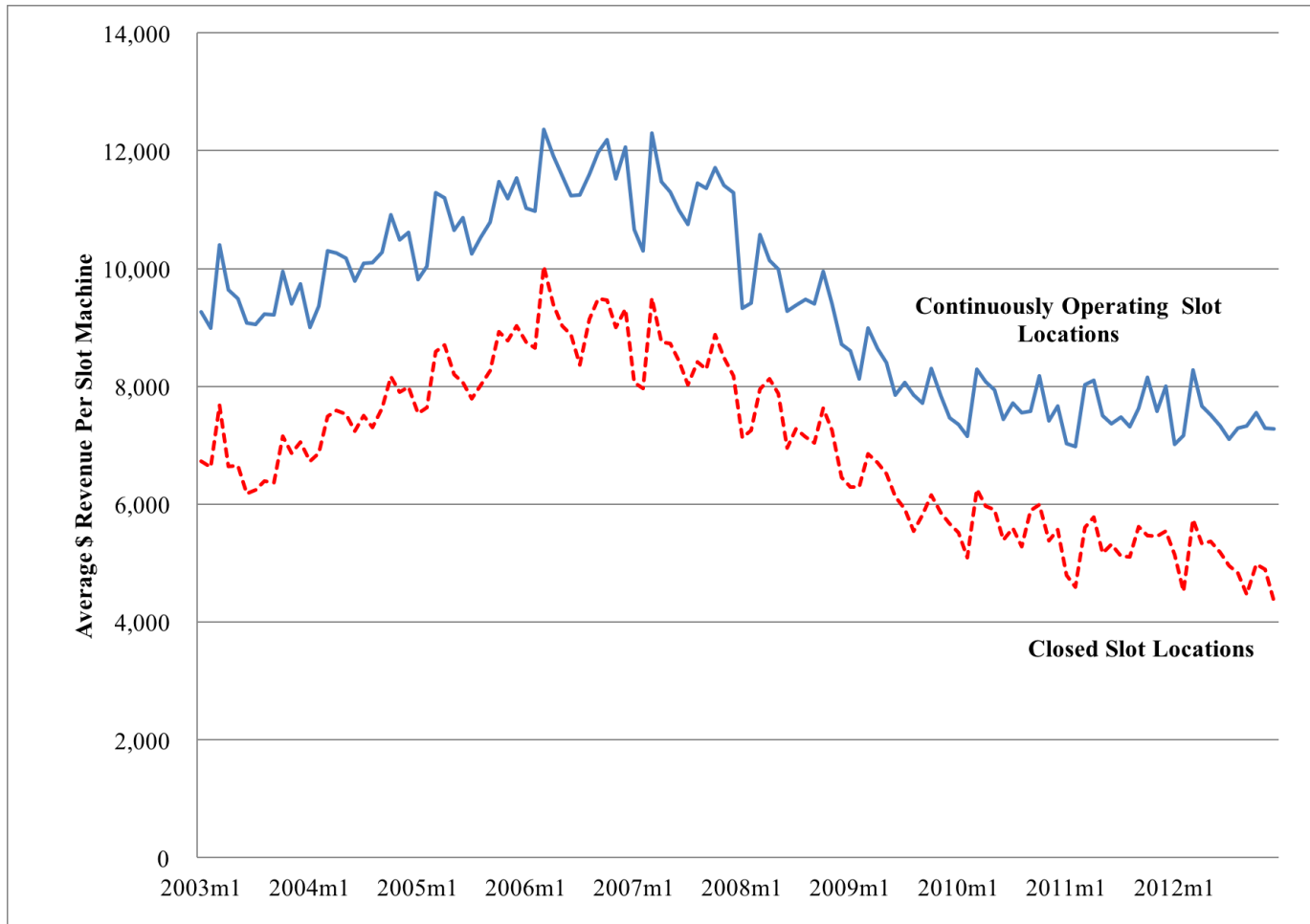
Table1: Summary Statistics

	Obs	Mean	Median	SD
Characteristics of Bars with Slots Removed				
Gambling Removed from Bars ($ Million)	39	0.237	0.134	0.254
Gambling Removed From Bars (log $ Million)	39	11.88	11.8	1.028
Number of Machines Removed from Bars	39	3.517	3	2.131
Gambling Per Machine Removed from Bars ($)	39	56,974	45,820	34,926
Characteristics of Postal Codes within 1.5km of Bars				
Number of Bankruptcy Filings (per Postal Code - Year)	6,215	0.173	0	0.734
Estimated Number of Households per postal code	6,215	23.1	16.1	30.6
Unemployment Rate (% in DA)	6,215	4	4	3
Average Income ($ in DA)	6,215	36,598	35,765	8,173
Marriage Rate (% in DA)	6,215	53.52	54.43	9.13
Completed College (% in DA)	6,215	17.33	16.92	4.5
Recent Immigrants (% in DA)	6,215	7.8	6.45	7.07

