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Did video gaming expansion boost municipal revenues in Illinois?

Gary A. Wagner[1] | **Douglas M. Walker**[2]

[1]Department of Economics and Finance, University of Louisiana at Lafayette, Lafayette, Louisiana, USA

[2]Department of Economics, School of Business, College of Charleston, Charleston, South Carolina, USA

Correspondence
Gary A. Wagner, Department of Economics and Finance, University of Louisiana at Lafayette, Lafayette, LA, USA.
Email: gary.wagner@louisiana.edu

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Abstract

One supposed benefit of authorizing video gaming terminals (VGTs) outside of casinos is to improve the fiscal health of local governments. Illinois passed the Video Gaming Act in 2009, enabling individual municipalities to allow VGTs. To date, the machines have generated $400 million in municipal tax revenues and $2 billion for the state. We use a difference-in-differences strategy that adjusts for staggered adoption to isolate the causal effect of VGTs on municipal revenues. We find that VGTs displace other local taxable retail, leaving total municipal revenues unchanged. The video gaming act merely reallocated economic activity and did little to improve municipal finances.

KEYWORDS

casinos, difference-in-differences, municipal revenue, variable treatment timing, video gaming

JEL CLASSIFICATION

H2, R1, R5, L83

1 | INTRODUCTION

Gambling continues to be a popular pastime in the United States, with legal lotteries and casinos in most states; recently, online gambling and sports betting have also become more common. While the expansion of gaming may partly reflect changing tastes and preferences, state and local policymakers often turn toward gambling to generate much-needed revenues and boost economic development. In fact, the December 2016 issue of State Legislatures magazine, published by the National Conference of State Legislatures, noted that "revenues from gambling

offer an appealing alternative to the politically unpopular, increasingly undoable and invariably conflict-laden effort to hike taxes."[1]

Seven states—Illinois, Louisiana, Montana, Nevada, Oregon, Pennsylvania, and West Virginia—currently allow video gambling machines outside of casinos.[2] The policy space is an active one: In 2019 alone, the Pennsylvania Gaming Board approved its first locations, and Illinois passed legislation to further expand video gambling. Tax revenues from gambling generally are split between state and local governments, with the fiscal health of local governments often cited as a benefit of allowing gaming (Alesch, 2015). Our objective in this paper is to investigate the extent to which video gaming has affected the fiscal health of municipal governments in Illinois.

Among the first states to introduce a lottery (1974) and riverboat casinos (1990), Illinois has also been one of the most aggressive in legalizing new types of gambling. Illinois's casino taxes are some of the highest in the United States, with both a casino admissions tax and a progressive tax on casino revenues. The top marginal tax rate is 50%, the highest tax rate in the Midwest.[3] The local (casino-hosting) government receives $1 of the admission tax, plus 5% of casino revenues.[4] Since 2007, Illinois riverboat casino revenues have been declining. In 2007, total riverboat casino revenues were $1.98 billion; state taxes were $718 million, and local casino tax revenue was $115 million. By 2018, total revenues had fallen to $1.37 billion, with $382 million in state taxes, and $79.6 million in local taxes (Illinois Gaming Board, 2018).

In 2009, Illinois passed the Video Gaming Act (VGA), which enabled individual municipalities to allow gambling machines in venues with a liquor license, as well as truck stops and fraternal and veterans' establishments.[5] Each venue in a municipality that approved video gaming terminals (VGTs) could be licensed to operate as many as five machines. Tax revenues are shared between the municipality and the state.

The VGA was passed toward the end of the Great Recession when the Illinois unemployment rate was over 11%.[6] Key motivations for the Act were to raise additional tax revenues to fund capital projects and to provide municipalities with a mechanism to supplement local tax revenues (Warnick, 2011).[7] The Act stipulated that VGT income would be taxed at a rate of 30%. Of that, 25% goes to the state, and 5% to the municipality that has licensed the machines.[8] The Act provided a new potential source of municipal tax revenue, one that had previously been available only to the 10 Illinois communities with casinos permitted by the Riverboat Gambling Act (1990).

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[1]The issue of State Legislatures magazine is available online at: https://www.ncsl.org/Portals/1/Documents/magazine/articles/2016/SL_1216-Gambling.pdf. The quote is on page 21.

[2]In addition, Delaware allows charitable video lottery terminals (VLTs), and New Mexico allows some at veteran and fraternal organizations; Arkansas and Kentucky have instant racing machines at racetracks (American Gaming Association, 2019).

[3]At one time, the highest marginal tax rate on casino revenues was 70%, suggesting Illinois might be following a tax-revenue maximization strategy, similar to that of state lotteries (Garrett, 2001).

[4]Source: Illinois Gaming Board, Monthly Casino Report, January 2020.

[5]As noted by Toossi and Zhang (2018), the VGA was later amended to allow VGTs in golf and yacht clubs, and other organizations. Other establishments have applied for liquor licenses in order to be able to offer VGTs.

[6]Source: Bureau of Labor Statistics.

[7]According to Warnick (2011), the Act was "introduced and passed in both the Senate and House in just over twenty-four hours at the very end of the legislative session" (pp. 783–784). Grotto et al. (2019) suggest that the bill was largely written by gaming industry lobbyists and was not debated by legislators.

[8]For most types of gambling, income or revenue is defined as total amount bet minus prizes paid to customers. In other words, this is the amount lost by customers. This amount is commonly called gross gaming revenue (GGR).

In 2019, Illinois passed Senate Bill 690, massively expanding legal gambling. The bill was part of a larger economic plan that legalized sports betting, approved up to six new casinos, approved casino games at racetracks, and increased from five to six the number of VGTs allowed at individual establishments. Senate Bill 690 also increased the tax levied on VGTs from 30% to 34%, earmarking the additional tax revenue for the Illinois Gaming Board (IGB). Illinois lawmakers' expansion of VGTs would seem to demonstrate their continued conviction that VGTs benefit the state.

As a result of its enthusiasm for legalized gambling, Illinois has been referred to as a "Midwest gambling mecca" and the "unofficial video gaming terminal capital of the U.S." (Channick, 2019). At the time Illinois SB 690 was passed, expectations for new gaming revenue were so high that private organizations like the Illinois Section of the American Society of Civil Engineers envisioned them playing a central role in funding infrastructure improvements (Illinois Section of the American Society of Civil Engineers, 2019).

Critics of the move to expand gambling in 2019 argued that there was little evidence that the 2009 VGA had been a success (Grotto et al., 2019; Warnick, 2011) and that despite almost a decade-long experiment with VGTs, there was scarce empirical evidence one way or the other about VGTs' efficacy as a public finance tool.

Using administrative data from the Illinois Comptroller's Office and, as a comparison group, municipalities in the state that have never permitted gaming, we pursue a difference-in-differences identification strategy to isolate the causal effect of authorizing and operating video gaming on municipal revenue. We adjust for staggered treatment timing using the stacked regression design proposed by Cengiz et al. (2019). We also leverage the variable treatment timing and explore the robustness of our results by expanding the comparison group to include "not-yet-treated" municipalities. Our results are robust to alternative subsamples and comparison groups.

Our analysis finds no evidence that total municipal revenues have increased as a result of VGT authorization. In fact, we find that real per capita local option sales taxes *fall* by $8 relative to the comparison group. This roughly 30% reduction in annual revenues indicates that spending at the VGTs displaces other taxable retail spending in jurisdictions with expanded gaming. For a municipality with an average local option sales tax rate, we estimate the displaced spending to be approximately $1.5 million per year.

We also find evidence of heterogeneous effects as municipalities that generate higher amounts of gaming revenue (95th percentile and up) are found to experience stronger displacement effects in local retail. In these municipalities, our estimates reveal that authorizing VGTs reduces local option sales tax revenues by as much as 50%. This implies that additional gaming expansion may exacerbate the cannibalization of local retail and redistribution of municipal economic activity. Overall, our results suggest that the expansion of video gaming has led to a substantial reallocation of municipal economic activity while doing nothing to bolster the fiscal health of municipal governments in Illinois.

The important information provided by our analysis may help Illinois municipalities and voters to (re)evaluate their positions on VGTs. This information can also be helpful for policymakers in jurisdictions such as the state of Missouri, which is considering the expansion of gambling, and for those in other states and localities struggling with fiscal stress.[9]

[9]https://www.missourinet.com/2020/01/29/sports-wagering-bill-is-now-heading-to-missouri-house-floor/.

2 | BACKGROUND

The prevalence of legalized gambling as a public finance tool has grown dramatically since New Hampshire introduced its lottery in 1964. By 2019, all but five states (Alabama, Alaska, Hawaii, Nevada, and Utah) had adopted lotteries. Casino gambling was introduced outside Nevada and Atlantic City, NJ, beginning with South Dakota, in 1989. By the end of 2019, 41 states had at least one tribal or commercial casino (American Gaming Association, 2019). With the "lottery tax" around 30%, and the average tax on gross casino revenues around 25%, it is clear why expansion of legal gambling is attractive to politicians.

2.1 | Overview of gambling research

Among the different sectors of the U.S. gambling industry, casinos and lotteries have garnered the most interest from researchers. Most "economics of gambling" studies fall into one of five categories: the decision to adopt; interindustry relationships; tax impacts and regressivity; positive economic benefits, such as growth and employment; and negative social impacts.[10]

Although different jurisdictions have different experiences with legal gambling, the academic literature, as a whole, suggests that the U.S. casino industry provides modest tax revenue, economic growth, and employment even as the industry's different sectors grow partially at one another's expense. For their part, politicians likely see legalizing a new form of gambling as a tool to avoid or delay either tax increases or spending cuts. While there may be significant social harms that come from legalized gambling, most of them attributable to gambling disorders, these have proven difficult to quantify. The bottom line is this: While academics continue to debate the costs and benefits of legalized gambling, politicians across the United States continue to expand it.

2.2 | Research on VGTs

Studies of VGTs are uncommon, both because VGTs outside casinos are relatively new in the industry and because they are allowed in only six states. Nevertheless, several studies provide valuable background for our analysis of Illinois.

