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SPATIAL COMMITMENT DEVICES AND ADDICTIVE GOODS: EVIDENCE FROM THE REMOVAL OF SLOT MACHINES FROM BARS

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Spatial Commitment Devices and Addictive Goods: Evidence from the Removal of Slot Machines from Bars

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Abstract

Commitment device theory suggests that temptations to consume addictive goods could be reduced by the regulatory removal of geographically close environmental cues. We provide new evidence on this hypothesis using a quasi-natural experiment, in which gambling regulators removed slot machines from some, but not all, neighborhood bars. We find that the removal of slot machines reduced personal bankruptcies of close neighbors (within 100 meters) but not neighbors slightly farther away. This is consistent with the removal of neighborhood slots serving as an effective spatial commitment device, which reduced close neighbors' temptation to gamble, thus allowing them to avoid bankruptcy.

Keywords: spatial commitment devices, addiction, regulation

JEL Codes: D12, K35, L83

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1. Introduction

An extensive literature in behavioral economics has examined the use of commitment devices, in which agents attempt to avoid behavior that is pleasurable in the short term but that can be harmful in the long term (see surveys by Bryan, Karlan, and Nelson, 2010; and DellaVigna, 2009). One important element of the commitment device literature has examined the impact of geographically close cues on the consumption of addictive goods (Bernheim and Rangel, 2004, and Beshears, Choi, Laibson, and Madrian, 2005). Examples of such cues include the sight of cigarettes causing smokers to crave a cigarette or the sound of ice in a glass causing an alcoholic to crave a drink (see e.g., Laibson, 2001). An important policy implication of this argument is that the regulatory removal of the geographically close cue can serve as a spatial commitment device because it can reduce the temptation to use the addictive product.

Despite the important policy implications of this issue (i.e., the very large social and personal costs of addiction), empirical evidence remains scarce. This is largely because of the difficulties of identifying and measuring commitment opportunities (e.g., regulators removing specific cues for addictive goods from individuals). Bernheim, Meer, and Novarro (2016, p. 42), for example, have recently highlighted this scarcity of evidence by commenting that “evidence that users of addictive substances actually value commitment opportunities is almost entirely limited to anecdotes.”

The aim of this paper is to provide such field evidence. Specifically, we examine the removal of a spatially close cue as a commitment device by exploiting a unique quasi-natural experiment, implemented by the gambling regulator in the Canadian province of Alberta. The Alberta gambling regulator removed slot machines from some, but not all, neighborhood bars over an 11-year period, so that approximately one-quarter of all neighborhood bars with slot machines in Alberta had their slot machines removed. The motivation for this policy by the Alberta gambling regulator was based on the argument that removing slot machines from specific neighborhoods would reduce consumption of these devices in those neighborhoods.

Our quasi-natural experiment is very similar to the theoretical discussion by Beshears, Choi, Laibson, and Madrian (2005), who describe how regulators could provide spatial commitment devices to help addicts avoid consumption by limiting the geographic availability of an addictive product. In the context of smokers and cigarettes, Beshears et al. (2005) argue that, in a “free market environment, rationing [the smoker’s] cigarette consumption is difficult.

Whenever he runs out of cigarettes, he is likely to be near a convenience store that will gladly sell him another pack. In contrast, if cigarette vendors were few and far between, then the smoker would be able to partially precommit to a particular quantity of cigarettes. For example, precommitment would be possible. … [if] impulses to buy additional cigarettes might be easy to resist given the transactions costs of … returning to the [more distant store]" (pp. 45–46).

This theoretical discussion by Beshears et al. (2005) describes the key mechanism underlying our Alberta quasi-natural experiment. In the terminology of Beshears et al. (2005), we examine whether regulatory increased distances between consumers and sellers of addictive products allow these consumers to precommit to reduced consumption because their increased distance-related transaction costs reduce their impulse to buy the addictive product. Both our context and that described by Beshears et al. (2005) can be described as a *soft* commitment, in that there is no direct financial or physical punishments for the individual to break the commitment, for example, by traveling to the remaining (but more distant) slot locations or cigarette stores.

While our focus is on the spatial regulation of slot machines, the regulatory issues we address are relevant to any addictive product in which supply is regulated (e.g., slot machines, cigarettes, alcohol, and, in an increasing number of jurisdictions, marijuana). Beshears et al. (2005) argue that, while regulators can implement various kinds of soft commitment devices (e.g., restrictions of supply over time; or restrictions of supply over space), all of these potential restrictions of supply require rigorous empirical evidence to determine their effectiveness.

Our quasi-natural experiment, therefore, allows us to provide new evidence on the regulatory policy of spatially restricting the supply of addictive products. Specifically, if the regulatory removal of this spatially close cue (i.e., the slot machines[1]) from a specific neighborhood is effective as a spatial commitment device (i.e., if individuals in the immediate neighborhood gamble less because of the removal of the slot machines), then we should observe a reduction in the negative outcomes associated with slot machine usage, for example, a reduction in bankruptcy filings in that neighborhood. Our empirical tests thus examine the

[1] In this paper, we use the term "slot machines" or "slots," commonly used in the U.S., even though these machines are known by various names around the world. They are also known as "video poker" in the U.S., "video lottery terminals" (VLTs) in Canada, "fruit machines" in Britain, and "pokies" in Australia. They are also known by the more generic term electronic gaming machines (EGMs) (Smith and Campbell, 2007).

impact that the regulatory removal of slot machines from a specific bar had on subsequent bankruptcy filings within that very small neighborhood.

Slot machines are an appropriate commodity to provide field evidence on the cue-triggered consumption (Laibson, 2001) of addictive products. First, slot machines are specifically designed (Dow Schull, 2012) to generate environmental cues (particularly loud noises and flashing lights), with the aim of increasing consumption (i.e., gambling). Second, the addictive nature of slots has been documented by an extensive literature (see e.g., Dow Schull (2012)). In the popular press, slot machines are often called "the crack cocaine of gambling" because they are considered to be so addictive.

Our main reason for using bankruptcy as an outcome variable is the extensive literature linking gambling to bankruptcy.[2] However, another important advantage for our use of bankruptcy as an outcome variable is that our data allow us to observe the exact geographic location of the universe of slot machines as well as the exact geographic location of the universe of bankruptcy filers in Alberta. These exact geographic data are central to our econometric methodology and identification strategy. We implement a difference-in-differences methodology, examining the removal of slot machines from the very small neighborhoods surrounding the various bars at the dates that the regulator removed the slots from each bar.[3]

Our spatial identification strategy is similar to previous studies of neighborhood-level shocks (e.g., Currie, Davis, Greenstone, and Walker (2015), and many others), in that we define treatment neighborhoods as areas within very short distances (within 100 meters) from the closed slot machine location and control neighborhoods as areas slightly farther away. These geographically very small treatment and control groups allow us to control for possible selection issues (e.g., where regulators select to remove slots from specific neighborhoods based on unobservable political bias). The identification assumption is that, while unobservable political bias could affect political choices between larger areas (e.g., politicians or regulators selecting

[2] Barron, Staten, and Wilshusen (2002); Daraban and Thies (2011); Garrett and Nichols (2008); Grote and Matheson (2014); Komoto (2014); and Thalheimer (2004).

[3] Because data on slot machine used by identifiable individuals do not exist, we do not examine the gambling activity of individuals as an outcome variable. We use the exact geographic location of the universe of individual bankruptcies as our outcome variable, specifically because of the individual-level nature of our spatial research design and identification strategy.

between various towns or cities), it is unlikely that there will be political bias between the few hundred meters that constitute our treatment and control groups.

We argue that the mechanism by which *distance* can act as a commitment device to avoid temptation is based on what Bernheim et al. (2016) describe as the *availability strategy*, which they describe as a strategy to "limit the availability of a problematic good by not maintaining an easily accessible supply" (p. 42). The examples of the availability strategy provided by Bernheim et al. (2016) all relate to not keeping temptation goods (e.g., cigarettes, alcohol) "at home." In our paper, we slightly extend the spatial context of the Bernheim et al. (2016) examples by examining temptation goods (in our case, slot machines) that are located within close distance from home, rather than "at home." Our testable version of the availability strategy hypothesis, therefore, is that temptation goods that are spatially very close to home will provide higher levels of temptation because they will be more readily available, compared with temptation goods that are slightly farther away from home, which we argue will provide lower levels of temptation because they are more likely to be "out of sight and out of mind."

Our empirical strategy allows us to differentiate between our availability strategy hypothesis, which suggests that removing a close environmental cue will act as a commitment device to reduce consumption, and an alternative hypothesis, which suggests that only distance-related transactions costs rather than commitment device issues will affect consumption. If a treatment group very close to the slots (e.g., within 100 meters) faced significantly higher temptation from slot machines compared with a control group (e.g., residents 500 meters to 600 meters away from slots), then the availability strategy hypothesis would predict that removing those slots should have a significantly larger effect on treatment compared with the control group.

On the other hand, if consumption in the neighborhood was *not* driven by commitment device issues (i.e., rejection of the availability strategy hypothesis), then, after the slots had been removed, both treatment and control groups could travel to the next closest operating slot machine. The differences in these travel costs between treatment and control neighbors would be the trivial costs of traveling the few hundred extra meters that differentiates treatment and control groups. Furthermore, our dependent variable is bankruptcy filing, which is an extreme form of financial distress. It is unlikely that the trivial additional cost of traveling a few hundred extra meters will be large enough to cause significant differences in bankruptcy filings between the treatment and control groups in the absence of a commitment device opportunity from slots removal. Thus, a finding of no significant differences between the treatment and control groups would be consistent with treated neighbors *not* using the removal of the slot machine as a spatial commitment device.

Another important advantage of our ability to define radii of various small distances from the slot location is that we can examine the effectiveness of this commitment device at various distances. Specifically, in our setup, we can redefine our treatment group as neighbors at various distances from the slot machine location (for example, within 0 meters to 100 meters or alternatively within 100 meters to 200 meters etc.) while holding the control group constant (e.g., 500 meters to 600 meters). This approach allows us to provide additional evidence on the availability strategy, which predicts that the commitment device should be more effective at very close distances (e.g., between 0 meters and 100 meters) compared with slightly farther distances (e.g., between 100 meters and 200 meters).