Notes: Only the sample using the 1.5 km outer ring is presented here. Summary statistics for the 2 km and 2.5 km outer rings are available upon request. Data on Slot Machines is provided by the AGLC. Data on Bankruptcies per postal code are provided by the Office of the Superintendent of Bankruptcy Canada (OSB). DA level data are provided by Statistics Canada using Census data.

Table 2: The Effect of Slots Removal on Bankruptcy with Dollar Magnitude of Gambling as Intensity of Treatment, and Slot Machine Location Fixed Effects

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outer ring	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km
Inner ring	0.25 km	0.25 km	0.25 km	0.5 km	0.5 km	0.5 km	0.75 km	0.75 km	0.75 km
(year -4)	-0.027	-0.015	-0.045	-0.056	-0.035	-0.042	-0.082	-0.049	-0.044
	(0.189)	(0.180)	(0.176)	(0.120)	(0.114)	(0.109)	(0.072)	(0.069)	(0.066)
(year -3)	-0.118	-0.074	-0.087	-0.063	-0.033	-0.041	-0.075	-0.046	-0.050
	(0.119)	(0.106)	(0.103)	(0.089)	(0.081)	(0.080)	(0.055)	(0.050)	(0.051)
(year -2)	-0.083	-0.070	-0.085	-0.067	-0.056	-0.063	-0.0815*	-0.068	-0.071
	(0.102)	(0.099)	(0.095)	(0.071)	(0.070)	(0.067)	(0.044)	(0.046)	(0.044)
(year 0)	-0.162	-0.173	-0.172	-0.095	-0.108	-0.104	-0.052	-0.067	-0.063
	(0.152)	(0.146)	(0.142)	(0.087)	(0.085)	(0.083)	(0.055)	(0.059)	(0.060)
(year 1)	-0.163	-0.203	-0.217	-0.027	-0.073	-0.085	-0.046	-0.095	-0.104
	(0.145)	(0.146)	(0.149)	(0.102)	(0.106)	(0.110)	(0.051)	(0.057)	(0.065)
(year 2)	-0.405*	-0.386**	-0.445**	-0.238*	-0.230*	-0.275**	-0.166**	-0.156**	-0.186**
	(0.201)	(0.186)	(0.186)	(0.134)	(0.124)	(0.130)	(0.077)	(0.073)	(0.085)
(year 3)	-0.256	-0.249	-0.214	-0.153	-0.152	-0.101	-0.110*	-0.108	-0.061
	(0.169)	(0.164)	(0.162)	(0.109)	(0.109)	(0.110)	(0.064)	(0.066)	(0.067)
(year 4)	-0.149	-0.153	-0.198	-0.063	-0.075	-0.105	-0.044	-0.059	-0.083
	(0.159)	(0.153)	(0.152)	(0.093)	(0.091)	(0.091)	(0.059)	(0.059)	(0.056)
Events (Bars)	39	39	39	39	39	39	39	39	39
Obs (post codes-years)	6,215	6,917	7,709	6,215	6,917	7,709	6,215	6,917	7,709
Number of postal codes	698	776	864	698	776	864	698	776	864
R-squared	0.432	0.427	0.417	0.431	0.426	0.416	0.431	0.425	0.415

Notes: This table reports results for our main test in equation (1). Each column is a separate regression with inner rings and outer rings of different radii as specified in the table header. We only report results from the three-way interaction term interacting Near X Time X Dollar Amount of Gambling. This coefficient measures the effect of the removal of the dollar amount of gambling from the inner ring (relative to the outer ring), for each year (relative to the omitted year t=-1). A negative coefficient implies the removal of slot locations reduces the number of bankruptcies. This specification includes slot location fixed effects, calendar year of bankruptcy fixed effects, and control variables reported in Table 1. Standard errors are clustered by slot location and reported in parentheses.