Increased revenues from VGT gaming are likely to come at the expense of revenues from other industry sectors, including casinos. For example, Phipps et al. (2020) find that VGTs have harmed Illinois casino admissions and revenues. This is an important finding insofar as it implies that locally available VGTs provide a suitable substitute for some potential casino patrons. However, it should also be noted that VGT availability in Illinois counties bordering other states may attract out-of-state players. Tosun and Skidmore (2004) find evidence of this when racetracks in West Virginia began offering VGTs.

In a series of interviews with six municipal budget directors in Illinois, Dudzinski (2017) addresses whether VGT revenues have fostered expanded services for residents. Based on his small sample, Dudzinski (2017, p. 32) concludes, "revenues from state regulated video terminals [have] had only a small impact on their budget and that virtually no services of any kind have

[10]For a recent, concise overview of the literature, see Walker and Sobel (2016).

expanded due to these revenues." One likely explanation for this is that the VGT revenues represent only 0.5% of municipalities' total revenues (p. 24).

As discussed by Bogot (2014), Illinois and many other states have long tolerated "gray area" machines which offer games of chance in a way that avoids their legally being considered gambling. For example, the player may buy phone or internet time coupons, or the payout may be in coupons instead of money. While it is likely that these machines become less popular as more traditional machine games become legal, some business owners may still see them as attractive because they are not taxed.

In West Virginia, Humphreys (2021) finds that the introduction of sports betting has cut into revenues from gaming machines in casinos. This finding has important implications for Illinois, as sports betting was authorized as part of SB 690, the 2019 bill that expanded gambling.

Using data from Illinois, Toossi and Zhang (2018) examine the motivations for municipalities' authorizing VGTs. They discuss four distinct motivations: "revenue," or the need to raise tax revenues due to fiscal stress; "political," the political expedience of introducing and taxing casinos rather than raising other types of taxes; "economic development," the potential reduction of other taxes or the spur to local tourism that increases local economic development; and "competition," the introduction of gambling in order to keep citizens and potential tax revenues within the jurisdiction (Toossi & Zhang, 2018, p. 71).

Among the various motivations for the introduction of VGTs, Toossi and Zhang also find evidence of partisan politics. That is, they find a significant relationship between local support for Republicans and the probability of adoption, interpreting their finding (a 10% increase in support for Republican candidates increases the probability of VGT adoption by 1.5%) as evidence of Republicans' fiscal conservatism and suggesting that the GOP is more likely to support VGTs than to support increasing other taxes.

While statistically significant, one can argue whether this political variable is meaningful.[11] Several demographic factors were also significant. Specifically, higher proportions of educated residents, religious adherents, and elderly residents all reduce the probability of VGT adoption, with stronger significance attached to these variables than to the Republican-support (i.e., political) variable.

One factor that seems overlooked in the adoption analysis, likely due to a lack of suitable data, is whether local residents enjoy gambling. If citizens enjoy playing slot machines, they may be more likely to approve VGTs being available at their favorite local restaurant or bar. In fact, research has shown that making gambling opportunities available nearby may be a significant benefit to legalized gambling (Grinols, 1999). Toossi and Zhang (2018) acknowledge that residents' affinity for gambling might be a factor affecting VGT adoption, though they do not directly test for it.

Toossi and Zhang (2018) do not address the question of whether municipalities have benefited from VGTs in terms of net new revenues, but they do explore factors correlated with aggregate municipal VGT revenues. Using data from 2012 to 2015, they find that municipalities with larger populations and more establishments hosting VGTs experience significantly more aggregate gaming revenues, as would be expected. Better educated and married residents were associated with lower aggregate VGT revenues. In addition, lower VGT revenues correlate with

[11]Consider that an enormous 50% increase in support of Republicans only increases the probability of VGT adoption by 7.5%.

municipalities located in the same counties as casinos, suggesting substitution for casino and VGT gambling.

Finally, the recent study by Grumstrup and Nichols (2021) examine Illinois VGTs using local demographic data and 2017 VGT data. In line with other research, they found that casinos and VGTs act as substitutes. More importantly, they analyzed VGT revenues and number of machines operating alongside local socioeconomic data, finding a positive relationship between local poverty rates and the density of VGTs and expenditures on VGTs. Specifically, a 1% higher poverty rate is associated with 1.47% higher VGT expenditures per capita and 1.17% more VGTs per 10,000 population. This finding suggests that the tax revenue raised from Illinois VGTs is regressive.

3 | DATA AND IDENTIFICATION STRATEGY

3.1 | Data

We combine data from the IGB, 2000 Decennial Census, and the Illinois Comptroller's Office to estimate the causal relationship between VGT expansion and municipal revenues. The Comptroller's Office publishes detailed annual financial reports documenting revenues, expenditures, indebtedness, assets, and liabilities for almost 5000 distinct governmental units in the state, ranging from cities and counties to special sanitation, housing, and fire districts. These rich financial data are currently available annually beginning in fiscal year 1994.[12]

To illustrate the richness of the fiscal data, Table 1 shows real per capita revenue sources for Illinois municipalities in our empirical sample (discussed in more detail in Section 3.2). We use the term municipality to describe any local governmental unit. We further split the data to show averages across all municipalities from 1994 to 2008 and 2009 to 2018 because these represent the pre- and post-VGA periods, respectively. Across the municipalities in our sample, real per capita total revenues average around $1,100 before the VGA was passed and $1,500 after the Act was passed. In terms of component sources of revenue, Illinois municipalities generate roughly one-third from service charges, which include fees for local utilities, garbage collection, parking, and other amenities such as libraries, parks, and public golf courses. Property taxes, a primary source of local revenue for many municipalities in the United States, account for between 17% and 18% in Illinois municipalities. In terms of relative importance, this revenue stream has remained very stable over the past quarter century.

As it is in most states, intergovernmental revenue is a key source of funding for municipalities in Illinois. State aid accounts for roughly 25% of the average municipality's total revenue, and direct federal aid accounts for an additional 2%–3%. In terms of relative importance, the sharing of sales tax and income tax revenue constitutes the bulk of state aid (10% and 7% of total revenue, respectively). Although the state collects and distributes a variety of sales and use taxes for local governments, the majority of state sales tax aid comes from a 20% share of the state's general sales tax and a 100% share of the state's 1% rate on food, drugs, and medical appliances.[13] Use tax revenue from out-of-state purchases is dispersed to municipalities based on the

[12]The annual data files are available at: https://illinoiscomptroller.gov/financial-data/local-government-division/financial-databases/.

[13]Detailed information on the sources of state sales and use tax aid for local governments is available here: https://www2.illinois.gov/rev/localgovernments/LocalTaxAllocation/Pages/Taxes-Distributed-to-Local-Governments.aspx.

TABLE 1 Sources of municipal revenue: Real per capita and shares

(1)	(2)	(3)	(4)	(5)
Source	1994–2008	Share (%)	2009–2018	Share (%)
Total revenue	1117.72	100.00	1476.81	100.00
Property tax revenue	186.30	16.70	263.90	17.90
Other revenue	0.07	0.00	0.00	0.00
Miscellaneous revenue	109.53	9.80	140.29	9.50
Interest	39.24	3.50	25.80	1.70
Service charges	305.48	27.30	547.34	37.10
Fines	14.17	1.30	16.94	1.10
Licenses and permits	19.34	1.70	20.63	1.40
Other intergovernmental revenue	9.06	0.80	5.54	0.40
Federal revenue	37.51	3.40	31.16	2.10
Other state revenue	41.82	3.70	41.17	2.80
State replacement revenue	15.31	1.40	15.16	1.00
State motor fuel revenue	40.32	3.60	31.60	2.10
State sales tax revenue	115.95	10.40	153.94	10.40
State income tax revenue	102.11	9.10	102.75	7.00
Gaming revenue	2.23	0.20	6.49	0.40
Other tax revenue	11.71	1.00	19.78	1.30
Utility tax revenue	33.35	3.00	40.71	2.80
Local sales tax revenue	34.29	3.10	16.61	1.10
Total state aid	315.50	28.20	344.64	23.30

Note: Real per capita figures in columns (2) and (4) are scaled by the 2000 Census population estimates and deflated using the CPI (2018 = 100). Mean values are shown for the periods from 1994 to 2008 and 2009 to 2018. All raw municipality finance data were obtained from the Illinois Comptroller's Office local government division. The fiscal year data files are available at: https://illinoiscomptroller.gov/financial-data/local-government-division/financial-databases/. Our full sample includes 800 municipalities. See Section 3.2 for details.

residence of the buyer, while distributions of the general and food/medical sales tax revenues are dispersed on a per capita basis. On the income tax side, funding has been shared with municipalities on a per capita basis through the Local Government Distributive Fund since the state enacted a personal income tax in 1969. Currently, local governments receive (in aggregate) 6.06% of all personal income tax and 6.85% of all corporate income tax collected. It is important to note that while state aid is one of the largest sources of municipal revenue, the formula-based distribution schemes leave municipalities with minimal control over it.

Property tax revenues, service charges, and miscellaneous revenues account for almost 90% of the average municipality's discretionary revenue streams, about 64% of total revenue. Gaming revenues, which include riverboat casinos, racing and VGTs, accounted for an average of 0.2% of total revenue from 1994 to 2008 and 0.4% from 2009 to 2018. The contribution made by gaming revenues may be more significant than these numbers suggest, however, both because figures in Table 1 include municipalities that do not allow gaming (and therefore have zero revenues) and because they cover a period of rapid expansion of gaming across the state. (More on

this is below). In municipalities that received some gaming revenues in 2018, the gaming component accounted for almost 1.5% of the total (about $25 per capita).

Illinois is a home rule state. Municipalities with home rule status have the ability to self-govern on all matters unless the General Assembly expressly has limited that power or retained the right to exercise authority in a particular area. From a fiscal perspective, home rule municipalities have far more discretion than non-home rule jurisdictions (Wood, 2011). Municipalities automatically achieve home rule status once their population reaches 25,000; smaller municipalities may a hold a referendum to achieve it. According the Illinois Municipal League, there are 217 home rule municipalities in the state.[14]

In accordance with the VGA, individual municipalities have the option to pass an ordinance that prohibits video gaming in any establishment within the municipality's corporate limits. For unincorporated areas, expansion authority rests with the appropriate county board. The IGB maintains an updated list of the legality of VGTs in 1496 distinct towns, villages, cities, and unincorporated areas in the state.[15]

At the time of this writing, the Gaming Board has confirmed that 1091 municipalities have explicitly authorized VGTs, 119 have prohibited VGTs, and the status of the remaining 286 municipalities is unknown/unverified. The timing of municipal adoptions (by Fiscal Year) are shown in Figure 1 alongside the expansion of business establishments with gaming terminals and the level of aggregate real per capita municipal gaming revenue.