Our main finding is that there is a significantly greater decline in personal bankruptcies after slot machines are removed in the treatment groups compared with control groups, which is consistent with the availability strategy hypothesis. In terms of economic magnitudes, we find that the removal of an additional 1 percent in total dollars gambled on slots causes a reduction of approximately 0.0046 bankruptcies per postal code year in the inner ring compared with the outer ring. This is a 2 percent reduction in bankruptcies compared with the average of 0.22 bankruptcies per postal code year in our full sample. These effects are greatest when neighbors are very close to the bar where the slots were removed (within 100 meters) but decrease in size and statistical significance when the neighbors are slightly farther away (within 100-meter increments between 200 meters and 500 meters).

Our results are thus consistent with the availability strategy hypothesis, which states that removing environmental cues within very close distances will have greater effectiveness as a commitment device compared with removing cues slightly farther away.

2. Contributions to the Literature

In addition to our main contribution of providing evidence on the regulatory removal of environmental cues being effective as a commitment device, our paper builds on, and makes contributions to, various other literatures.

First, our paper contributes to the empirical literature on the use of commitment devices in that we examine commitment devices that operate over *space*. Most of the recent empirical literature testing regulatory commitment devices has been on *time*-based regulation (for example, days on which alcohol can be sold, as in Bernheim, Meer, and Novarro (2016); Hinnosaar (2016); and Ben-David and Bos (2017)) rather than space-based regulation. This is despite authors such as Beshears, Choi, Laibson, and Madrian (2005) arguing that regulatory commitment devices can be either time based or space based.

The central empirical advantage of examining space-based (rather than time based) commitment devices is that we can exploit fine-grained geographic data to specifically test the availability strategy mechanism of Bernheim et al. (2016), for example, by defining treatment groups as neighbors within various close distances of a closed slots location (i.e., within 100 meters, within 100 meters to 200 meters).

Our second contribution is to the literature on neighborhood disamenities. Examples of this neighborhood disamenity literature 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 neighborhood infant health); Pope and Pope (2014; Walmart openings on neighborhood house prices); Currie and Tekin (2015; foreclosure on neighborhood hospital visits); and Currie, Davis, Greenstone, and Walker (2015; toxic plants on neighborhood infant health).

In the existing neighborhood disamenity literature, a variety of context-specific mechanisms have been suggested to explain why a shock to the disamenity has a larger effect on close neighbors (which we call inner rings) compared with somewhat more distant neighbors of the disamenity (outer rings). For example, research examining the neighborhood effects from toxic plants and from pollution related to traffic congestion (e.g.; Currie, Greenstone, and Moretti, 2011; Currie and Walker, 2011; and Currie, Davis, Greenstone, and Walker, 2015) is based on which pollutants are transmitted through air. Because of this, these authors can test the hypothesis that pollutants will have a bigger effect on inner ring neighbors relative to those in the outer ring. The contribution of this paper to the literature on neighborhood disamenities, is that we propose the “availability strategy” for commitment devices, as described by Bernheim et al.

(2016), as a novel mechanism explaining why a shock to a neighborhood temptation good will have a larger effect on inner ring relative to outer ring neighbors. This "availability strategy" mechanism is based on the greater levels of temptation to consume the addictive good felt by inner ring, compared with outer ring, neighbors.

Our paper also contributes to the literature on household bankruptcy.[4] An extensive literature has examined a wide variety of exogenous shocks as possible causes of household bankruptcy. This paper is the first to provide evidence that the removal of slot machines can reduce personal bankruptcy, using spatial econometric techniques to identify causality.

3. Institutional Context and Data

The Alberta Gaming and Liquor Commission (AGLC), an arms-length agency of the Government of Alberta, regulates all slot machines in Alberta.[5, 6] The AGLC is both the *regulator* of gambling across the province (with a mandate to implement government legislation and policies) and the gambling *operator* on behalf of the province (with a mandate to maximize government revenue from gambling). 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 has provided us with monthly data for the universe of retail slot locations in the province. These data include six-digit postal codes for each slot machine location and the exact dates that the slots were introduced or withdrawn from a location. We can also observe

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[4] There is extensive literature examining the impact of exogenous shocks on bankruptcy filings: Fay, Hurst, and White (2002); Gross and Souleles (2002); Gross and Notowidigdo (2011); Hankins, Hoekstra, and Skiba (2011); Gross, Notowidigdo, and Wang (2014); Dobbie and Song (2015); Li, White, and Zhu (2011); and White (2007, 2011). Using the same Canadian bankruptcy data used in this paper, Mikhed and Scholnick (2016) examine an exogenous government cash payment as an exogenous shock, and Agarwal, Mikhed, and Scholnick (2016) examine lottery winnings as an exogenous shock.

[5] Studies that examine the addictive properties of slot machines include Blaszczynski (2013); Afifi et al. (2010); Buchanan, Elliott, and Johnson (2009); Rintoul et al. (2013); Hodgins et al. (2012); Lund (2009); Mishra et al. (2010); Wheeler, Round, and Wilson (2011); and Young and Tyler (2008). A variety of studies have examined gambling issues in the geography literature, including Doran, McMillen, and Marshall (2007); Markham, Doran, and Young (2014); McMillen and Doran (2006); Storer, Abbott, and Stubbs (2009); Wheeler, Rigby, and Huriwai (2006); Young, Lamb, and Doran (2009); Young, Markham, and Doran (2012 A and B).

[6] 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); and Buchanan, Elliott, 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); Williams and Wood (2004); and Williams, West, and Simpson (2012).

monthly data on the total dollars gambled and the number of slot machines at each location. 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 in Figures 1 and 2.

3.1. The Natural Experiment: The Mandated 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 original 2001 document noted that the aim of this policy was to "control and manage the accessibility of [slot] gaming" (AGLC, 2001, p. 37). The AGLC implemented this policy by removing slot machines from neighborhood bars and placing them in new custom-built mini-casinos. Figures 1 and 2 provide monthly data on the number of bars and mini-casinos providing slots and the number of slot machines in Alberta, respectively. These figures show that, during the course of our study (2003–2013), the AGLC concentrated its slot machines in fewer total locations by significantly reducing the number of bars that provide slot machines while moving those removed slots to larger mini-casinos. Figures 3 and 4 provide maps of the two largest cities in Alberta (Edmonton and Calgary) and show the exact locations of these bars and mini-casinos.

The AGLC 2001 policy document stated that the aim of this policy was to "manage the accessibility" (AGLC, 2001, p. 37) of slot machines by removing them from specific neighborhoods. This policy justification is similar to the way Bernheim et al. (2016) describe their availability strategy as occurring where reduced accessibility or availability will reduce the temptation to consume addictive goods. We argue that this AGLC policy (removing slot machines from specific neighborhoods to limit accessibility within those neighborhoods) constitutes a good context in which to test the space-based availability strategy form of regulatory commitment devices.

Our context of removing slot machines from neighborhoods can be described as a s*oft commitment* in the terminology of Bryan et al. (2010) because no direct economic or physical penalty is imposed on the individual for breaking the commitment by consuming the addictive product. Bryan et al. (2010) describe the implications of soft commitments in the context of removing an environmental cue as making it more likely for the agent to "succumb easily to some temptation, albeit in a different environment" (p. 679; i.e., an environment in which the cue has not been removed). The soft commitment nature of our context can be seen from Figures 3 and 4, showing maps of the geographic locations of bars with slots removed as well as bars with slots still available. The point of these maps is to show that, even after slots were removed from some bars, many other easily accessible locations continued to provide slots throughout our sample period.

This discussion forms the basis of our empirical strategy. For example, if we find that the regulatory removal of slots from a neighborhood did not cause a reduction in bankruptcies in that neighborhood, then this finding implies that individuals could simply have accessed the other continuously operating slot machines in slightly more distant neighborhoods. In other words, this soft commitment device was not effective at reducing slot machine use. On the other hand, if we find that the removal of slots from a neighborhood did indeed significantly reduce bankruptcies in that immediate neighborhood, then this is consistent with some slot users relying on the regulatory removal of the slots as a commitment device to reduce their slot machine use, in spite of this commitment device being soft.

Further institutional details concerning the exact timing of the removal of slots from each bar and the inability of bar owners to influence the mechanism by which the AGLC selected which slot machines to remove are described in the Appendix.

3.2. 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 U.S.), our data include the complete universe of every bankruptcy filing in Canada. This OSB database includes the 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.

3.3. 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 small geographic areas; there are often multiple postal codes within a single city block or within one apartment building. Because we can observe the exact longitude and latitude of the centroid of each postal code, 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, which is called a dissemination area (DA). The DA contains an average of 200 households, with an average area of 0.2 square kilometers. Neighborhood-level census data are available at the DA level. Because we can observe the exact postal code of every bar and every bankruptcy filer, we can include extensive neighborhood-level data in our empirical specifications to control for observable confounding factors. The census data available to us include all the standard census demographics including income, unemployment, education, homeownership, marriage rate, and share of immigrants as measured at the neighborhood DA level.

Besides our bankruptcy data, no other observable neighborhood data exist at the postal code levels, largely because of their extremely small size. However, we can observe the total number of postal codes in each DA and 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 Canada's census to be homogeneous in terms of population density and other neighborhood characteristics. We include the average population size per postal code as a control variable in all regressions.

4. Identification

4.1. Inner Ring–Outer Ring Identification Strategy

Our identification strategy is taken from the extensive neighborhood disamenity literature (described in Section 2 above), which uses very close neighbors (inner rings) of the disamenity as the treatment group and neighbors slightly farther away (outer rings) as the control group. The primary use of this setup in our context is to provide evidence on the availability strategy of Bernheim et al. (2016) by comparing inner rings (e.g., within 100 meters of the bar) as treatment groups with outer rings (e.g., between 500 meters and 600 meters of the bar) as control groups. Furthermore, following the neighborhood disamenity literature described in Section 2, we also argue that our inner ring–outer ring setup provides a number of additional econometric advantages related to issues of identification and selection on unobservable characteristics.