Table 3: The Effect of Slots Removal on Bankruptcy with Number of Slot Machines Removed as Intensity of Treatment, and Slot Machine Location Fixed Effects

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outer ring	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km
Inner ring	0.25 km	0.25 km	0.25 km	0.5 km	0.5 km	0.5 km	0.75 km	0.75 km	0.75 km
(year -4)	-0.033	-0.029	-0.032	-0.019	-0.015	-0.014	-0.015	-0.009	-0.007
	(0.044)	(0.043)	(0.043)	(0.015)	(0.014)	(0.014)	(0.010)	(0.010)	(0.010)
(year -3)	-0.004	-0.003	-0.003	-0.001	0.000	0.000	-0.005	-0.003	-0.002
	(0.039)	(0.039)	(0.038)	(0.017)	(0.017)	(0.017)	(0.008)	(0.008)	(0.008)
(year -2)	-0.031	-0.031	-0.033	-0.013	-0.013	-0.014	-0.010	-0.009	-0.009
	(0.023)	(0.023)	(0.022)	(0.010)	(0.010)	(0.010)	(0.007)	(0.008)	(0.008)
(year 0)	-0.0682*	-0.0695*	-0.0698*	-0.023	-0.025	-0.024	-0.004	-0.006	-0.005
	(0.036)	(0.036)	(0.036)	(0.014)	(0.015)	(0.015)	(0.010)	(0.011)	(0.011)
(year 1)	-0.057	-0.063	-0.064	-0.004	-0.010	-0.011	-0.008	-0.015	-0.016
	(0.040)	(0.040)	(0.040)	(0.018)	(0.018)	(0.019)	(0.009)	(0.009)	(0.011)
(year 2)	-0.0731**	-0.0714**	-0.0770**	-0.0278*	-0.0266*	-0.0314*	-0.0171*	-0.0148*	-0.0180*
	(0.036)	(0.034)	(0.036)	(0.016)	(0.015)	(0.018)	(0.009)	(0.008)	(0.010)
(year 3)	-0.039	-0.039	-0.034	-0.016	-0.016	-0.008	-0.008	-0.008	-0.001
	(0.031)	(0.031)	(0.030)	(0.013)	(0.013)	(0.013)	(0.007)	(0.007)	(0.007)
(year 4)	-0.041	-0.043	-0.046	-0.004	-0.006	-0.009	0.000	-0.003	-0.006
	(0.030)	(0.030)	(0.029)	(0.012)	(0.012)	(0.011)	(0.008)	(0.009)	(0.007)
Events (Bars)	39	39	39	39	39	39	39	39	39
Obs (post codes-years)	6,215	6,917	7,709	6,215	6,917	7,709	6,215	6,917	7,709
Number of postal codes	698	776	864	698	776	864	698	776	864
R-squared	0.427	0.422	0.412	0.426	0.421	0.411	0.426	0.421	0.411

Notes: This table reports results for our main test (Equation 1). Each column is a separate regression with inner rings and outer rings of different radii as specified in the table header. We only report results from the three-way interaction term interacting Near X Time X Slot Machines Removed. This coefficient measures the effect of the removal of slot machines from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies the removal of slot machines reduces the number of bankruptcies. This specification includes slot location fixed effects, calendar year of bankruptcy fixed effects and control variables reported in Table 1. Standard errors are clustered by slot location location and reported in parentheses.

Table 4: The Effect of Slots Removal on Bankruptcy with Dollar Magnitude of Gambling as Intensity of Treatment, and FSA Fixed Effects