As Panel A shows, more than half of the 1091 (verified) municipalities that allow VGTs authorized them either in the same fiscal year or in the year prior to the first operation of live terminals (October 2012 or FY 2013). Since that time, the rate of expansion has slowed gradually from 54 new municipalities authorizing VGTs in FY 2014 to fewer than nine per year by FY 2018.

Unlike VGT authorizations, both the number of establishments with VGTs and the aggregate number of terminals operating in the state have steadily increased since 2012. This is shown in Panel B of Figure 1. At the end of 2018, more than 32,000 terminals were operating in more than 15,000 establishments within the state. A naive look at aggregate per capita gaming revenue for municipalities, shown in Panel C, suggests that VGT expansion may have provided a boost to municipal revenues. Between 2010 and 2012, after the VGA was passed but before VGTs began operating, real per capita municipal gaming revenues averaged around $2.80. Revenues increased sharply beginning in FY 2014 and have grown steadily with the expansion of terminals and establishments. By FY 2018, real per capita municipal gaming revenue had reached almost $16, five times what it had been less than a decade earlier.

The rapid expansion of terminals around the state has led to an increase in net terminal income (NTI) and, consequently, tax revenue for the state and municipalities. NTI is the money players spent at VGTs less their winnings. As specified by law, total tax revenues equal 30% of NTI, with the state receiving 25% and the municipality hosting the licensed terminal receiving the remaining 5%. NTI is likely to be the primary mechanism through which expansion of gaming impacts municipal revenues.

Figure 2 shows the growth in aggregate terminal tax revenues from 2012 to 2018. In the calendar year 2018, the state received nearly $450 million in terminal tax revenue while municipalities across the state shared close to $75 million. Since the terminals began operating in October 2012, the state has received just shy of $2 billion in video gaming tax revenue and

[14]See www.iml.org/homerule.

[15]Available here: https://www.igb.illinois.gov/VideoProhibit.aspx.

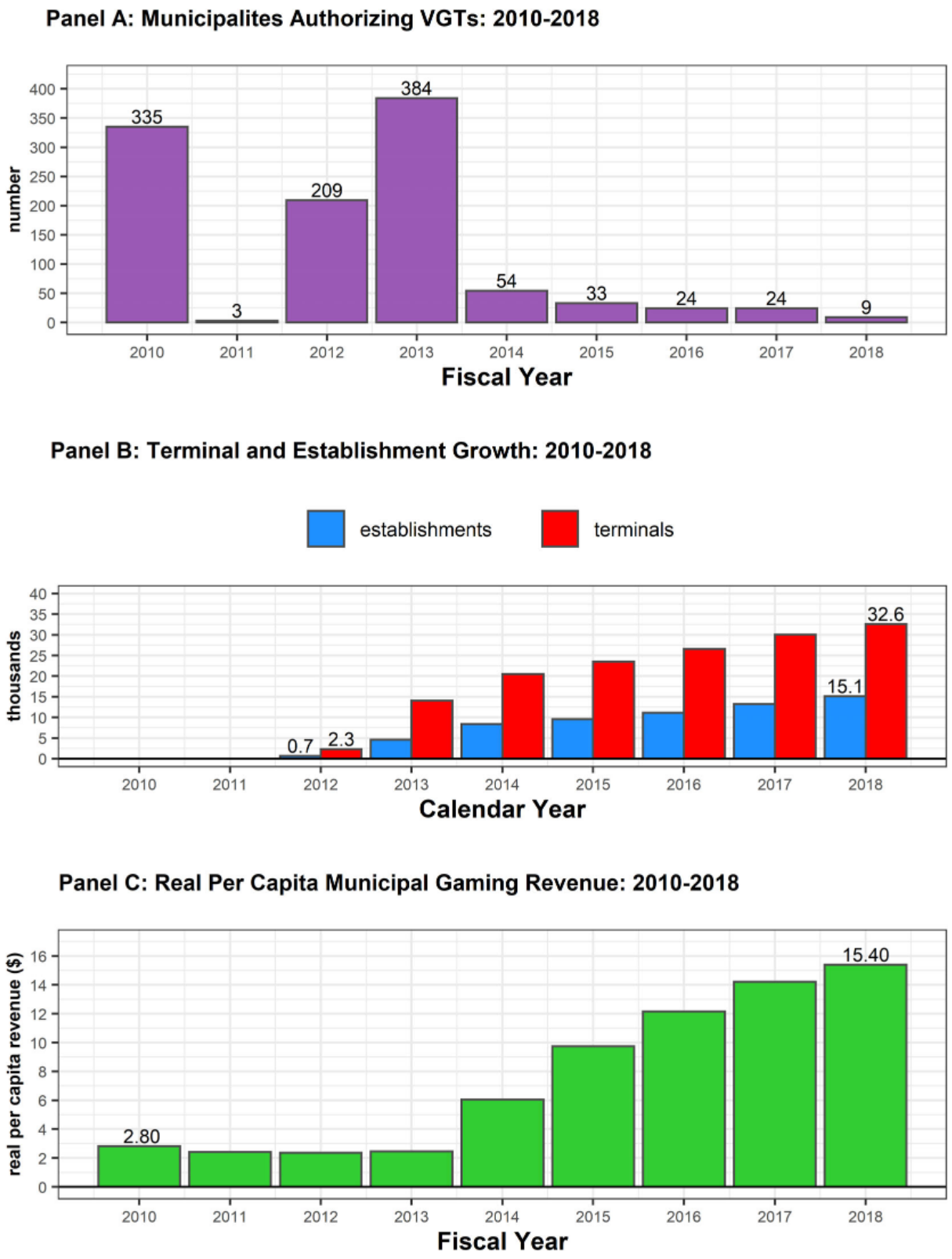

FIGURE 1 Municipal authorizations, terminal expansion, and gaming revenue. Authors' calculations using data from the Illinois Gaming Board and Illinois Comptroller's Office [Color figure can be viewed at wileyonlinelibrary.com]

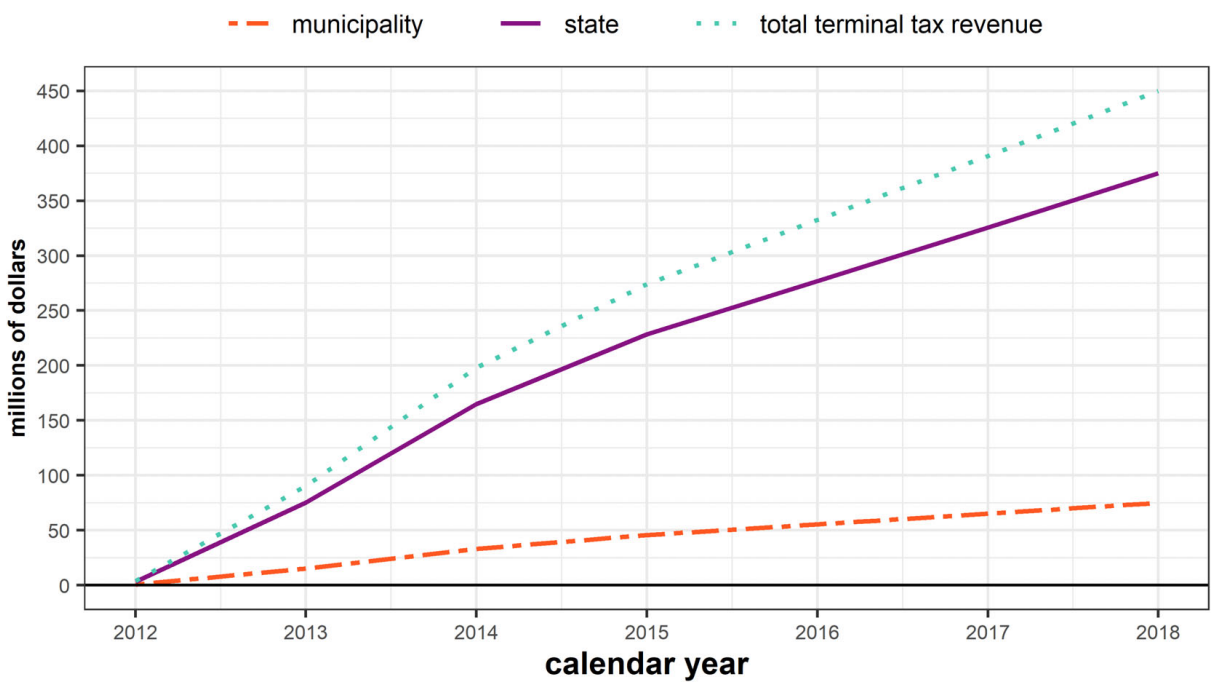

FIGURE 2 Aggregate terminal tax revenue: 2012–2018. Authors' calculations using data from the Illinois Gaming Board [Color figure can be viewed at wileyonlinelibrary.com]

municipalities have received nearly $400 million. As for the remainder of NTI, the system operator received 0.85%, and terminal owners and establishment owners equally split the rest (almost 70%).

3.2 | General identification strategy

Since we have a sample of municipalities that have authorized VGTs and a sample of municipalities that have prohibited them, one approach to estimating the causal effect of video gaming expansion on municipal revenues is to pursue a difference-in-differences strategy. With panel data, this model can be expressed as:

$$f_{i,c,t} = \alpha + \gamma_i + \lambda_t + \delta post_{i,c,} + \phi homerule_{i,c,t} + \omega\left(othergaming_{i,c,t} * T\right) + \theta X_{c,t} + \varepsilon_{i,c,t} \quad (1)$$

where $f_{i,c,t}$ is the fiscal outcome variable of interest in municipality i in county c at time t, γ_i are municipality fixed effects, λ_t are fixed year effects, and $post_{i,c,t}$ is an indicator variable that equals unity for municipalities that have authorized VGTs (the treated units) in the post-treatment periods. $homerule_{i,c,t}$ is an indicator variable that equals unity if municipality i in county c has home rule status at t, and equals zero otherwise. This will adjust for the different levels of fiscal autonomy granted to home rule communities.[16]

[16]According to the Illinois Municipal League, there are 217 municipalities in the state with home rule status. The municipalities with home rule status in our sample include 88 that have authorized VGTs and 20 that have not for some or all of our sample period.