In terms of identification, the main advantage of the inner ring–outer ring setup as an identification strategy is to control for neighborhood unobservable attributes. 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 that are shared by residents of *both* the inner and outer rings. If these distances are very small (e.g., a few hundred meters), then the argument in this literature is that unobservable neighborhood characteristics will be the same between the treatment group (inner rings) and the control group (outer rings). Thus, by comparing the treatment group with the control group, this identification strategy "differences out" neighborhood unobservable attributes in the inner and outer rings.

In our context, there are at least three possible types of neighborhood unobservable traits that will be controlled by using this inner ring–outer ring identification strategy. First, in contexts such as ours, in which the shock to the neighborhood (removing slot machines) is determined by regulatory edict, a key identification challenge is the possible selection of neighborhoods to treat by regulators, based on unobservable political motivations. As noted by Hinnosaar (2016) in her study of regulators restricting alcohol sales, "in the case of a policy change, it is difficult to identify the impact, since … the policy change itself is usually a political decision" (p. 110). In our context, the concern is that the Alberta gambling regulators could possibly select which neighborhoods would face the removal of their slot machines, based on unobservable political considerations.

Using similar arguments to those in the neighborhood disamenity literature, we argue that, while gambling regulators in Alberta could be politically biased between larger neighborhoods (e.g., suburbs or towns), it is unlikely that politically motivated regulators would make political distinctions *within* the very small areas (i.e., 600 meters or 1,000 meters in various specifications) containing *both* the inner and outer rings. Thus, by defining inner rings as our treatment group and outer rings as our control group, we can "difference out" any unobservable politically biased selection of that neighborhood by Alberta gambling regulators.

A second possible neighborhood unobservable attribute 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 near slots locations or alternatively if slot operators select to locate near gamblers. In both of these cases, our methodology follows the existing neighborhood disamenity literature cited in Section 2 above, by arguing that, at the very small distances involved (fractions of a kilometer), the supply of locations available at the time the 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, italics added). 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).

A third possible neighborhood unobservable trait concerns neighborhood specific shocks, such as the closure of a local plant. The assumption in the inner ring–outer ring neighborhood 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).

5. Research Design Issues

5.1. Sample Selection

Our aim in the sample selection design is to provide the cleanest test of the effect of removing slot machines on neighborhood bankruptcies. To create this clean natural experiment, we define our main sample of slot machine removal events to include only bars in which there is a single slot removal event during the sample, with no other slot openings or slot removals 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 four years before to four years after the event date) to reflect findings in the bankruptcy literature, noting 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 removal and subsequent reopening events in our econometric framework with very long event windows, we remove all such removal and subsequent reopening events from our main sample.[7]

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. For this reason, we do not include such instances as events in our tests.

Because our identification strategy exploits small neighborhoods within fractions of a kilometer of the bar that had slots removed, our tests focus only 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 racetracks because their customer catchment areas are much larger than the small neighborhoods that are central to our identification strategy.

Another important restriction in our main sample is that there can be no proximate slot machine locations operating within a close distance (2 kilometers) of the bar that had its slot machines removed. The reason for this restriction is that, if there was an alternative slot location operating close to the bar with removed slots, this could result in geographic substitution between neighboring locations and thus affect our main results of interest. The possibility of geographic substitution between locations is particularly relevant, given that all slot machines across 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 (e.g., see the maps in Figures 3 and 4).

However, while our main sample only includes slot locations with a single slot removal event and no other proximate slot locations, we also replicate our tests on a slightly less restrictive sample. Specifically, we examine slot locations with only a single slot removal event

[7] Some opening and removal 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. During the period of this background check, 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.

but those with other slot locations within close proximity (within 2 kilometers), which operate continuously within our sample period. This sample allows us to test the specific hypothesis that, if there are indeed alternative slot locations within close proximity of the location that faced the removal of its slots, then the commitment device mechanisms described previously would be less effective.

5.2. Geographic Sizes of Inner and Outer Rings

An important advantage of our identification strategy is that, because of the fine-grained geographic data for both of our databases (slot machine locations and bankruptcy filer locations), we have considerable freedom in defining our treatment (inner rings) and control (outer rings) groups.

Given the lack of concrete theory as to what constitutes appropriate distances for these concepts, our empirical tests examine a variety of specifications, with different-sized inner and outer rings. For example, all of our specifications examine inner rings with nine different sizes of these rings (in meters: 0–100; 100–200; 200–300; 300–400; 400–500; 0–200; 0–300; 0–400, and 0–500). Similarly, all our outer-ring specifications examine two separate distances for these rings (in meters: 500–600 and 500–1,000). One reason for choosing these specific distances is that in no cases do treatment group postal codes (always less than 500 meters) become control group postal codes; they are always more than 500 meters.

The main prediction of the availability strategy form of commitment device is that those environmental cues that are very close will cause larger amounts of temptation compared with environmental cues farther away. We can provide evidence of this by showing that the effect of the environmental cue will be greater, for example, when the inner ring is defined as being within 100 meters compared with when the inner ring is defined as being within 200, 300, 400, or 500 meters, and so forth.

5.3. Continuous Intensity of Treatment

An important element of our research design is our use of measures of the continuous intensity of treatment. Our data allow us to observe the dollar amount of gambling at each bar for each month that the slot machines are operating as well as the number of slot machines operating in each bar in each month. We can observe the amount of gambling in the months just before the removal of the machines by the AGLC. In our models, therefore, treatment is defined as the removal of slots from a bar, while the *intensity* of treatment is defined as, depending on specification, either: 1) the dollar magnitude of gambling removed from each bar, or 2) the number of slot machines removed from each bar. While these two measures will be correlated, we examine both in turn because they reflect somewhat different elements of the treatment. As we show in our results section that follows, our main empirical conclusions from using either of these two measures are very similar.

The main advantage in using the dollar amount of gambling as our intensity of treatment term is that these dollar amounts provide us with a very fine-grained measure of the actual gambling removed from that bar. This fine-grained measure of intensity of treatment allows us to test how variation in the magnitude of this treatment affects our outcome of interest. The advantage of using the number of slot machines removed as our intensity of treatment term is that the changes to the number of slot machines in a location can be interpreted as a supply shock. Recall that the AGLC regulator has complete authority to determine the number of slot machines at any location at any date. Thus, in the context of the events we are interested in, the AGLC decided at some past date on the number of slot machines to allocate to a specific bar and then subsequently decided on our event date to remove all those machines from that bar.

5.4. Intention to Treat Estimates

As with many policy evaluation-type studies using administrative data, our study estimates the intention-to-treat (ITT) effects, rather than the treatment-on-the-treated (TOT) effects. Our study makes the treatment (i.e., the slot machine removal acting as a commitment device) available to all members of the treatment group (i.e., all residents of the inner ring), but we are not able to observe which fraction of the treated group make use of the treatment (i.e., what fraction of residents of the inner ring specifically use the slot removal commitment device to reduce gambling). We are only able to observe the aggregate outcome of the treated group (i.e., the reduction in the total number of bankruptcies in the treated inner ring). As with all ITT-type studies, because we cannot observe the fraction of treated individuals who specifically use the treatment, our ITT coefficients *underestimate* the effect of the treatment on individuals who do indeed use the treatment.

6. Results

6.1. Event Study Results

In this section, we provide evidence on the effect of slots removal on bankruptcies using an event study framework. This model allows us to show when exactly these effects occurred and to test whether the parallel trends assumption of the difference-in-differences method is satisfied in our data. To accomplish this, we estimate the following distributed lag model that is similar to that of Gallagher (2014); Agarwal, Liu, and Souleles (2007); and Agarwal and Qian (2014):

$$Y_{ijt} = \beta_0 + \sum_{s=-4}^{4} \beta_{1s} Near_{ij} \times T_s \times \ln(Gambling)_j + \beta_2 \ln(Gambling)_j \times Near_{ij} + \sum_{s=-4}^{4} \beta_{3s} Near_{ij} \times T_s + \sum_{s=-4}^{4} \beta_{4s} ln(Gambling)_j \times T_s + \beta_5 \ln(Gambling)_j + \beta_6 Near_{ij} + \alpha_t + \beta_7 (Controls)_{da} + \delta_j + \varepsilon_{ijt,} \qquad (1)$$

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 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. The time invariant intensity of treatment term *Gambling* is either 1) the log of the dollar amount of gambling in the bar with removed slots in the 12-month period before the closure or 2) the number of machines operating in this bar before the closure. *T* represents a set of indicator variables equal to 1 for a particular year relative to the closure year equal to 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*. In other words, the coefficients on this triple interaction terms, β_{1s}, measure the effect of a removal of the specific dollar amount of gambling or the number of machines 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 subcomponents 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 or the number of machines 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 or the number of machines removed at that location. Even though we 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 one-way *Gambling* term from our specifications.

Our event windows are long (four years before and four 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) with that year.

To control for the possible correlation of the residuals within a specific postal code, we cluster standard errors by postal code. Our main specification also includes bar location fixed effects, δ_j, 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 affect 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 number 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 slot removal event. A negative sign on the β_{1s} coefficients 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 1) comparing bankruptcies in inner rings with those in outer rings, 2) comparing bankruptcies in various years with bankruptcies in year t = -1, and 3) accounting for the magnitude of gambling withdrawn because of the closure.

Our distributed lag results are presented in Tables 2 and 3 and Figures 5 and 6. These results show that, for all specifications for all inner-ring sizes, the year-by-year coefficients are all insignificant for all years before the event date, which is consistent with the parallel trends assumption.

These tables and figures show, however, a marked difference for years after the event date between the inner rings within 100 meters and all the other inner-ring sizes (within 200, 300, 400, and 500 meters). The 100-meter inner-ring specifications have a number of years after the event date when the coefficient is significant and negative, while the larger inner-ring specifications do not have any significant coefficients after the slots closure. Thus, these results show that personal bankruptcies declined in the years after the slots were removed in the smallest inner-ring neighborhoods (0–100 meters) compared with outer-ring neighborhoods (e.g., 500–600 meters) with each dollar of gambling removed (or each slot machine removed).