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outer ring	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km
Inner ring	0.25 km	0.25 km	0.25 km	0.5 km	0.5 km	0.5 km	0.75 km	0.75 km	0.75 km
(year -4)	-0.021	-0.012	-0.043	-0.054	-0.034	-0.041	-0.081	-0.048	-0.043
	(0.189)	(0.181)	(0.176)	(0.120)	(0.114)	(0.109)	(0.072)	(0.070)	(0.066)
(year -3)	-0.104	-0.064	-0.078	-0.056	-0.028	-0.036	-0.072	-0.044	-0.047
	(0.118)	(0.106)	(0.103)	(0.088)	(0.081)	(0.080)	(0.055)	(0.050)	(0.051)
(year -2)	-0.074	-0.064	-0.081	-0.062	-0.053	-0.061	-0.0784*	-0.066	-0.069
	(0.103)	(0.099)	(0.095)	(0.071)	(0.071)	(0.068)	(0.044)	(0.047)	(0.045)
(year 0)	-0.167	-0.177	-0.177	-0.097	-0.111	-0.106	-0.053	-0.068	-0.064
	(0.152)	(0.147)	(0.143)	(0.087)	(0.085)	(0.083)	(0.054)	(0.058)	(0.060)
(year 1)	-0.158	-0.197	-0.212	-0.024	-0.070	-0.082	-0.046	-0.094	-0.106
	(0.141)	(0.141)	(0.145)	(0.101)	(0.105)	(0.110)	(0.051)	(0.057)	(0.067)
(year 2)	-0.426**	-0.408**	-0.467**	-0.250*	-0.243*	-0.288**	-0.172**	-0.163**	-0.193**
	(0.210)	(0.196)	(0.197)	(0.138)	(0.129)	(0.135)	(0.079)	(0.075)	(0.088)
(year 3)	-0.271	-0.265	-0.231	-0.162	-0.160	-0.110	-0.114*	-0.111	-0.065
	(0.178)	(0.173)	(0.171)	(0.114)	(0.113)	(0.114)	(0.066)	(0.067)	(0.068)
(year 4)	-0.164	-0.170	-0.217	-0.072	-0.084	-0.116	-0.047	-0.063	-0.088
	(0.167)	(0.161)	(0.161)	(0.098)	(0.095)	(0.096)	(0.061)	(0.061)	(0.058)
Events (Bars)	39	39	39	39	39	39	39	39	39
Obs (post codes-years)	6,215	6,917	7,709	6,215	6,917	7,709	6,215	6,917	7,709
Number of postal codes	698	776	864	698	776	864	698	776	864
R-squared	0.304	0.299	0.291	0.303	0.298	0.290	0.303	0.297	0.289

Notes: This table reports results for our main test (Equation 1) with FSA Fixed effects. Each column is a separate regression with inner rings and outer rings of different radii. We only report results from the three-way interaction term interacting Near X Time X Dollar Amount of Gambling. This coefficient measures the effect of the removal of the dollar amount gambling from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies the removal of slot machines reduces the number of bankruptcies. This specification includes FSA fixed effects and calendar year of bankruptcy fixed effects. Standard errors are clustered by slot location and reported in parentheses.

Table 5: The Effect of Slots Removal on Bankruptcy with Dollar Magnitude of Gambling as Intensity of Treatment, and DA Area Fixed Effects

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outer ring	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km
Inner ring	0.25 km	0.25 km	0.25 km	0.5 km	0.5 km	0.5 km	0.75 km	0.75 km	0.75 km
(year -4)	-0.023	-0.010	-0.039	-0.054	-0.033	-0.039	-0.081	-0.047	-0.042
	(0.184)	(0.176)	(0.171)	(0.119)	(0.113)	(0.108)	(0.072)	(0.070)	(0.066)
(year -3)	-0.085	-0.043	-0.056	-0.043	-0.014	-0.023	-0.066	-0.038	-0.041
	(0.122)	(0.110)	(0.106)	(0.089)	(0.082)	(0.080)	(0.056)	(0.051)	(0.052)
(year -2)	-0.078	-0.067	-0.082	-0.065	-0.055	-0.062	-0.0804*	-0.068	-0.071
	(0.101)	(0.098)	(0.094)	(0.070)	(0.070)	(0.067)	(0.044)	(0.047)	(0.045)
(year 0)	-0.160	-0.167	-0.166	-0.095	-0.106	-0.101	-0.052	-0.065	-0.061
	(0.148)	(0.143)	(0.140)	(0.087)	(0.085)	(0.084)	(0.055)	(0.061)	(0.062)
(year 1)	-0.163	-0.199	-0.212	-0.028	-0.071	-0.083	-0.046	-0.092	-0.102
	(0.143)	(0.144)	(0.147)	(0.102)	(0.105)	(0.110)	(0.051)	(0.057)	(0.065)
(year 2)	-0.393*	-0.370**	-0.428**	-0.232*	-0.222*	-0.266**	-0.164**	-0.153**	-0.182**
	(0.195)	(0.180)	(0.179)	(0.132)	(0.122)	(0.128)	(0.077)	(0.072)	(0.084)
(year 3)	-0.244	-0.232	-0.196	-0.147	-0.142	-0.091	-0.108*	-0.103	-0.056
	(0.163)	(0.159)	(0.156)	(0.107)	(0.106)	(0.108)	(0.063)	(0.065)	(0.066)
(year 4)	-0.137	-0.140	-0.185	-0.057	-0.068	-0.098	-0.041	-0.055	-0.080
	(0.153)	(0.148)	(0.146)	(0.091)	(0.090)	(0.089)	(0.058)	(0.059)	(0.055)
Events (Bars)	40	40	40	40	40	40	40	40	40
Obs (post codes-years)	6,242	6,949	7,741	6,242	6,949	7,741	6,242	6,949	7,741
Number of postal codes	701	780	868	701	780	868	701	780	868
R-squared	0.436	0.432	0.423	0.436	0.432	0.423	0.437	0.433	0.424