Three municipalities have authorized VGTs and also host riverboat casinos; they are Peoria in Tazwell County, Metropolis in Massac County, and Alton in Madison County. Crete and Stickney Villages, located in Will and Cook Counties respectively, authorized VGTs and also host racetracks. All of these casinos and racetracks began operating before 1994 so any time-invariant differences that might affect fiscal outcomes will be absorbed by municipal fixed effects. To adjust for time-varying differences associated with these other gaming segments, we interact an "other gaming" indicator variable (=1 if a riverboat casino or racetrack is present in municipality i in county c at time t) with a linear trend term (T).

Annual time-varying municipal factors that could affect fiscal outcomes are unavailable before the five-year American Community Survey began in 2009.[17] As an alternative, we include county-level covariates ($X_{c,t}$) that cover our entire sample period (1994–2018) to adjust for differences in fiscal outcomes between the treated and comparison municipalities. These variables include real median household income, unemployment rate, business establishment growth rate, and real home price growth rate. We also include the 10-year growth rate in real per capita personal income to capture longer-term changes in the county's economic trajectory.

We focus our primary empirical analysis on six fiscal outcomes: total revenues, service charges, local option sales tax revenues, property tax revenues, property tax rates, and the assessed value of all real taxable property in the municipality. Property tax revenues, service charges, and property tax rates are the primary areas where municipalities may have some discretion to make policy changes. Since most state aid is distributed on a per capita basis, municipalities have limited ability to adjust this important source of funding.

As noted in Section 3.1, municipalities receive 5% of the NTI generated within their jurisdiction. If the VGTs lead to net new economic activity, perhaps by attracting out-of-area patrons, then VGT expansion may increase total revenues directly via the NTI channel. If these patrons purchase other local taxable goods and services, then local option sales taxes may increase as well. If VGTs boost economic activity, service fees (such as for parking) might increase as might property values, leading to an increase in property tax revenues. (Examining property tax rates and assessed values separately should reveal the mechanism driving any observed changes in property tax revenue.)

On the other hand, if the expansion of VGTs does not attract new patrons from outside the municipality, then the VGTs may simply displace other economic activity. For example, consumers might spend money on VGTs rather than on other taxable retail items, leaving municipal revenues unaffected. Evidence of such local displacement or cannibalization could most likely be found in the reduction of local option sales tax collections. A large majority of municipalities in Illinois (900+) are authorized to collect local option sales taxes. The local option rates vary but can increase the base state rate by as much as 4.75 percentage points. The median local option sales tax rate is 2.5%.[18]

Of the 1210 municipalities whose VGT status is verified, we found a usable sample of 800. Two hundred and ninety-five municipalities on the IGB list either have only partial data or are missing completely from the Illinois Comptroller Office's local fiscal database. We also adjust for differences in the size of municipalities by transforming all revenue variables into per capita terms using the 2000 Decennial Census estimates.[19] One hundred and two municipalities on

[17]The ACS one-year samples begin in 2005 but only cover areas with populations of 65,000 or more. The three-year ACS samples, which begin in 2007, cover populations for areas of 20,000 or more but were discontinued in 2013.

[18]Details on local option sales taxes are available at the Illinois Department of Revenue. URL: https://www2.illinois.gov/rev/questionsandanswers/Pages/140.aspx.

[19]We use the 2000 Decennial Census for population rather than the 2009 5-year American Community Survey estimate because the Census year estimate is likely to have less error.

the IGB list do not have a population estimate from the 2000 Census. Of the 813 remaining municipalities with complete data, 741 have authorized VGTs. However, nine of these authorized VGTs in FY 2018 or later and, for lack of observable posttreatment fiscal outcomes, must be excluded from the difference-in-differences analysis. Finally, we omit four municipalities from the comparison group because they had some form of permissible gaming before the VGT expansion.[20] This leaves us with a sample of 732 treated municipalities and a comparison group of 68 municipalities that have never authorized any form of gaming.

Source information and descriptive statistics for our variables are reported for the treated and comparison groups in Table 2. The treated and comparison municipalities are fairly similar across many fiscal margins, such as (per capita) total revenues, local option sales tax revenue, and property tax rates. Comparison group municipalities have larger average populations than treated municipalities (10,376 vs. 4722). They also have much larger tax bases in terms of real property, with a real per capita assessed value of $680 versus only $53 for municipalities that have authorized VGTs.

The City of Chicago, which is the largest and most notable municipality that has not permitted video gaming (to date), is omitted from the comparison group because the Illinois Comptroller Office's fiscal data for it was incomplete.

Our sample includes at least one municipality that has authorized VGTs from every county in the state. The counties with the most authorizing municipalities tend to be in the state's largest metropolitan statistical areas (MSAs). For instance, the top four counties include Cook County in the Chicago MSA (34 authorizing municipalities), Champaign County in the Champaign-Urbana MSA (20 authorizing municipalities), St. Clair County in the St. Louis MSA (19 authorizing municipalities), and Sangamon County in the Springfield MSA (18 authorizing municipalities). Of the eight counties that have only one municipality authorizing VGTs, five are located in the southeast region of the state near the Kentucky and Indiana borders.[21]

Our comparison group municipalities are fewer in number and less geographically diversified than the authorizing municipalities. They also tend to be clustered in the Chicago and St. Louis metro areas, the northeast and southwest corners of the state. Twenty-four of the comparison group municipalities are located in Lake and Cook counties, both in the Chicago metro area. Despite this concentration, the average authorizing municipality is just 21 miles from a comparison group municipality. Eighty percent of authorizing municipalities are located 44 miles or less from a comparison municipality, within an hour's drive under most circumstances.

3.3 | Parallel trends and staggered adoption

The key identifying assumption in a difference-in-differences strategy, which is inherently untestable, is that the fiscal outcomes in the comparison group municipalities are a valid counterfactual (potential outcome) for how outcomes would have evolved in treatment municipalities had they never authorized VGTs. To provide evidence of the parallel trends assumption holding in the pretreatment period, we follow Muralidharan and Prakash (2017) and explicitly

[20]The municipalities that reported nonzero gaming revenue before 2009 but have not authorized VGTs (to date) are Forest Park Village, Lisle Village, Maryville Village, and Kell Village.

[21]The eight counties (in our sample) with a single municipality authorizing VGTs are Edwards, Wabash, Boone, Alexander, Brown, Pope, Schuyler, and Hamilton.

TABLE 2 Descriptive statistics for treated and comparison municipalities

(1)	(2)	(3)	(4)	(5)	(6)
Variable	Geography	Treatment mean	Treatment *SD*	Comparison mean	Comparison *SD*
Total revenue	Municipality	1249.95	5490.36	1384.10	1051.78
State share (*100)	Municipality	35.11	16.91	30.02	17.64
Local option sales tax	Municipality	26.89	105.90	30.75	81.42
Property tax	Municipality	212.00	1465.41	274.80	353.18
Property tax rate	Municipality	0.93	0.89	0.86	0.68
Assessed value	Municipality	53.03	459.63	680.19	5113.82
Population	Municipality	4722.03	9426.21	10376.93	14982.42
Home rule (1 = yes)	Municipality	0.10	0.30	0.22	0.41
Other gaming (1 = yes)	Municipality	0.01	0.08	0.00	0.00
Business establishment growth	County	0.21	2.69	0.54	2.95
Home price growth	County	2.16	3.60	2.22	4.23
Median household income	County	58.84	12.44	65.15	19.44
10-year growth in personal income	County	12.81	7.94	13.10	8.52
Unemployment rate	County	6.14	2.12	6.36	2.17

Note: The column labeled "Geography" denotes whether the variable is at the municipality or county level. Total revenue, local option sales tax revenue, property tax revenue, and assessed value are in real per capita terms. All real variables were deflated using the CPI (2018 = 100). State share is the fraction of total revenue the municipalities receive from the state (in various forms of aid). Population figures are from the 2000 decennial census and are time-invariant. The property tax rate and assessed value are from 2000 to 2018. All other variables are from 1994 to 2018. All raw municipality finance data were obtained from the Illinois Comptroller's Office local government division. The fiscal year data files are available at: https://illinoiscomptroller.gov/financial-data/local-government-division/financial-databases/. Our full sample includes 732 treated and 68 comparison group municipalities. Business establishment growth is the annual percentage change (* 100) in establishments based on the Quarterly Census of Earnings and Wages data from the Bureau of Labor Statistics. The unemployment rate (* 100) is from the Bureau of Labor Statistics. Median household income, from the Census Bureau's Small Area Income and Poverty Estimates, is in thousands of real dollars. Per capita personal income data, used to construct the 10-year growth rate in real per capita personal income, are from the Bureau of Economic Analysis. Real home price growth (* 100) was constructed from the Federal Housing and Finance Association's developmental index for counties using the base year of 2000 (https://www.fhfa.gov/DataTools/Downloads/Documents/HPI/HPI_AT_BDL_county.xlsx).

test for differential trends between the treatment and comparison municipalities *before* VGT adoption. We do this by estimating a generalized version of Equation (1) that can be expressed as:

$$f_{i,c,t} = \alpha + \gamma_i + \lambda_t + \delta post_{i,c,} + \phi homerule_{i,c,t} + \omega\left(othergaming_{i,c,t}{}^{*}T\right) + \theta X_{c,t} + \psi\left(pretreat_{i,c,t}{}^{*}T\right) + \gamma\left(pretreat_{i,c,t}{}^{*}T^{2}\right) + \varepsilon_{i,c,t} \quad (2)$$

where $pretreat_{i,c,t}$ equals unity in the pretreatment period for treated municipality i in county c at time t and T is a linear trend. The term $(pretreat_{i,c,t} * T)$ allows the fiscal outcome potentially

to differ for the treatment group in the pretreatment period up to a linear trend. Since a trend difference could also be nonlinear, adding the term ($pretreat_{i,c,t} * T^2$) allows the fiscal outcome for the treatment group potentially to differ in the pretreatment period up to a quadratic trend.