6.2. Difference-in-Differences Results

Our standard difference-in-differences (DID) model is very similar to the distributed lag model presented in the previous section with the exception that we designate all years before the slots closure, including year 0, as a "before" period and all years after the closure as an "after" period. Thus, this DID specification is as follows:

$$Y_{ijt} = \beta_0 + \beta_1 Near_{ij} \times After_t \times \ln(Gambling)_j + \beta_2 \ln(Gambling)_j \times Near_{ij} + \beta_3 Near_{ij} \times After_t + \beta_4 ln(Gambling)_j \times After_t + \beta_5 \ln(Gambling)_j + \beta_6 Near_{ij} + \alpha_t + \beta_7 (Controls)_{da} + \delta_j + \varepsilon_{ijt}. \quad (2)$$

In this specification, we include a single indicator variable, $After_t$, which equals 1 in the four years after slot removal event and 0 in the four years before the event and the year of the event (year 0). The main coefficient of interest is the coefficient, β_1, which is the average effect of removing an extra dollar of gambling (or an extra slot machine) from the inner neighborhood relative to the outer neighborhood for the average of the four years after the removal.

Our various results from this specification are presented in Tables 4 to 9. In all these tables, we report various alternative measures of inner-ring sizes as columns and various alternative measures of outer-ring sizes as rows, or as panels, within each table. In all of these tables, we present only the estimated coefficient of the main variable of interest, the three-way interaction term including time, space, and intensity of treatment. In all the tables, we report five separate columns in which the outer limit of the inner ring is 100, 200, 300, 400, or 500 meters, respectively.

Tables 4 and 5 provide standard difference-in-differences results for the two intensity of treatment terms (dollars of gambling removed and machines removed, respectively). The first column of Table 4 shows that slots removal reduces personal bankruptcy in the smallest inner ring we define (within 100 meters of the bar) for all outer rings we consider. This result is very robust, and the coefficients are statistically significant at 5 percent and 1 percent levels for outer-ring controls of either 500–600 meters (coefficient of -0.459) or 500–1,000 meters (coefficient of -0.411).

The coefficient estimate of -0.459 in the top left cell of Table 5 implies that an additional 1 percent of dollar value of gambling removed decreased bankruptcies by 0.0046 per postal code year. This coefficient is obtained comparing the five years prior (including the event year), with the four years after the event date and comparing the treated area between 0 meters and 100 meters with the control area between 500 meters and 600 meters. This estimated reduction of 0.0046 bankruptcies per postal code year after a slot location removal can be compared with the mean amount of 0.22 bankruptcies per postal code per year (as measured across our entire sample; see Table 1), which implies a 2 percent reduction in the number of bankruptcies after a slot machine is removed.

Table 5 reports results for the same difference-in-differences specification but with the number of slot machines removed used as the intensity of treatment instead of the dollar value of gambling removed. Overall, all results in this table are very similar to the results in Table 4, with the largest effects and most statistically significant results occurring in column 1, where neighbors are within 100 meters of the bar. This result is robust across the various outer-ring control groups we examine, including 500–600 meter (coefficient of -0.091) and 500–1,000 meter (coefficient of -0.078). This coefficient implies that each additional slot machine removed from the bar will reduce bankruptcies by 0.091 per postal code year, comparing the five years prior (including the event year), with the four years after the event date and comparing the inner ring between 0 meters and 100 meters with the outer ring between 500 meters and 600 meters.

Columns 2 through 5 of Tables 4 and 5 show that the effect of slots removal dissipates quickly with distance. The coefficients become progressively smaller in magnitude and less statistically significant as the distance of the treated inner ring increases from the bar.

While the results in Tables 4 and 5 are for a sample in which there were no other bars in close proximity (within 2 kilometers) of the closed slots location, in Tables 6 and 7, we report results in which this constraint is relaxed. In these tables, we report coefficients for a sample in which there was one other bar with continuously operating slot machines within 2 kilometers of the bar with its slots removed. This setup can serve as a placebo test. We argue that, in neighborhoods with an alternative, continuously operating, slots location, it would be harder to precommit to a reduced quantity of gambling. Thus, the commitment device opportunity presented by a closed slots bar would be weaker or ineffective. The results from Table 6 (with dollars as intensity of treatment) and Table 7 (with machines as intensity of treatment) show that, in all cases (including inner rings of 100 meters), the effect of slots removal (the triple interaction term) is statistically insignificant at conventional levels.

To examine whether the main results are driven by certain types of neighborhoods, we split the sample based on neighborhood income as measured by the DA-level census data described previously. We split the sample at the median level of income and replicate the results from Table 4 for high- and low-income neighborhoods. Tables 8 and 9 report results for these two subsamples with the dollar amount of gambling removed as the ITT effect. The results in Tables 8 and 9 show that slots removal reduces personal bankruptcies in low-income neighborhoods (Table 8) and that these effects are highly statistically significant, especially within 100 meters of a removed slot location. However, the results in Table 9 suggest that in high-income neighborhoods removing the slots decreases personal bankruptcy by a smaller amount than in low-income areas and that these coefficients are mostly statistically insignificant. These results imply that our proposed mechanism of removing slot machines acting as a commitment device for very close neighbors is significantly more effective in low- relative to high-income neighborhoods.[8]

[8] We report the results for the number of machines removed as the ITT in the Appendix in Tables A1 and A2. These coefficients imply the same conclusions as Tables 8 and 9, but they are significant only at 10 percent, possibly because of the finer-grained nature of the dollar magnitude measure.

7. Conclusion

This paper examines the effect of the regulatory removal of an environmental cue on the consumption of addictive goods. While this issue has important theoretical as well as policy implications, existing research on it is rare because of the difficulty of identifying and measuring the exogenous removal of cues for addictive products from specific individuals. We exploit a unique Canadian natural experiment in which slot machines were removed from some, but not all, neighborhood bars by gambling regulators. We use this experiment to examine the effect of the regulatory removal of the slot machines on neighboring bankruptcy filings.

Our methodology exploits fine-grained data on the exact geographic location of the universe of both slot machines and bankruptcy filings. Our identification strategy compares treatment groups very close (within 100 meters) of the bar with control groups slightly farther away. This identification strategy allows us to control for neighborhood-level unobservable traits, such as the unobservable political bias of regulators regarding which neighborhood to treat.

Our main result is that the regulatory removal of slot machines from a neighborhood bar causes a significant reduction in subsequent bankruptcies of those neighbors within 100 meters of the bar. However, we find increasingly less significant results when we examine neighbors slightly farther away from the bar (more than 100 meters). Our results are thus consistent with the argument that individuals very close to the bar (e.g., within 100 meters) used the regulatory removal of the slot machines as a commitment device to reduce gambling, thus reducing the number of subsequent bankruptcy filings by those individuals.

Our results have important policy implications in terms of actions by regulators to reduce the harmful effects of slot machine gambling. We show that removing slot machines from neighborhoods can indeed reduce the harmful outcomes from gambling (in our case, personal bankruptcy), albeit only within a very close distance (e.g., within 100 meters from the slot machines) where the availability strategy operates.

In addition to the specific slot machine context, our results are also important in the broader context of additive products in which supply is legal but is curtailed in some way by regulators to provide possible commitment devices to consumers (e.g., cigarettes, alcohol, and marijuana). Given the possible tradeoffs between the costs and benefits of these regulated supply restrictions, empirical evidence on the effectiveness of regulatory restrictions is clearly necessary. Our paper provides evidence in one specific context, but additional evidence on the effectiveness of such regulation is required in many other contexts.

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Table 1. 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 $)	39	11.88	11.8	1.028
Number of Machines Removed from Bars	39	3.517	3	2.131
Gambling per Machine Removed from Bars ($)	39	56,974	45,820	34,926
Characteristics of Postal Codes Within 1 km of Bars				
Number of Bankruptcy Filings (per Postal Code – Year)	3,515	0.22	0	0.88
Estimated Number of Households per Postal Code	3,515	28.07	17.4	40.02
Unemployment Rate (% in DA)	3,515	3.76	3.8	3.21
Average Income ($ in DA)	3,515	35,064	34,854	8,901
Marriage Rate (% in DA)	3,515	53.06	53.1	8.76
Completed College (% in DA)	3,488	17.36	16.67	4.67
Recent Immigrants (% in DA)	3,488	8.21	6.34	8.30
Homeownership (% in DA)	3,488	80.36	82.35	16.66

Notes: Summary statistics for all observations in our sample are presented here. 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. Annual Changes in Personal Bankruptcy After Slots Closure with Dollar Amount of Gambling Removed as Intensity of Treatment

Inner Ring	0-0.1 km	0.1-0.2 km	0.2-0.3 km	0.3-0.4 km	0.4-0.5 km
Outer Ring	0.5-0.6 km	0.5-0.6 km	0.5-0.6 km	0.5-0.6 km	0.5-0.6 km
(year 4)	-0.370	0.124	0.0196	0.144	-0.0118
	(0.238)	(0.138)	(0.106)	(0.138)	(0.188)
(year 3)	-0.474*	0.0899	0.0776	-0.00139	-0.0178
	(0.253)	(0.101)	(0.117)	(0.0985)	(0.175)
(year 2)	-1.008***	-0.00918	-0.112	-0.0973	0.0242
	(0.276)	(0.100)	(0.130)	(0.0932)	(0.0918)
(year 1)	-0.186	0.0593	0.247*	0.230	0.222*
	(0.220)	(0.0803)	(0.131)	(0.258)	(0.121)
(year 0)	-0.0540	-0.0782	0.0136	-0.0651	0.101
	(0.195)	(0.0827)	(0.0818)	(0.0864)	(0.133)
(year -1)					
(year -2)	-0.175	-0.0260	-0.0173	-0.0995	0.121
	(0.198)	(0.126)	(0.107)	(0.0954)	(0.0984)
(year -3)	-0.124	0.0596	0.00116	0.197	0.246**
	(0.237)	(0.115)	(0.127)	(0.165)	(0.118)
(year -4)	-0.113	0.110	-0.0714	-0.112	0.0414
	(0.297)	(0.0773)	(0.108)	(0.0871)	(0.106)
Observations	842	477	513	585	459
R-Squared	0.587	0.140	0.205	0.116	0.134