Notes: This table reports results for our main test (Equation 1). Each column is a separate regression with inner rings and outer rings of different radii. We only report results from the three-way interaction term interacting Near X Time X Dollar Amount of Gambling. This coefficient measures the effect of the removal of the dollar amount gambling from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies the removal of slot machines reduces the number of bankruptcies. This specification includes DA area fixed effects, calendar year fixed effects. Standard errors in parentheses are clustered by slot location.

Table 6: The Effect of Slots Removal on Bankruptcy with Dollar Magnitude of Gambling as Intensity of Treatment, and Postal Code Area Fixed Effects

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outer ring	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km	1.5 km	2 km	2.5 km
Inner ring	0.25 km	0.25 km	0.25 km	0.5 km	0.5 km	0.5 km	0.75 km	0.75 km	0.75 km
(year -4)	-0.023	-0.010	-0.039	-0.054	-0.033	-0.039	-0.081	-0.047	-0.042
	(0.182)	(0.174)	(0.170)	(0.118)	(0.112)	(0.108)	(0.072)	(0.070)	(0.066)
(year -3)	-0.085	-0.043	-0.056	-0.043	-0.014	-0.023	-0.066	-0.038	-0.041
	(0.121)	(0.109)	(0.105)	(0.088)	(0.082)	(0.080)	(0.055)	(0.050)	(0.051)
(year -2)	-0.078	-0.067	-0.082	-0.065	-0.055	-0.062	-0.0804*	-0.068	-0.071
	(0.100)	(0.097)	(0.093)	(0.070)	(0.069)	(0.067)	(0.043)	(0.046)	(0.044)
(year 0)	-0.160	-0.167	-0.166	-0.095	-0.106	-0.101	-0.052	-0.065	-0.061
	(0.147)	(0.142)	(0.139)	(0.086)	(0.085)	(0.083)	(0.055)	(0.061)	(0.062)
(year 1)	-0.163	-0.199	-0.212	-0.028	-0.071	-0.083	-0.046	-0.092	-0.102
	(0.142)	(0.143)	(0.146)	(0.101)	(0.105)	(0.109)	(0.051)	(0.056)	(0.064)
(year 2)	-0.393**	-0.370**	-0.428**	-0.232*	-0.222*	-0.266**	-0.164**	-0.153**	-0.182**
	(0.193)	(0.178)	(0.178)	(0.131)	(0.121)	(0.127)	(0.076)	(0.072)	(0.084)
(year 3)	-0.244	-0.232	-0.196	-0.147	-0.142	-0.091	-0.108*	-0.103	-0.056
	(0.162)	(0.158)	(0.155)	(0.106)	(0.105)	(0.107)	(0.063)	(0.065)	(0.066)
(year 4)	-0.137	-0.140	-0.185	-0.057	-0.068	-0.098	-0.041	-0.055	-0.080
	(0.152)	(0.147)	(0.145)	(0.090)	(0.089)	(0.089)	(0.057)	(0.058)	(0.055)
Events (Bars)	40	40	40	40	40	40	40	40	40
Obs (post codes-years)	6,242	6,949	7,741	6,242	6,949	7,741	6,242	6,949	7,741
Number of postal codes	701	780	868	701	780	868	701	780	868
R-squared	0.048	0.045	0.047	0.045	0.042	0.044	0.044	0.04	0.043

Notes: This table reports results for our main test (Equation 1) with Postal Code Fixed effects. Each column is a separate regression with inner rings and outer rings of different radii. We only report results from the three-way interaction term interacting Near X Time X Dollar Amount of Gambling. This coefficient measures the effect of the removal of the dollar amount gambling from the inner ring, (relative to the outer ring), for each year (relative to the omitted year t=-1). A negative coefficient implies the removal of slot machines reduces the number of bankruptcies. This specification includes postal codes fixed effects and calendar year of bankruptcy fixed effects. Standard errors are clustered by slot location and reported in parentheses.