Consider a hypothetical situation in which the comparison group is a perfect counterfactual for the treatment group, implying that the parallel trends assumption holds perfectly in the pretreatment period. In this situation, the estimated coefficients on the differential trend terms in Equation (2), ψ and ε, would equal zero because the pretreatment trends are identical (after conditioning on the covariates). In this case, Equation (2) simply reduces to Equation (1).

We estimate two versions of Equation (2) for every fiscal outcome for guidance on the parallel trends assumption; one that includes only a differential linear trend term (setting $\varepsilon = 0$) and one that includes the linear and quadratic trend terms. We then conduct the appropriate hypothesis test (single t test or joint Wald test) for the significance of the differential trend terms.

If one rejects the null hypothesis that the differential trend terms equal zero, then the parallel trends assumption is likely violated because Equations (1) and (2) differ in a meaningful way in the pretreatment period. Conversely, failing to reject the null hypothesis that the differential trend terms equal zero provides evidence in support of the parallel trends assumption holding in the pretreatment period (conditional on the covariates). It is worth noting that we also conduct an event study formulation for our key results to provide additional evidence of the parallel trends assumption holding in the pretreatment period. This is discussed in greater detail in Section 4.1.

The final identification issue is addressing the staggered timing of VGT adoption. As Goodman-Bacon (2021) shows, conventional two-way fixed effects (TWFE) estimators can be biased in the presence of staggered adoption because units treated earlier in the sample end up serving as part of the comparison group for units treated later in the sample. If there are any treatment dynamics or heterogeneity across treatment groups, which could occur if early adopters experience a larger/smaller effect, then TWFE will be biased and will not recover the average treatment effect on the treated (ATT).

Baker et al. (2021) conduct a series of simulations and show that TWFE with variable treatment timing is biased if the treatment effect is dynamic (changing over time) or heterogeneous across treated units. They also show that TWFE can be unbiased with staggered adoption, but this requires the treatment effect to be constant both over time and across all treated units.

We address staggered adoption using the difference-in-differences approach proposed by Cengiz et al. (2019). Baker et al. (2021) refer to the approach as a "stacked regression" design. This strategy is simple to implement and, as Baker et al. (2021) show through simulations, yields an unbiased estimate of the average treatment effect in the presence of treatment effect dynamics and/or heterogeneity.

The idea underlying the stacked regression approach is construction of a comparison group for each treatment group cohort so that treated units cannot later contaminate the comparison group. Each cohort's data set is also re-centered, or converted to relative time, so that the treatment date is the same for all cohorts. In our application, we have 732 treated municipalities with authorizations occurring every year from 2010 to 2017. Our comparison group is 68 municipalities or "never adopters" that have never authorized any form of gaming.

Consider the cohort of municipalities, g, that adopted VGTs in 2010. This cohort's data set consists of all treated units adopting VGTs in 2010 and all 68 "never adopters" over the period

1994–2018. For this cohort, there are 16 pretreatment periods (1994–2009) and nine posttreatment periods (2010–2018). For the cohorts of municipalities adopting VGTs in later years, one simply creates a new cohort-specific data set re-centered such that pretreatment observations are aligned by event time.

As an illustration, consider the cohort of municipalities that adopt VGTs in 2013. This cohort's data set consists of all treated units (adopting in 2013) and all "never adopters" using the sample period 1997–2018. This is 16 pretreatment observations and six posttreatment observations. The sample period is different from the cohort adopting in 2010 in order to align the treatment date with the data sets from the other cohorts. Every cohort g has 16 pretreatment periods because this is the maximum number we can use given available data.

If the treatment date for any treated cohort g is re-defined as period 0, then cohort g's data set uses treated *and* comparison observations from period -16 up to a maximum of eight. The number of posttreatment periods for each cohort depends on the timing of adoption. We observe nine posttreatment periods for municipalities that adopted in 2010 (2010 to 2018), which in relative time reflects periods 0, 1, 2, 3, 4, 5, 6, 7, 8. For municipalities that authorized VGTs in 2017, we only observe two posttreatment periods (0 and 1 in relative time).

The final data set is formed by stacking (row concatenating) all of the individual cohort data sets. This structure does two things. First, it strictly ensures that no treated units are ever part of a comparison group. Second, it guarantees that every treated cohort is treated in the same relative time period (period 0). The one important caveat to the stacked design is the need to fully saturate the municipality- and year-fixed effects for each specific cohort data set (Cengiz et al., 2019).

4 | EMPIRICAL RESULTS

4.1 | Baseline regression results

Our baseline results of the "stacked regression" difference-in-differences estimates are presented in Table 3. Each model was estimated with cohort-specific year fixed effects, cohort-specific municipality fixed effects, and a constant term, all of them omitted in favor of brevity. *SE*, shown in parentheses below the coefficient estimates, are multi-way clustered following Cameron et al. (2011) at the municipality and county dimension.

The rows labeled "p-value (differential pretreatment linear trend)" and "p-value (differential pretreatment quadratic trend)" show the p-values from the t test or Wald test that allow for differential trend fiscal outcomes for the treatment municipalities in the pretreatment period. Across all fiscal margins, the p-values easily fall outside of the range of conventional significance ($p > .10$). This indicates that, after conditioning on the covariates and fixed effects, fiscal outcomes were on a similar trajectory in the treatment and comparison municipalities before VGTs were authorized. This further suggests that the comparison group is a valid counterfactual for the potential outcome of the treated municipalities.

Across most fiscal margins, we find no evidence that authorizing or operating VGTs has led to a change in municipal finances relative to the comparison municipalities. The one dimension where we do find an effect is in local option sales taxes (column [3] in Table 3). Relative to the comparison group, we find that real per capita local option sales taxes *decrease* by $8.25 following the authorization of VGTs. This result is significant at the 1% level.

TABLE 3 Baseline difference-in-differences results

	Dependent variable					
	Total revenue	**Service charges**	**Local option sales taxes**	**Property taxes**	**Property tax rates**	**Assessed value**
	(1)	(2)	(3)	(4)	(5)	(6)
Post	32.338	4.736	−8.245***	30.079	−0.049	−68.137
	(103.809)	(52.875)	(2.486)	(24.902)	(0.059)	(85.286)
Home rule	225.386***	−8.296	11.225	39.346**	−0.030	−38.217
	(67.283)	(60.643)	(12.580)	(17.582)	(0.031)	(44.806)
Median household income	11.667	9.447	−0.504***	1.878	−0.008*	9.664
	(12.859)	(6.794)	(0.183)	(1.724)	(0.004)	(10.409)
Unemployment rate	42.137	10.798	−2.924**	5.107	−0.003	2.566
	(37.652)	(17.556)	(1.282)	(4.247)	(0.016)	(13.172)
10-year growth in personal income	42.656	−49.881	−4.265	−107.770	0.185	569.502
	(488.612)	(184.336)	(9.362)	(80.599)	(0.118)	(571.355)
Business establishment growth	7.498	−1.727	−0.472	−0.251	−0.007	7.068
	(5.074)	(2.795)	(0.405)	(1.222)	(0.006)	(8.992)
Home price growth	2.494	1.693	−0.867***	−1.594	0.004*	5.632
	(5.934)	(2.569)	(0.194)	(1.075)	(0.002)	(5.235)
Other gaming*T	4.366	−0.209	1.677**	2.852	0.031*	0.030
	(22.252)	(15.043)	(0.845)	(3.374)	(0.017)	(1.249)
N	28,412	28,412	28,412	28,412	24,142	24,142
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	.780	.624	.464	.938	.856	.969
p-value (differential pretreatment linear trend)	.932	.822	.285	.874	.306	.337
p-value (differential pretreatment quadratic trend)	.655	.708	.631	.579	.579	.406

Note: This table presents the effect of authorizing video gaming terminals on municipal fiscal outcomes. "Post" denotes the difference-in-differences parameter of interest. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.3 for complete details. *SE* are multi-way clustered following Cameron et al. (2011) at the municipality and county dimensions and reported in parentheses. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level. All models include a constant term, municipality fixed effects, and year fixed effects that are not reported. The row "*p*-value (differential pretreatment linear trend)" reports the *p*-value for the coefficient from a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a linear trend in the pretreatment period. Similarly, the row "*p*-value (differential pretreatment quadratic trend)" reports the *p*-value from a Wald test in a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a quadratic trend in the pretreatment period.

Since total revenues do not change the following authorization, the decline in local option sales taxes is evidence that spending at VGTs is displacing other taxable retail spending within the jurisdiction. Consider an extreme scenario where all VGT patrons come from outside of the state and no in-state residents patronize the terminals. In this case, local option sales taxes would increase for municipalities that authorize VGTs (relative to the comparison group) if out-of-state patrons also purchase other taxable goods and services within the municipality. If these patrons do not spend anything on non-gaming taxable goods or services, the treatment effect for local option sales tax revenue should be zero relative to the comparison group. Consider the other extreme case where VGT patrons come from only within the authorizing jurisdiction and all spending at video terminals displaces spending on other taxable goods and services. In this scenario, one would expect to observe local option sales taxes declining relative to those in the comparison group.

The average municipality received roughly $27 in real per capita local option sales taxes between 1994 and 2009, accounting for between 1% and 3% of revenue. The point estimate in column (3) suggests that local option sales tax revenue declined by nearly 30% after the authorization of VGTs. Since we find no evidence that total municipal revenues increase, our results suggest that VGTs result only in distributional effects within authorizing municipalities. That is, the net increase in revenue experienced by establishment owners and VGT operators comes from the reduction in spending on other taxable goods and services in the jurisdiction. Without more granular spending data on taxable goods and services, we are unable to identify the specific business lines bearing the brunt of the reallocation of economic activity.

To further explore the parallel trends assumption and any treatment effect dynamics for local option sales tax revenue, we also estimated an event study formulation for this fiscal outcome. We follow standard practices and exclude the time period just before treatment from the set of relative time indicators (period -1). The coefficients for the relative time indicators, shown in Figure 3, therefore capture the mean difference in fiscal outcomes relative to period -1.

The event study formulation for local option sales taxes further confirms that the parallel trends assumption holds in the pretreatment period. There are two large point estimate differences between the treatment and comparison groups in relative time periods -16 and -14, but the imprecisely measured estimates make it impossible to reject the null hypothesis that they equal zero. Excluding those two pretreatment periods, the point estimates between treated and comparison municipalities are generally quite close.