Notes: This table reports results for the model 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 report results only from the three-way interaction term interacting Near X Time X Log of 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 that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table 3. Annual Changes in Personal Bankruptcy After Slots Closure with Number of Machines Removed as Intensity of Treatment

Inner Ring	0-0.1 km	0.1-0.2 km	0.2-0.3 km	0.3-0.4 km	0.4-0.5 km
Outer Ring	0.5-0.6 km	0.5-0.6 km	0.5-0.6 km	0.5-0.6 km	0.5-0.6 km
(year 4)	-0.0852*	0.0171	-0.0175	0.0346	-0.00192
	(0.0512)	(0.0289)	(0.0173)	(0.0311)	(0.0307)
(year 3)	-0.104*	0.0209	-0.00890	-0.00873	-0.0130
	(0.0530)	(0.0253)	(0.0192)	(0.0160)	(0.0269)
(year 2)	-0.190***	0.00597	-0.0344	-0.0191	-0.000236
	(0.0621)	(0.0193)	(0.0215)	(0.0136)	(0.0144)
(year 1)	-0.132**	0.0180	0.0154	0.0663	0.0276
	(0.0552)	(0.0183)	(0.0229)	(0.0613)	(0.0205)
(year 0)	-0.0887	-0.0214	-0.00456	-0.0122	0.0182
	(0.0541)	(0.0198)	(0.0138)	(0.0108)	(0.0232)
(year -1)					
(year -2)	-0.0503	-0.00263	-0.0168	-0.0150	0.00942
	(0.0397)	(0.0221)	(0.0171)	(0.0123)	(0.0106)
(year -3)	0.0128	0.0157	-0.0172	0.0374	0.0162
	(0.0578)	(0.0238)	(0.0204)	(0.0333)	(0.0144)
(year -4)	-0.0944	0.0283	-0.0238	-0.0234*	-0.000355
	(0.0624)	(0.0216)	(0.0188)	(0.0125)	(0.0152)
Observations	842	477	513	585	459
R-Squared	0.581	0.111	0.207	0.130	0.132

Notes: This table reports results for the model 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 report results only from the three-way interaction term interacting Near X Time X Number of Machines Removed. This coefficient measures the effect of the removal of an extra slots machine from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table 4. The Effect of Slots Closure on Personal Bankruptcy with Dollars of Gambling Removed as Intensity of Treatment

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.459***	0.0251	0.0729	0.0847	-0.0477
	(0.159)	(0.0579)	(0.0562)	(0.0687)	(0.0693)
Observations	842	477	513	585	459
R-Squared	0.576	0.082	0.155	0.070	0.086
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	-0.411***	-0.241**	-0.144	-0.0910	-0.0799
	(0.149)	(0.116)	(0.0906)	(0.0734)	(0.0653)
Observations	2,642	2,822	3,038	3,326	3,488
R-Squared	0.532	0.524	0.519	0.514	0.511
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.459***	-0.276**	-0.179*	-0.129*	-0.120*
	(0.159)	(0.119)	(0.0918)	(0.0743)	(0.0665)
Observations	842	1,022	1,238	1,526	1,688
R-Squared	0.576	0.570	0.569	0.564	0.561

Notes: This table reports results for our main model in equation (2). Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Log of 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 that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table 5. The Effect of Slots Closure on Personal Bankruptcy with Number of Machines Removed as Intensity of Treatment

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.0910***	0.0161	0.00110	0.0209	-0.00558
	(0.0326)	(0.0156)	(0.00996)	(0.0145)	(0.0102)
Observations	842	477	513	585	459
R-Squared	0.572	0.064	0.156	0.075	0.088
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	-0.0781**	-0.0460**	-0.0169	-0.00534	-0.00352
	(0.0322)	(0.0221)	(0.0127)	(0.0103)	(0.00863)
Observations	2,642	2,822	3,038	3,326	3,488
R-Squared	0.529	0.521	0.517	0.512	0.509
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.0910***	-0.0574**	-0.0284**	-0.0167	-0.0148*
	(0.0326)	(0.0222)	(0.0131)	(0.0106)	(0.00898)
Observations	842	1,022	1,238	1,526	1,688
R-Squared	0.572	0.567	0.566	0.561	0.558

Notes: This table reports results for our main model in equation (2). Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Number of Machines Removed. This coefficient measures the effect of the removal of an extra slots machine from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table 6. No Effect of Slots Closure on Personal Bankruptcy with Other Available Slots Locations and Dollars of Gambling Removed as Intensity of Treatment

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	0.0244	-0.0108	-0.00142	0.00451	0.0323
	(0.0295)	(0.0273)	(0.0252)	(0.0356)	(0.0234)
Observation	1,674	1,746	2,071	2,121	2,099
R-Squared	0.088	0.122	0.112	0.113	0.082
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	0.00737	-0.0133	-0.0216*	-0.0223*	-0.0133
	(0.0251)	(0.0155)	(0.0127)	(0.0133)	(0.0112)
Observation	8,104	8,557	9,335	10,163	10,969
R-Squared	0.092	0.092	0.092	0.091	0.087
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 0.6 km	0.0244	0.0137	0.00638	0.00682	0.0143
	(0.0295)	(0.0231)	(0.0218)	(0.0222)	(0.0210)
Observation	1,674	2,127	2,905	3,733	4,539
R-Squared	0.088	0.095	0.101	0.105	0.096

Notes: This table reports results for a placebo test using the model in equation (2). This test is based on a sample of postal codes with a closed slots location and an alternative slots location available within 2 km. Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Log of 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 that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table 7. No Effect of Slots Closure on Personal Bankruptcy with Other Available Slots Locations with Number of Machines Removed as Intensity of Treatment

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	0.00756	-0.116	-0.0194	0.0107	0.0292*
	(0.0162)	(0.104)	(0.0229)	(0.0193)	(0.0151)
Observation	1,764	1,836	2,161	2,220	2,234
R-Squared	0.087	0.122	0.111	0.111	0.080
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	-0.00226	-0.000189	-0.0169	-0.00743	0.00168
	(0.0161)	(0.0152)	(0.0126)	(0.0105)	(0.00793)
Observation	8,500	8,953	9,731	10,568	11,419
R-Squared	0.088	0.088	0.088	0.088	0.084
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 0.6 km	0.00756	0.0126	-0.00143	0.00587	0.0142
	(0.0162)	(0.0166)	(0.0160)	(0.0151)	(0.0138)
Observation	1,764	2,217	2,995	3,832	4,683
R-Squared	0.087	0.092	0.100	0.104	0.093

Notes: This table reports results for a placebo test using the model in equation (2). This test is based on a sample of postal codes with a closed slots location and an alternative slots location available within 2 km. Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Number of machines removed. This coefficient measures the effect of the removal of an extra slots machine from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table 8. Larger Effects of Slots Closure on Personal Bankruptcy in Low-Income Areas (Dollars of Gambling Removed as Intensity of Treatment)

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.528**	-0.0609	0.0982	0.141	0.410
	(0.206)	(0.0782)	(0.117)	(0.0941)	(0.589)
Observation	500	288	225	207	198
R-Squared	0.485	0.150	0.197	0.164	0.166
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	-0.420**	-0.211	-0.155	-0.124	-0.121
	(0.189)	(0.146)	(0.131)	(0.123)	(0.118)
Observation	1,463	1,598	1,670	1,724	1,769
R-Squared	0.416	0.392	0.387	0.382	0.380
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.528**	-0.315**	-0.261*	-0.231*	-0.229*
	(0.206)	(0.153)	(0.137)	(0.129)	(0.124)
Observation	500	635	707	761	806
R-Squared	0.485	0.463	0.459	0.454	0.453

Notes: This table reports results for a heterogeneity test using the model in equation (2). This test is based on a sample of below-median income postal codes as defined by Census Canada. Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Log of 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 that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table 9. Smaller Effect of Slots Closure on Personal Bankruptcy in High-Income Areas (Dollars of Gambling Removed as Intensity of Treatment)

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.480*	0.133	0.0260	-0.00191	-0.0503
	(0.266)	(0.103)	(0.0664)	(0.0533)	(0.0646)
Observation	342	189	288	378	261
R-Squared	0.624	0.145	0.213	0.072	0.088
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	-0.416*	-0.311	-0.120	-0.0848	-0.0727
	(0.245)	(0.202)	(0.111)	(0.0847)	(0.0713)
Observation	1,179	1,224	1,368	1,602	1,719
R-squared	0.598	0.597	0.593	0.588	0.583
Inner ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer ring					
0.5 to 0.6 km	-0.480*	-0.353	-0.148	-0.112	-0.100
	(0.266)	(0.214)	(0.114)	(0.0871)	(0.0737)
Observation	342	387	531	765	882
R-squared	0.624	0.625	0.624	0.618	0.614

Notes: This table reports results for a heterogeneity test using the model in equation (2). This test is based on a sample of above-median income postal codes as defined by Census Canada. Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Log of 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 that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Figure 1. Number of Slot Locations in Alberta by Month in 2003–2013