Figure 3 clearly shows a growing difference in local option sales tax revenues between treated and comparison municipalities post-VGT authorization. In the last three posttreatment periods, relative time 6, 7, and 8, all of the point estimates suggest that local option sales tax revenue for municipalities authorizing VGTs drops by more than $10 per capita relative to municipalities that have never authorized gaming.

4.2 | Robustness checks with alternative samples and comparison groups

The baseline results in Table 3 compare all treated municipalities to the set of municipalities that have never authorized any form of gaming ("never adopters"). This section explores the robustness of the local option sales tax revenue result to alternative samples and comparison

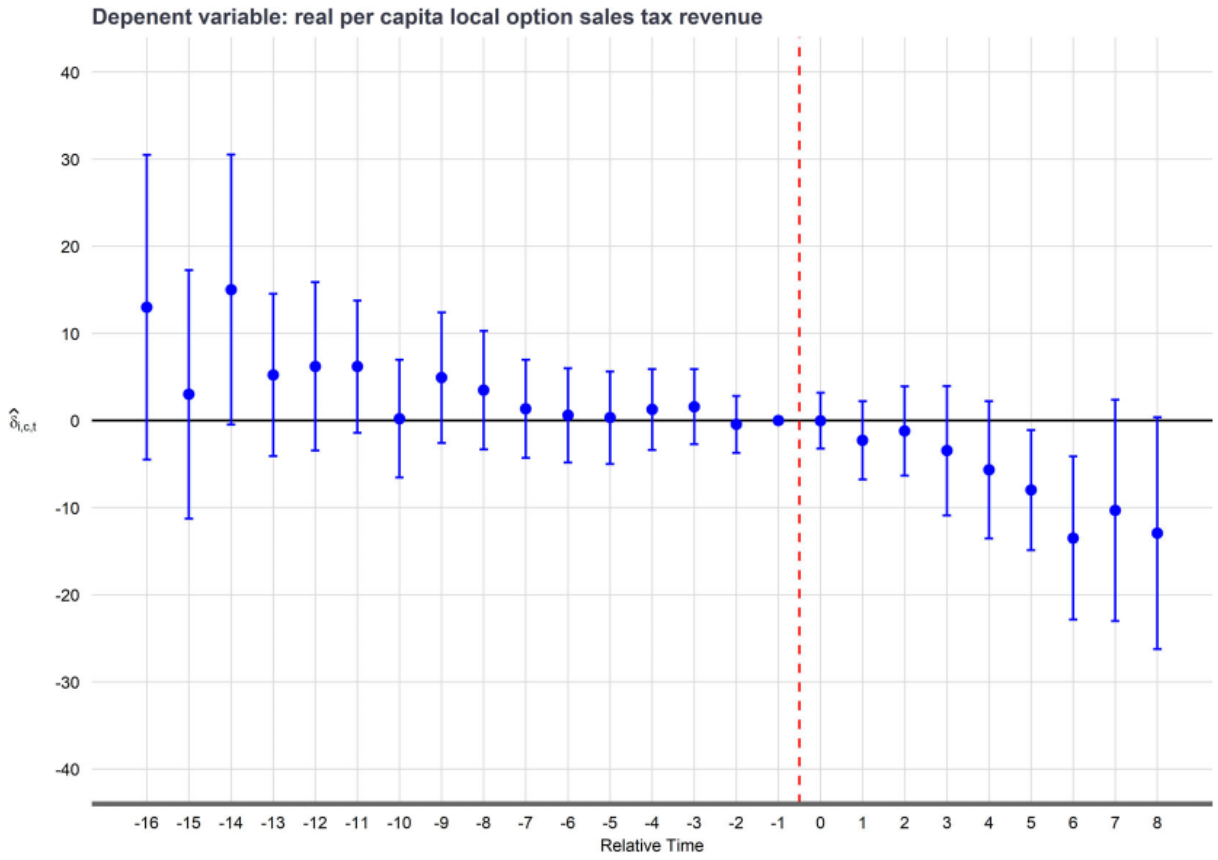

FIGURE 3 Event study formulation for local option sales tax revenue. This figure presents the effect of municipalities authorizing video gaming terminals on real per capita local option sales tax revenue. The markers plot the event study coefficients in which the outcome variable is regressed on relative time indicator variables and the covariates and fixed effects from Table 3. Error bars show the 95% confidence intervals. Year 0 is the treatment period and year −1 was the omitted relative time indicator [Color figure can be viewed at wileyonlinelibrary.com]

groups.[22] Given that the concentration of treated and comparison municipalities is higher in the Chicago and St. Louis MSAs, we truncate our sample to only those municipalities that are within those two MSAs. This includes a total of 207 treated municipalities and 37 comparison group municipalities (located in 17 different counties).

Since we have staggered treatment timing, it is possible to expand our comparison group by adding municipalities that have yet to authorize VGTs (the "not-yet-treated"). For instance, suppose municipality A authorized VGTs in 2010 (the first possible year) and municipality B authorized VGTs in 2014. If municipality B did not alter its fiscal behavior in advance of authorizing VGTs, then, in principle, it is a potential comparison unit for municipality A, at least through 2013. Once municipality B authorizes VGTs in 2014 and becomes treated, it is no longer a valid comparison unit.

Generalizing on this framework, let g denote the cohort of treated municipalities that authorize VGTs at time t. Any "not-yet-treated" municipality that authorizes VGTs in period $t+1$ or

[22]We also applied the same robustness checks presented in this section to the other five fiscal outcomes shown in Table 3. We find no evidence of a statistically significant difference between the treatment and comparison groups in any specification. These additional results will be provided upon request.

later is a potential comparison unit for cohort *g* up to period *K*-1, where *K* is the year the not-yet-treated become treated. Given that our first and last VGT authorizations occur in 2010 and 2017, we can form six additional comparison groups by adding all of the potential "not-yet-treated" municipality cohorts to the baseline comparison group of 68 "never adopters."

The largest possible comparison group, which we refer to as NYT1, includes all 68 "never adopters" and any "not-yet-treated" municipality that authorizes VGTs 1 year or more *later* than cohort *g*. A second comparison group, NYT2, includes all 68 "never adopters" and any "not-yet-treated" municipality that authorizes VGTs 2 years or more after cohort *g*. The smallest additional comparison group that we can construct based on our data, NYT6, includes all 68 "never adopters" and any "not-yet-treated" municipality that authorizes VGTs 6 years or more after cohort *g*.

Table 4 reports the robustness checks of the baseline results for local option sales tax revenue using the full sample of municipalities and all seven comparison groups. These estimates follow the same stacked regression approach detailed in Section 4.1.

Column (0) in Table 4 uses the "never adopters" as the comparison group so it is identical to column (3) in Table 3. Columns (1), (2), ..., (6) reflect comparison groups formed from the "never adopters" plus different groups of "not-yet-treated" municipalities. Column (1), which is comparison group NYT1, adds all municipalities that authorize VGTs 1 year or more later than cohort *g* to the comparison group. At the other extreme, column (6), which is comparison group NYT6, adds all municipalities that authorized VGTs 6 years or more later than cohort *g* to the comparison group.

As is evident from Table 4, our key finding that local option sales tax revenue falls after VGT authorization is robust to expansion of comparison units to include the "not-yet-treated" municipalities. Across all specifications, we find that real per capita local option sales taxes fall between $6.58 and $8.54, which is a reduction ranging from 24% to 31% and a median reduction of 28%. These seven comparison groups are all very different both in composition and location. The fact that the estimated treatment effects are so stable across them gives us greater confidence that we are identifying a true effect.

We report the results of our estimates using only the Chicago and St. Louis area municipalities in Table 5. The column layout matches the order in Table 4. Column (0) uses only "never adopters" as the comparison group, and columns (1) through (6) add different groups of "not-yet-treated" municipalities to the "never adopters" to expand the comparison group.

When the sample is restricted to the greater Chicago and St. Louis metro areas, we find robust evidence consistent with our full sample results that local option sales tax revenues fall after VGT authorization. Looking across all specifications, we find that real per capita local option sales taxes fall between $11.62 and $12.43. This apparently larger magnitude than that found in results based on the full sample is probably due to higher local option sales tax revenue overall in these municipalities. Based on the pretreatment mean value of $36 per capita for local option sales taxes, the results in Table 5 imply that municipalities in the Chicago and St. Louis regions have experienced between a 32% and 34% reduction in local option sales tax revenue after VGT authorization. These point estimates are very much in line with our full sample results.

As a final robustness check, we exclude from our sample any treated municipality for which the year of authorization was an estimate made by the IGB. Because all 217 of these municipalities authorized VGTs before the Gaming Board began verifying adoption, we were advised to use 2010 as the adoption date. Table 6 below excludes these municipalities from the treatment group. Column (0) uses only "never adopters" as the comparison group, and columns (1) through (6) add different groups of "not-yet-treated" municipalities to the "never adopters" to expand the comparison group.

TABLE 4 Full sample with alternative comparison groups

Dependent variable: local option sales tax							
	(0)	(1)	(2)	(3)	(4)	(5)	(6)
Post	−8.245***	−6.588***	−7.038***	−7.576***	−7.512***	−8.541***	−8.221***
	(2.486)	(1.869)	(1.974)	(2.152)	(2.332)	(2.328)	(2.390)
Home rule	11.225	7.429	8.406	10.400	11.020	10.962	11.187
	(12.580)	(10.733)	(10.720)	(11.536)	(11.768)	(12.103)	(12.330)
Median household income	−0.504***	−0.454**	−0.541***	−0.509***	−0.505***	−0.530***	−0.511***
	(0.183)	(0.196)	(0.189)	(0.189)	(0.186)	(0.177)	(0.179)
Unemployment rate	−2.924**	−2.062*	−2.284*	−2.942**	−3.094**	−2.901**	−2.918**
	(1.282)	(1.132)	(1.163)	(1.251)	(1.285)	(1.287)	(1.284)
10-year growth in personal income	−4.265	−6.169	−1.875	−2.988	−3.701	−2.496	−4.017
	(9.362)	(11.690)	(10.514)	(9.441)	(9.421)	(9.274)	(9.216)
Business establishment growth	−0.472	−0.287	−0.365	−0.613	−0.638	−0.409	−0.440
	(0.405)	(0.398)	(0.423)	(0.456)	(0.462)	(0.394)	(0.402)
Home price growth	−0.867***	−0.904***	−0.922***	−0.916***	−0.919***	−0.905***	−0.868***
	(0.194)	(0.266)	(0.232)	(0.192)	(0.192)	(0.193)	(0.193)
Other gaming*T	1.677**	1.106	1.260	1.574*	1.662*	1.600*	1.655*
	(0.845)	(0.976)	(0.918)	(0.829)	(0.842)	(0.832)	(0.840)
N	28,412	48,574	39,358	32,480	30,660	29,505	28,757
Comparison group	Never	NYT1	NYT2	NYT3	NYT4	NYT5	NYT6
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	.464	.422	.422	.419	.417	.468	.464
p-value (differential pretreatment linear trend)	.285	.252	.217	.289	.391	.162	.244

TABLE 4 (Continued)

Dependent variable: local option sales tax							
p-value (differential pretreatment quadratic trend)	.631	.113	.048	.142	.356	.129	.380

Note: This table presents the effect of authorizing video gaming terminals on local option sales tax revenue. "Post" denotes the difference-in-differences parameter of interest. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.3 for complete details. Column (0) compares all 732 treated municipalities to the 68 municipalities who have never authorized gaming. Columns (1) through (6) sequentially add "not-yet-treated" municipalities to the "never adopted" comparison group; see Section 4.2 for details. *SE* are multi-way clustered following Cameron et al. (2011) at the municipality and county dimensions and reported in parentheses. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level. All models include a constant term, municipality fixed effects, and year fixed effects that are not reported. The row "*p*-value (differential pretreatment linear trend)" reports the *p*-value for the coefficient from a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a linear trend in the pretreatment period. Similarly, the row "*p*-value (differential pretreatment quadratic trend)" reports the *p*-value from a Wald test in a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a quadratic trend in the pretreatment period.

TABLE 5 Chicago and St. Louis metro area counties with alternative comparison groups

	Dependent variable: local option sales tax						
	(0)	(1)	(2)	(3)	(4)	(5)	(6)
Post	−12.437**	−11.620***	−11.888***	−12.000***	−12.045**	−12.415**	−12.263**
	(4.575)	(3.532)	(3.929)	(3.938)	(4.377)	(4.460)	(4.367)
Home rule	5.280	−0.869	1.006	4.006	4.646	4.954	5.178
	(15.833)	(15.300)	(14.770)	(15.332)	(15.445)	(15.732)	(15.771)
Median household income	0.208	0.207	0.073	0.039	0.121	0.166	0.209
	(0.600)	(0.426)	(0.428)	(0.507)	(0.545)	(0.575)	(0.574)
Unemployment rate	−4.112	−3.248	−3.932	−4.534	−4.199	−4.087	−3.916
	(3.732)	(3.794)	(3.686)	(3.656)	(3.820)	(3.807)	(3.703)
10-year growth in personal income	−27.870	−41.354	−25.112	−13.105	−21.741	−26.181	−29.236
	(33.129)	(36.394)	(34.001)	(32.002)	(34.255)	(33.714)	(33.257)
Business establishment growth	−1.898	−1.709	−1.961	−1.888	−2.036	−1.950	−1.842
	(1.516)	(1.347)	(1.357)	(1.324)	(1.416)	(1.451)	(1.493)
Home price growth	−1.151***	−1.719***	−1.632***	−1.440***	−1.315***	−1.197***	−1.133***
	(0.285)	(0.364)	(0.302)	(0.244)	(0.238)	(0.226)	(0.273)
Other gaming*T	1.904**	1.390*	1.452*	1.491*	1.714*	1.766*	1.883**
	(0.882)	(0.675)	(0.720)	(0.809)	(0.843)	(0.876)	(0.870)
N	10,972	16,807	14,017	12,174	11,554	11,239	11,041
Comparison group	Never	NYT1	NYT2	NYT3	NYT4	NYT5	NYT6
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	.459	.446	.458	.472	.470	.465	.459

TABLE 5 (Continued)

Dependent variable: local option sales tax							
p-value (differential pretreatment linear trend)	.649	.534	.529	.525	.548	.528	.640
p-value (differential pretreatment quadratic trend)	.891	.418	.383	.481	.579	.583	.929

Note: This table presents the effect of authorizing video gaming terminals on local option sales tax revenue. The sample includes only treated and comparison municipalities located in one of the 17 counties that make up the Chicago and St. Louis MSAs. "Post" denotes the difference-in-differences parameter of interest. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.3 for complete details. Column (0) compares all 732 treated municipalities to the 68 municipalities who have never authorized gaming. Columns (1) through (6) sequentially add "not-yet-treated" municipalities to the "never adopted" comparison group; see Section 4.2 for details. *SE* are multi-way clustered following Cameron et al. (2011) at the municipality and county dimensions and reported in parentheses. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level. All models include a constant term, municipality fixed effects, and year fixed effects that are not reported. The row "*p*-value (differential pretreatment linear trend)" reports the *p*-value for the coefficient from a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a linear trend in the pretreatment period. Similarly, the row "*p*-value (differential pretreatment quadratic trend)" reports the *p*-value from a Wald test in a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a quadratic trend in the pretreatment period.

TABLE 6 Only known adoption dates with alternative comparison groups

Dependent variable: local option sales tax							
	(0)	(1)	(2)	(3)	(4)	(5)	(6)
Post	−7.140**	−5.926**	−6.094**	−6.270**	−6.578**	−7.074**	−7.164**
	(2.831)	(2.515)	(2.557)	(2.630)	(2.707)	(2.796)	(2.828)
Home rule	24.813*	16.889	19.225*	22.606*	23.593*	23.878*	24.484*
	(13.279)	(10.591)	(10.910)	(12.124)	(12.405)	(12.768)	(13.025)
Median household income	−0.596**	−0.510**	−0.601**	−0.560*	−0.554*	−0.591**	−0.583**
	(0.279)	(0.239)	(0.255)	(0.286)	(0.287)	(0.271)	(0.273)
Unemployment rate	−4.723***	−2.796**	−3.283***	−4.275***	−4.650***	−4.381***	−4.533***
	(1.415)	(1.184)	(1.210)	(1.329)	(1.388)	(1.372)	(1.384)
10-year growth in personal income	−15.608	−13.338	−10.223	−12.717	−14.511	−12.400	−14.543
	(12.529)	(13.125)	(12.353)	(12.384)	(12.527)	(12.399)	(12.530)
Business establishment growth	−0.679	−0.357	−0.465	−0.752	−0.806*	−0.559	−0.610
	(0.435)	(0.385)	(0.413)	(0.466)	(0.481)	(0.408)	(0.422)
Home price growth	−0.698***	−0.814***	−0.813***	−0.784***	−0.766***	−0.769***	−0.718***
	(0.111)	(0.241)	(0.186)	(0.139)	(0.127)	(0.119)	(0.111)
Other gaming*T	−1.405***	−1.522***	−1.622***	−1.421***	−1.414***	−1.430***	−1.420***
	(0.253)	(0.274)	(0.277)	(0.249)	(0.255)	(0.253)	(0.253)
N	24,261	45,011	35,723	28,769	26,869	25,630	24,794
Comparison group	Never	NYT1	NYT2	NYT3	NYT4	NYT5	NYT6
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	.501	.429	.431	.433	.430	.505	.500

TABLE 6 (Continued)

Dependent variable: local option sales tax							
p-value (differential pretreatment linear trend)	.572	.354	.505	.788	.725	.601	.597
p-value (differential pretreatment quadratic trend)	.903	.075	.179	.722	.783	.671	.870

Note: This table presents the effect of authorizing video gaming terminals on local option sales tax revenue. The sample excludes 217 treated municipalities where the Illinois Gaming Board estimated the date of adoption. "Post" denotes the difference-in-differences parameter of interest. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.3 for complete details. Column (0) compares all 732 treated municipalities to the 68 municipalities who have never authorized gaming. Columns (1) through (6) sequentially add "not-yet-treated" municipalities to the "never adopted" comparison group; see Section 4.2 for details. *SE* are multi-way clustered following Cameron et al. (2011) at the municipality and county dimensions and reported in parentheses. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level. All models include a constant term, municipality fixed effects, and year fixed effects that are not reported. The row "*p*-value (differential pretreatment linear trend)" reports the *p*-value for the coefficient from a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a linear trend in the pretreatment period. Similarly, the row "*p*-value (differential pretreatment quadratic trend)" reports the *p*-value from a Wald test in a separate regression that allows the fiscal outcome between the treated and comparison municipalities to vary by a quadratic trend in the pretreatment period.

Excluding the municipalities with estimated adoption dates has little effect on our results. Across all comparison groups, we continue to find robust evidence that local option sales tax revenue falls after VGT authorization.

4.3 | A closer look at local sales tax displacement

Although we find evidence that on average VGTs displace local spending on taxable goods and services, it is useful to investigate if this finding is being driven either by municipalities with very high or very low levels of video gaming activity. We do this by re-estimating the stacked difference-in-differences local sales tax regression with selected interaction terms to isolate the marginal treatment effect for high- and low-gaming municipalities. We use the full sample of municipalities and use the 68 "never adopters" as the comparison group. Using mean gaming revenues from 2010 to 2018, we create indicator variables for municipalities that are in the bottom 2%, bottom 5%, bottom 10%, top 10%, top 5%, and top 2% of the gaming revenue distribution.[23] The results of these regressions are reported in Table 7.

Based on columns (1), (2), and (3), we find no evidence that local option sales taxes fall more (or less) than the average in areas with low levels of gaming revenue. However, as columns (5) and (6) show, there is evidence that local option sales taxes fall significantly more (in real per capita terms) for municipalities with high levels of gaming activity. For municipalities that have averaged at least $40 per capita in gaming revenue since 2010 (the top 5%), local option sales taxes fall by nearly $23 per capita relative to the comparison group. Since local option sales taxes averaged $60 per capita in these municipalities prior to VGT expansion, local option sales tax revenue dropped by ∼40%.