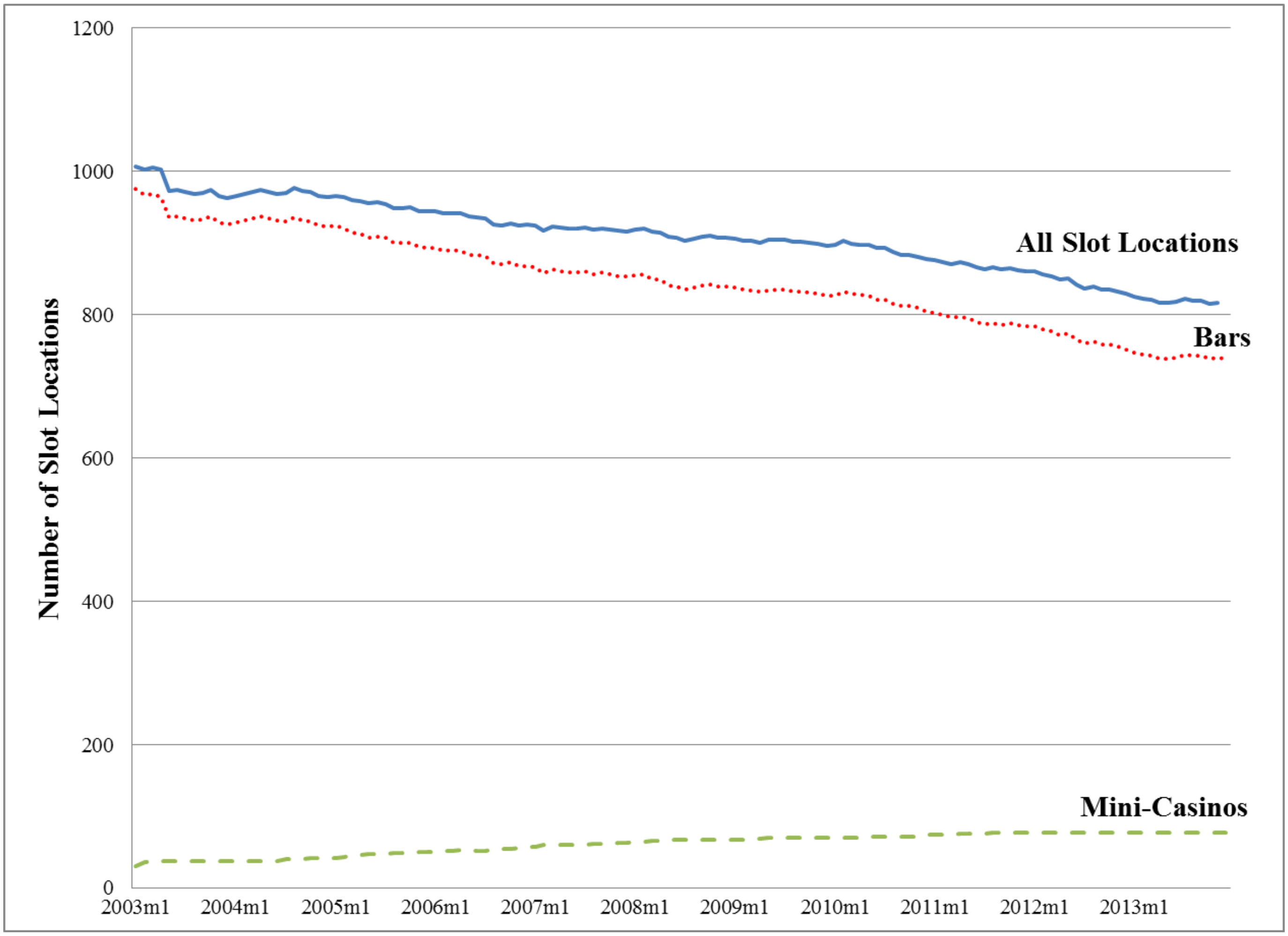


Figure 2. Number of Slot Machines in Alberta by Month in 2003–2013

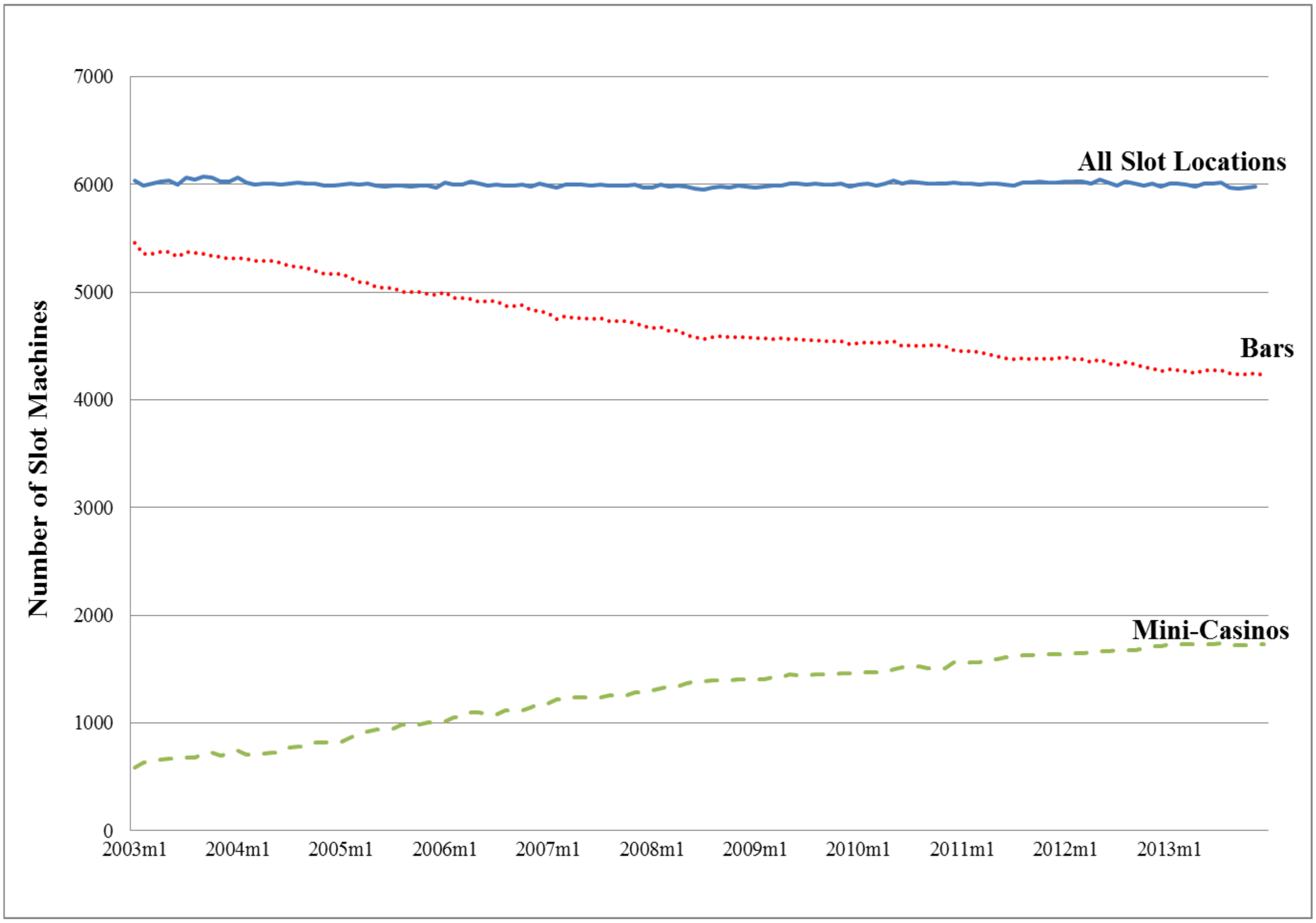


Figure 3. Slots Locations in Edmonton

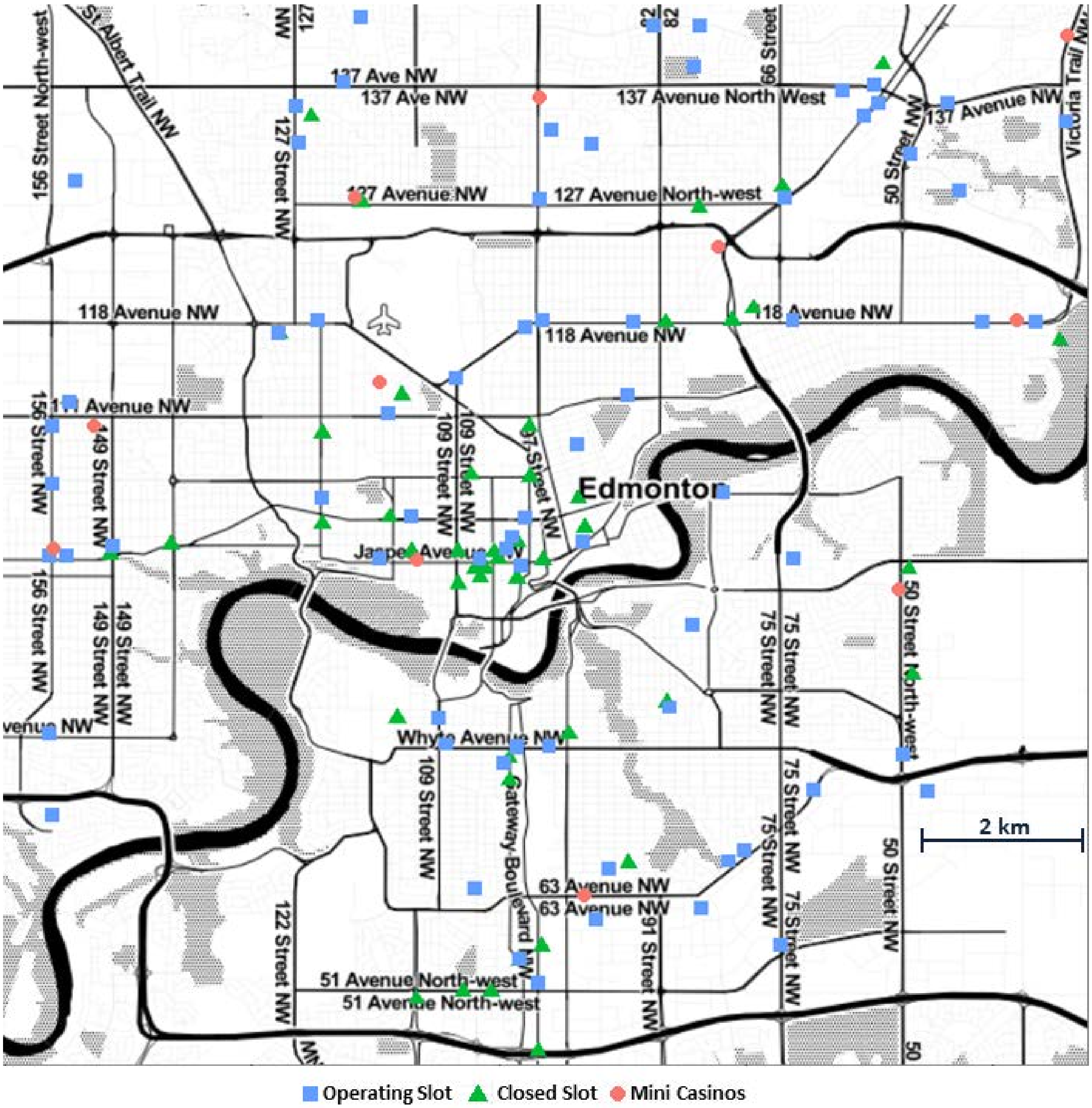


Figure 4. Slots Locations in Calgary

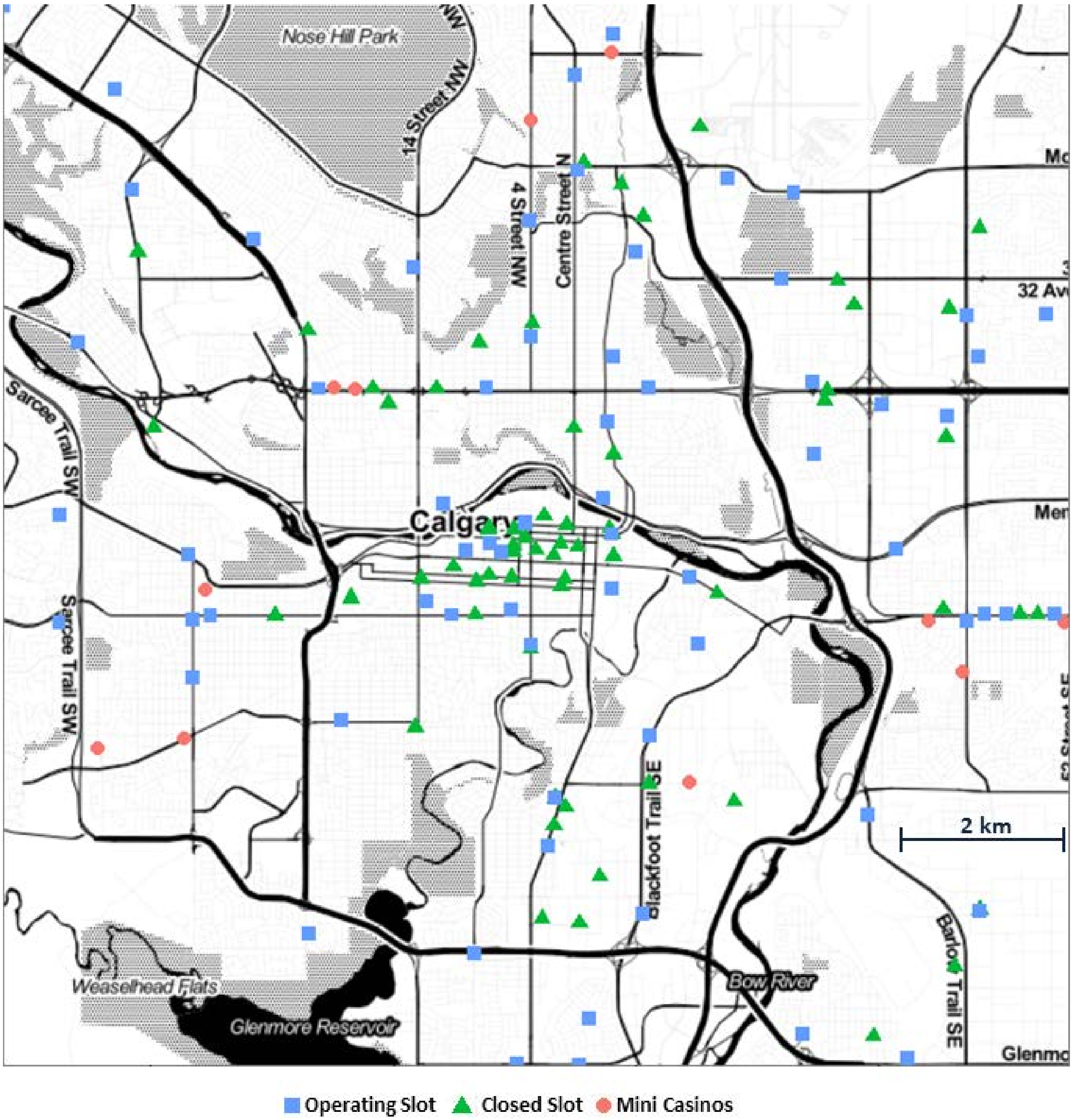
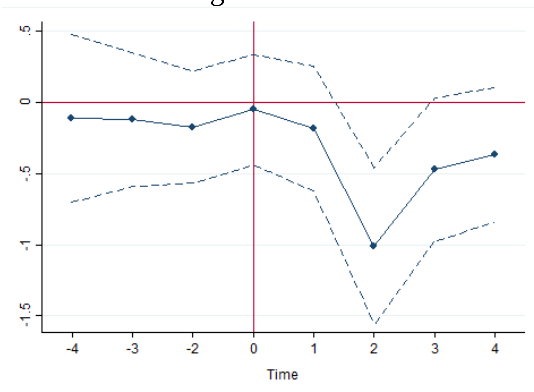
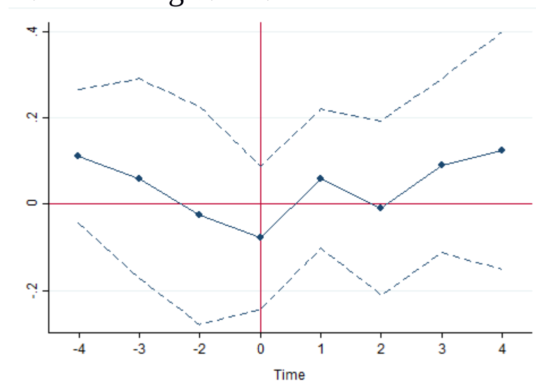


Figure 5. Effect of Slots Removal on Personal Bankruptcy of Close Neighbors with Dollar Amount of Gambling Removed as Intensity of Treatment