Similarly, for municipalities in the top 2% of gaming revenue ($60 or more per capita), the estimates in column (6) show that local option sales taxes decreased by $55 per capita in the post-VGT era. Local option sales taxes averaged $116 per capita in these municipalities before expansion, so this points to a roughly 50% reduction in local option sales taxes. Tying this back to the discussion in Section 3.2 of the potential channels through which expanded VGTs could affect revenue, this is evidence that the machines have not attracted substantial tourism.

To ensure that our baseline results in Table 3 are not driven by treatment effect heterogeneity coming from municipalities at the high end of the gaming revenue distribution, we re-estimate the regressions for total revenue and local sales taxes excluding those municipalities. Columns (7) and (8) in Table 7 show the estimates excluding the top 5% of municipalities and columns (9) and (10) exclude the top 2%.[24] The estimated treatment effect for local option sales taxes drops from around $8 per capita to $6.61–$6.68 and remains statistically significant at the 1% level. As with Table 3, we find no evidence total municipal revenues change when the municipalities that generate the most gaming revenue are excluded. In fact, the difference-in-differences point estimates are negative for total revenue when areas with high gaming revenues are excluded (columns 7 and 9).

Therefore, while we do find evidence for treatment effect heterogeneity in the form of greater local retail displacement in municipalities with very high levels of gaming, this heterogeneity is not driving our baseline results. Whereas the typical municipality experiences a 30%

[23]The real per capita gaming revenue thresholds for the indicator variables are as follows: bottom 2%—$0.84, bottom 5%—$1.77, bottom 10%—$2.63, top 10%—$27.65, top 5%—$40.81, and top 2%—$60.10.

[24]There are 32 treated municipalities in the top 5% and 13 in the top 2%.

TABLE 7 Difference-in-differences interaction results

	Dependent variable									
	Local sales taxes	Local sales taxes	Local sales taxes	Local sales taxes	Local sales taxes	Local sales taxes	Total revenue	Local sales taxes	Total revenue	Local sales taxes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post	−8.308***	−8.562***	−8.624***	−8.272***	−7.361***	−7.353***	−40.098	−6.617***	−38.762	−6.680***
	(2.499)	(2.544)	(2.687)	(2.164)	(2.488)	(2.456)	(79.621)	(2.445)	(79.165)	(2.411)
Home rule	11.227	11.199	11.222	11.217	11.605	11.696	245.115***	20.334*	248.990***	19.633*
	(12.580)	(12.588)	(12.579)	(12.482)	(12.279)	(12.186)	(48.316)	(11.123)	(47.655)	(11.037)
Median household income	−0.504***	−0.507***	−0.504***	−0.505***	−0.501***	−0.507***	−0.663	−0.621**	−0.848	−0.612**
	(0.183)	(0.183)	(0.183)	(0.182)	(0.183)	(0.181)	(4.519)	(0.241)	(4.538)	(0.239)
Unemployment rate	−2.925**	−2.918**	−2.918**	−2.923**	−2.974**	−2.963**	20.544	−3.338***	20.114	−3.362***
	(1.282)	(1.282)	(1.282)	(1.274)	(1.293)	(1.284)	(28.955)	(1.171)	(28.659)	(1.151)
10-year growth in personal income	−4.236	−4.056	−4.129	−4.254	−4.408	−4.671	342.982	−8.801	337.453	−8.560
	(9.354)	(9.307)	(9.335)	(9.482)	(9.409)	(9.439)	(398.760)	(10.017)	(393.836)	(9.939)
Business establishment growth	−0.470	−0.467	−0.469	−0.472	−0.454	−0.463	9.208*	−0.465	9.200*	−0.490
	(0.406)	(0.407)	(0.405)	(0.407)	(0.402)	(0.398)	(4.970)	(0.382)	(4.872)	(0.377)
Home price growth	−0.867***	−0.868***	−0.867***	−0.867***	−0.870***	−0.870***	−2.757	−0.770***	−2.751	−0.750***
	(0.194)	(0.194)	(0.194)	(0.194)	(0.194)	(0.193)	(2.569)	(0.143)	(2.517)	(0.141)
Other gaming*T	1.680**	1.693**	1.693**	1.669*	2.336**	3.338**	23.740**	0.933**	24.112***	0.922**
	(0.845)	(0.848)	(0.847)	(0.938)	(1.020)	(1.335)	(9.053)	(0.457)	(9.043)	(0.456)

(Continues)

TABLE 7 (Continued)

	Dependent variable									
	Local sales taxes	Local sales taxes	Local sales taxes	Local sales taxes	Local sales taxes	Local sales taxes	Total revenue	Local sales taxes	Total revenue	Local sales taxes
Post*bottom2	2.767									
	(4.922)									
Post*bottom5		6.312								
		(4.205)								
Post*bottom10			3.642							
			(3.197)							
Post*top10				0.321						
				(13.138)						
Post*top5					−22.642**					
					(10.536)					
Post*top2						−54.954**				
						(21.600)				
N	28,412	28,412	28,412	28,412	28,412	28,412	27,666	27,666	28,101	28,101
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	.464	.464	.464	.464	.464	.465	.716	.485	.717	.483

Note: This table presents the effect of authorizing video gaming terminals on municipal fiscal outcomes. "Post" denotes the difference-in-differences parameter of interest. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.3 for complete details. Column (0) compares all 732 treated municipalities to the 68 municipalities who have never authorized gaming. Columns (1) through (6) sequentially add "not-yet-treated" municipalities to the "never adopted" comparison group; see Section 4.2 for details. *SE* are multi-way clustered following Cameron et al. (2011) at the municipality and county dimensions and reported in parentheses. *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level. All models include a constant term, municipality fixed effects, and year fixed effects that are not reported. Columns (7) and (8) exclude the 32 treated municipalities in the top 5% of gaming revenues over the period from 2010 to 2018. Columns (9) and (10) exclude the 13 municipalities in the top 2% of gaming revenues over the period from 2010 to 2018.

reduction in (annual) real per capita local option sales tax revenue, areas with high levels of gaming experience sales tax revenue reductions of as much as 50%. Recall that Senate Bill 690, passed in 2019, authorizes establishments to add one terminal and increases the tax rate on NTI. If this legislation leads to more terminals and more gaming activity, our estimates intimate that municipalities should anticipate more local retail displacement rather than additional revenues.

While the cannibalization effect we find is sizable, its significance to municipal budgets is mitigated by the fact that local option sales taxes are such a small component of overall revenue. As Table 1 noted, local sales tax revenues accounted for 3.1% of total revenue between 1994 and 2008 before falling to just above 1% between 2009 and 2018. From the point of view of policymakers and the public whose interests they protect, the more important effect of cannibalization lies in reallocation of economic activity. As a back-of-the-envelope exercise, assume a municipality with 4700 residents authorizes VGTs. Our baseline estimates imply that after authorization, annual local option sales tax revenues fall to about $19 per capita from a pre-authorization average of roughly $27. The direct loss in tax revenues is only $37,600 per year. However, assuming that the local option sales tax rate (on top of the state rate) is 2.5%, which is the midpoint in the state, this points to an annual reallocation of about $1.5 million in spending (=$37,600/0.025). This is a significant shift in economic activity for a local community and provides some of the strongest evidence to date about how VGTs may cannibalize other, non-gambling sectors of the local economy.

5 | CONCLUSION

Illinois, one of the most aggressive states in approving new forms of legalized gambling, also has some of the highest tax rates on casinos and gambling in the United States. In 2009, the state authorized VGTs in municipalities that approved them. The key incentive for local approval is, presumably, the 5% tax on NTI that goes to the hosting municipality. A new source of tax revenue would be expected to increase total local tax revenues and enable politicians to increase discretionary spending or lower other taxes, and more than 1000 Illinois municipalities have authorized VGTs since the 2009 legislation.

This study provides some of the first evidence of the real effects of VGTs on local governments. Utilizing tax data from the Illinois Comptroller's Office in a difference-in-differences framework of municipal revenues, we find that in Illinois at least, the authorization and operation of VGTs has not affected overall municipal revenues—a finding that suggests VGT tax revenues are coming at the expense of other revenue sources.

Our analysis further reveals that the only tax source significantly affected by VGTs, the local option sales tax, experienced an average 30% revenue decline after their authorization. Furthermore, those municipalities that rank in the top 5% of VGT revenues have seen a decline in local option sales tax revenue of upwards of 50%.

Lobbyists and other advocates of VGTs might argue that the hit to local sales tax revenue is trivial. However, our results also suggest that the average VGT municipality, based on a median local option sales tax rate of 2.5%, experiences an annual reallocation of consumer spending to VGTs from other taxable goods and services equal to $320 per capita. Even if this reallocation is the result of increased competition in the entertainment sector, the implications for the businesses that suffer a decline in sales are serious.

Given the acute fiscal distress occasioned by COVID-19 restrictions, policymakers nationwide may be considering authorization or expansion of VGTs. The analysis in this paper provides important evidence for them and for voters.

While a tax on a new gambling sector might be beneficial to state-level tax revenues (e.g., 25% of NTI goes to Illinois state government), and may seem attractive to politicians and voters at the municipal level ex ante, our analysis suggests that the net impacts may be much smaller than the nominal gambling tax rate implies, and the harm to competing local businesses may be even more significant.

Two additional concerns, beyond the scope of this paper, should be noted. First, the psychology of addiction literature has suggested that machine gambling, such as VGTs, may pose the greatest risk to vulnerable populations, in the sense of being most closely associated with gambling disorders. (e.g., Harris and Griffiths 2018). If fact, some researchers have referred to machine games as the "crack cocaine of gambling" (for different perspectives, see Dowling et al. 2005). Second, taxes on VGTs may be regressive. As Grumstrup and Nichols (2021) found in Illinois, lower-income communities tend to have more VGTs and more consumer spending on VGTs, which suggests that tax revenue from VGTs is regressive.

As with most government policies, VGT authorization creates both winners and losers. State government, VGT owners and businesses with VGTs all likely benefit from VGT revenues and associated taxes. To reiterate: Municipalities do not benefit, and other businesses are harmed because spending is diverted from retail goods. Neither do VGTs yield the kind of economic boost previous studies associate with casinos, which require a large capital investment and the creation of jobs. In the context of the local economy, VGTs are more like lottery terminals, which can be installed in existing businesses without significant capital investment and without job creation.

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