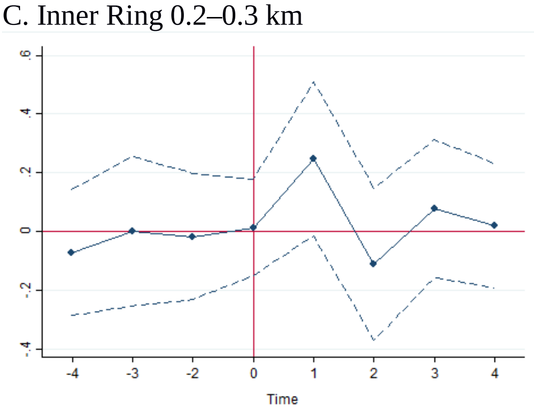
A. Inner Ring 0–0.1 km



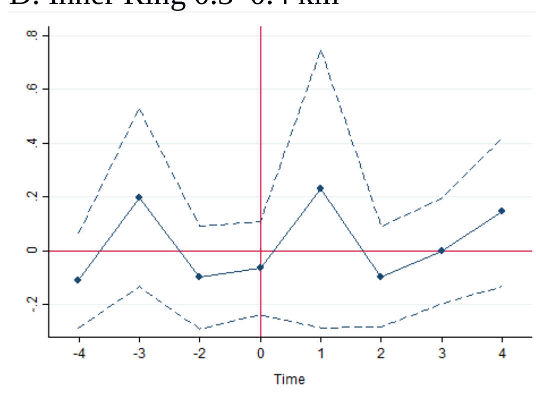
B. Inner Ring 0.1–0.2 km



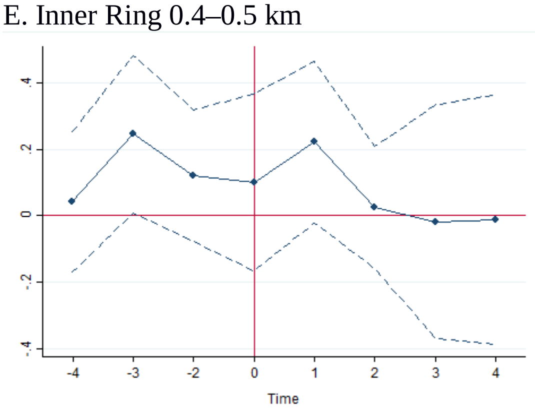
C. Inner Ring 0.2–0.3 km



D. Inner Ring 0.3–0.4 km

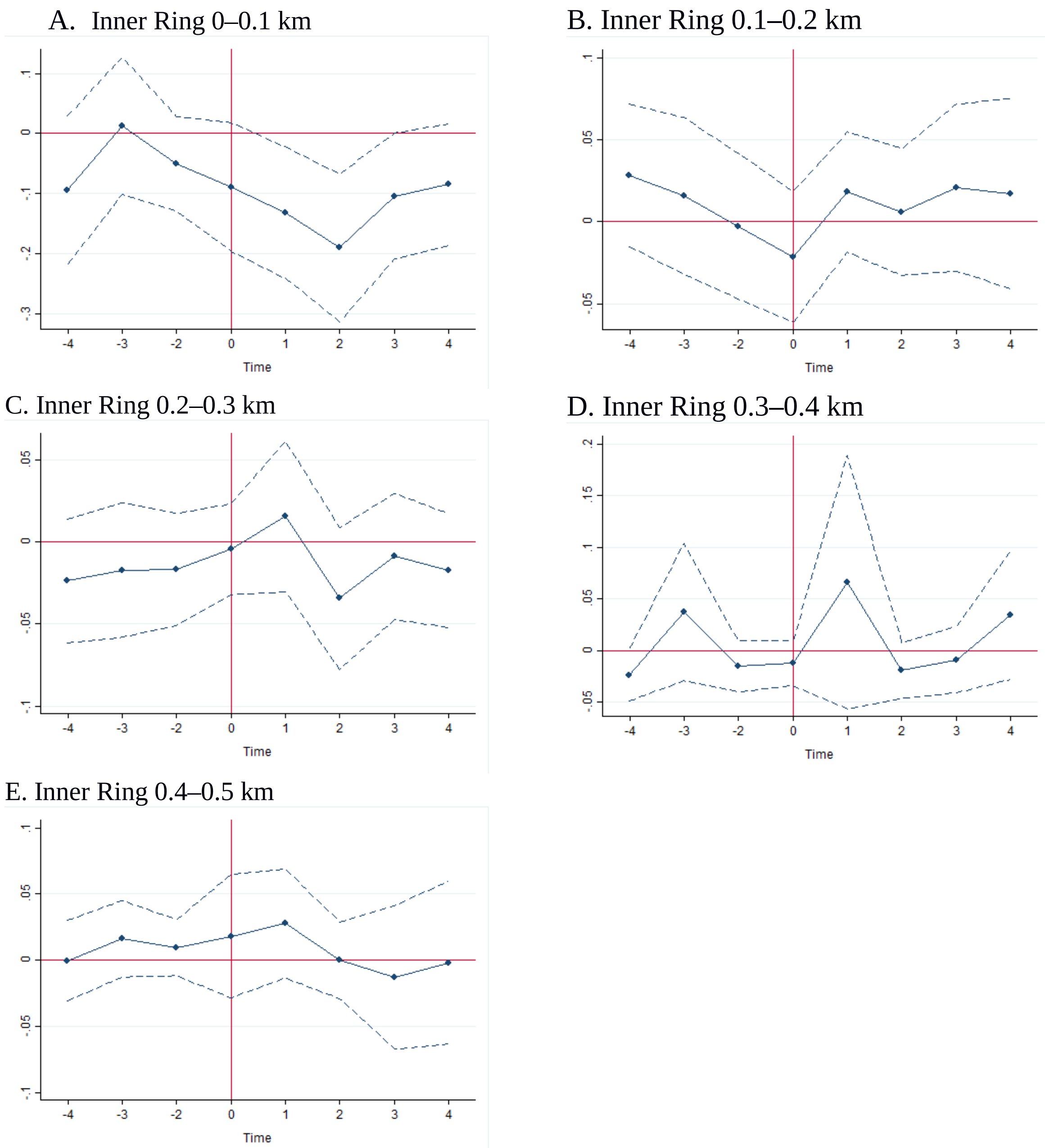


E. Inner Ring 0.4–0.5 km



Notes: This figure shows coefficients from the model in equation (1). Each dot represents a coefficient estimate, with dashed lines representing 95 percent confidence intervals. Figure headings show inner rings radii used. We use 0.5–0.6 km outer rings in all regressions. We report coefficients only from the three-way interaction term interacting Near X Time X Log of 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 that 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.

Figure 6. Effect of Slots Removal on Personal Bankruptcy of Close Neighbors with Number of Machines Removed as Intensity of Treatment



Notes: This figure shows coefficients from the model in equation (1). Each dot represents a coefficient estimate, with dashed lines representing 95 percent confidence intervals. Figure headings show inner rings radii used. We use 0.5–0.6 km outer rings in all regressions. We report coefficients only from the three-way interaction term interacting Near X Time X Number of Slot Machines. This coefficient measures the effect of the removal of an extra slot machine from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies that 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.

Appendix

In this Appendix, we describe various additional institutional details related to slot machines in Canada and the quasi-natural experiment implemented by the Alberta Gaming and Liquor Commission (AGLC) to reduce access to slots machines.

A.1. Timing of Slot Removal

We argue here, based on very idiosyncratic institutional details unique to our context, that the *exact timing* of the removal of slot machines from a specific bar can be considered plausibly exogenous.

Recall that the aim of the AGLC policy described here was to concentrate the geographic supply of slot machines into fewer, but larger, locations. During the entire period of our study, Alberta provincial legislation mandated that the *total* number of slots in *both* bars and mini-casinos could not exceed 6,000.[9] While the maximum number of machines is fixed at 6,000, however, under this legislation, the AGLC is free to distribute slots across different locations.

The AGLC serves as both a regulator and a profit maximizer (of government revenues). 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 was to ensure that all 6,000 slot machines approved by the province were continuously operating at any given time, rather than "gathering dust in a warehouse," in the words of an AGLC official. Thus, the important element of these institutional details for our purposes is that the AGLC had an incentive to only remove slots from bar locations at the point of time when a new mini-casino facility, at some *unrelated* geographic location, had been constructed and was able to start operating.

These motivations of the AGLC are reflected in Figures 1 and 2. Figure 1 shows that, over this period, the number of slot locations in Alberta was significantly reduced because of the reduction in bars providing slots even while a smaller number of new (larger) mini-casinos were built and began operating. However, Figure 2 shows that the total number of slot machines in operation each month remained approximately constant at the 6,000 ceiling imposed by

[9] 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 racetracks (which are governed by other legislation to that examined here).

provincial legislation.[10] The fact that the number of operating slot machines remained approximately constant at the legislative maximum (6,000 machines) for every month over this entire period reflects the fact that the AGLC only shifted machines from a bar to a new mini-casino when the new mini-casino was ready to start operating. We thus argue that, because the timing of the building and the opening of the new mini-casino (at some unrelated location) can be considered plausibly exogenous with respect to the bar losing its slot machines, the *exact date* at which a specific bar had its slot machines removed can also be considered plausibly exogenous.

A.2. Can Bar Owners Influence the AGLC Decision to Remove Their Slots?

In this section, we provide institutional evidence that is consistent with individual bar owners not being able to manipulate the AGLC decision to remove slot machines from their bar (i.e., this decision is not endogenous to the bar owner).

The mechanism by which the AGLC selected bars that would have their slots removed was based on revenues per slot machine (see AGLC, 2001), with bars with lower revenue per machine having their machines removed by the AGLC. The AGLC (2001; p. 39) described this decision-making process as follows: "Based on specific criteria, [slot machines] would be re-allocated from locations with lesser demand or poorer sales to those experiencing greater demand or higher sales." We argue here that there are a variety of institutional details that preclude individual bar owners from manipulating this decision process to their own advantage.

First, all slot machines in the province are identical because all are provided to bar owners by the AGLC; thus, it is not possible for a bar owner to increase slot revenue by changing the characteristics of their machines. Second, all bar owners can be assumed to be revenue maximizers in all periods, because of the 85 percent/15 percent revenue split with the provincial government, whether or not they are facing an imminent threat 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. Third, the mechanics of this AGLC decision rule are transparent to all bar owners

[10] Figure 4 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 were moved from one location to another within the same month.

over time. Bar owners are aware of revenues generated by competing bars because the average slot machine revenues are reported each year in the AGLC Annual Report.

Fourth, there is strong excess demand by bar owners for slot machines, as indicated by a long waiting list of bar owners wanting to install slot machines. The strong incentives for bar owners to have slot machines in their locations arise because of the direct revenue from the slot machine they receive but also possibly because of the increased alcohol sales from users of the slot machines. This significant excess demand for slot machines by bar owners without slots implies that bar owners who currently have slot machines will be very unlikely to unilaterally (i.e., endogenously) give up their slot machine licenses from the AGLC.[11]

[11] 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 shut it down completely. This is because such a bar would have two very valuable assets — a liquor license and an allocation of slot machines — that would both lose all their value if the bar was shut down; however, they would both retain their value if the bar was sold as a going concern.

Table A1. Larger Effects of Slots Closure on Personal Bankruptcy in Low-Income Areas (Number of Machines Removed as Intensity of Treatment)

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.108*	0.00277	-0.0118	0.0319	0.0210
	(0.0637)	(0.0168)	(0.0210)	(0.0197)	(0.0301)
Observation	500	288	225	207	198
R-Squared	0.473	0.144	0.195	0.168	0.166
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	-0.0959	-0.0334	-0.0164	-0.00260	-0.00149
	(0.0624)	(0.0329)	(0.0243)	(0.0201)	(0.0174)
Observation	1,463	1,598	1,670	1,724	1,769
R-Squared	0.406	0.385	0.381	0.377	0.375
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.108*	-0.0537	-0.0372	-0.0230	-0.0214
	(0.0637)	(0.0343)	(0.0255)	(0.0211)	(0.0182)
Observation	500	635	707	761	806
R-Squared	0.473	0.454	0.451	0.446	0.446

Notes: This table reports results for a heterogeneity test using the model in equation (2). This test is based on a sample of below-median income postal codes as defined by the census in Canada. Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Number of machines removed. This coefficient measures the effect of the removal of an extra slots machine from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.

Table A2. Smaller Effect of Slots Closure on Personal Bankruptcy in High-Income Areas (Number of Machines Removed as Intensity of Treatment)

	(1)	(2)	(3)	(4)	(5)
Inner Ring	**0-0.1 km**	**0.1-0.2 km**	**0.2-0.3 km**	**0.3-0.4 km**	**0.4-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.0769*	0.0396	0.00406	-0.000568	-0.00852
	(0.0400)	(0.0306)	(0.0116)	(0.0159)	(0.0106)
Observation	342	189	288	378	261
R-Squared	0.623	0.145	0.217	0.072	0.087
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 1 km	-0.0667*	-0.0540*	-0.0135	-0.00759	-0.00628
	(0.0359)	(0.0316)	(0.0141)	(0.0112)	(0.00917)
Observation	1,179	1,224	1,368	1,602	1,719
R-Squared	0.597	0.595	0.592	0.587	0.583
Inner Ring	**0-0.1 km**	**0-0.2 km**	**0-0.3 km**	**0-0.4 km**	**0-0.5 km**
Outer Ring					
0.5 to 0.6 km	-0.0769*	-0.0607*	-0.0193	-0.0132	-0.0119
	(0.0400)	(0.0339)	(0.0153)	(0.0120)	(0.00997)
Observation	342	387	531	765	882
R-Squared	0.623	0.623	0.623	0.618	0.613

Notes: This table reports results for a heterogeneity test using the model in equation (2). This test is based on a sample of above median income postal codes as defined by the census in Canada. Each cell is a separate regression with inner rings and outer rings of different radii as specified in the cell header. We report results only from the three-way interaction term interacting Near X Time X Number of machines removed. This coefficient measures the effect of the removal of an extra slot machine from the inner ring (relative to the outer ring) for each year (relative to the omitted year t=-1). A negative coefficient implies that 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 postal code and reported in parentheses. Statistical significance at 1, 5, 10 % levels is denoted by ***, **, and *, respectively.