# How local is local development? Evidence from casinos*

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**Abstract**

One rationale for place-based policy is that local development produces positive productivity spillovers. We examine the employment spillovers from a large local development project: opening a casino. Comparing employment in neighborhoods that won a casino license to runner-up neighborhoods that narrowly lost, we find casinos create jobs in their immediate vicinity. However, we estimate net job losses overall when considering the broader neighborhood. Employment gains concentrate in the leisure and hospitality industry, suggesting spillovers are industry-specific or are driven by demand-side forces like trip-chaining. We develop theory to show that our estimates imply a rapid spatial decay of productivity spillovers.

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# 1 Introduction

Governments implement local development policies to increase local employment, raise tax revenue, and ‘revitalize’ struggling areas on equity grounds. These policies often take the form of marquee projects like the construction of sports arenas, casinos, presidential libraries, or commercial districts. Economic development policy accounts for a large share of government spending: state and local governments spend on average 40 percent of corporate tax revenue on incentives for firms to operate within their jurisdictions (Slattery and Zidar, 2020). These place-based policies are thought to have positive productivity spillovers, yielding benefits that exceed costs (Kline and Moretti, 2014; Neumark and Simpson, 2015). This paper asks: how spatially dispersed are the benefits of local development projects?

This question is of both practical and theoretical interest. To plan for local growth, policymakers ought to know whether local development projects indeed affect the neighborhoods in which they occur. Theoretically, economists posit that the mechanism by which development projects expand employment beyond their premises is through agglomeration spillovers. A long tradition in urban economics posits that agglomeration forces operate at local levels (Fujita and Ogawa, 1982; Lucas, 2001; Ahlfeldt et al., 2015), with productivity at one location spilling over onto nearby producers. However, there is a lack of direct empirical evidence regarding the spatial extent of these externalities. This paper aims to fill this gap by using quasi-experimental evidence.

We employ a difference-in-differences design using granular labor market data to measure the spatial spillovers created by a large development project: opening a casino. Using ‘runner-ups’ defined via the bidding process for casino licensure as counterfactual locations, we estimate an average treatment effect on the treated that attenuates sharply in space. We find that casinos create jobs at the casino site relative to counterfactual land use and that employment growth extends beyond their premises. However, while employment increases occur in blocks close to the casino, casino entry decreases employment at slightly more distant locations. In short, local development is hyper-local. A casino opening increases total employment in the casino’s block by a factor of three, relative to control blocks. Within a two-minute drive from the casino, employment increases by approximately 82%, but this effect declines to 42% at blocks 2-3 minutes away. In areas 6-8 minutes from casinos, employment decreases by around 20%. In total, there is a net decrease in employment within an

8-minute drive. On average, we estimate that for every job created at the casino, 0.7 jobs are created nearby, yet when we expand to the broader neighborhood we find net job losses, which are statistically indistinguishable from zero. Thus, the positive agglomeration effects associated with opening a casino are outweighed by the 'agglomeration shadow' it casts. In short, when the goal is to increase employment in the broader area, opening a casino is an ineffective local development strategy.

Casinos make a good case study of how urban development projects impact local labor markets. First, they are large: casino projects cost hundreds of millions of dollars and employ hundreds, if not thousands, of people. Second, casinos are highly regulated, allowing us to track the licensure process, which in some cases provides the locations of alternative sites strongly considered in the site-selection process. Finally, as deregulation continues, casinos have become a common development strategy. For example, Virginia recently legalized casino gambling, and major cities such as New York and Chicago have won approval for additional licenses. Our work speaks to the current policy debates on the costs and benefits of large urban casinos.

Casino location is not random, and casinos may select to operate in economically improving areas. We address the identification problem caused by endogenous site selection using 'runner-up' sites that competed for the license but narrowly did not place a winning bid to open. The number of casino licenses in a given city is typically capped at zero, one, or two casinos. When states expand the quota, the additional casino is often awarded via a public bidding process. These are high-stakes bids; for example, bids ranged from $1.3-2 billion for Chicago's recently awarded license. We use the location of the runner-up bidder to define a counterfactual site and neighborhood, mirroring the use of 'almost-winners' in the site selection process for manufacturing establishments in Greenstone et al. (2010), or census tracts that applied for but did not receive a federal Empowerment Zone designation in Busso et al. (2013). This allows us to compare labor market outcomes in census blocks near the actual and counterfactual sites before and after casino opening, and difference out trends common to both locations, which would otherwise confound the identification of treatment effects.

Comparable work estimates agglomeration effects either at the county level using quasi-experimental variation, or at spatially granular levels with selection-on-observables designs. For example, Greenstone et al. (2010) find that the entry of 'Million Dollar Plants' – large manufacturing establishments – has productivity spillovers that raise the productivity of establishments in the same county by 12%, while Adams (2016) studies the employment gains associated with plant openings in car manufacturing, and finds evidence of local input-output spillovers. With census tract data and a propensity-score matching design, Qian and Tan (2021) show that the effect of high-skilled firm entry on labor market outcomes and welfare attenuate in space, affecting residents within a twenty-minute drive time to the treated site; however, their design lacks a natural experiment that our setting provides and uses coarser spatial measures. Focusing on high-skill service sector establishments, Baum-Snow et al. (2024) use rich panel data on firm location and revenue alongside location fixed effects to estimate productivity spillovers that decay within 75 meters. We combine quasi-experimental variation and spatially disaggregated data to address how quickly agglomeration forces deteriorate over space following a large 'place-based' policy shock. While our design allows us to estimate employment effects within the city, we cannot speak to citywide changes.

Productivity spillovers are driven by the diffusion of tacit production knowledge (Arzaghi and Henderson, 2008; Balsmeier et al., 2023), or other agglomeration economies associated with co-location, such as pecuniary externalities from the spatial concentration of service sector establishments and chained shopping trips (Shoag and Veuger, 2018; Koster et al., 2019; Miyauchi et al., 2021; Leonardi and Moretti, 2022; Oh and Seo, 2022; Vitali, 2022). The positive employment spillovers we measure are concentrated within the leisure and hospitality sector, consistent with within-industry technological spillovers or trip-chaining. Past research has quantified agglomeration forces using structural models that measure the magnitude and spatial reach of spillovers using variation from historical shocks (Ahlfeldt et al., 2015; Dericks and Koster, 2021). Using a simple model of an urban labor market with productivity spillovers, we show that our estimates map onto a parameter used in these structural models that governs the rate at which agglomeration spillovers decay. Our quasi-experimental estimates are of comparable magnitude, consistent with prior evidence that points to highly localized agglomeration economies in urban environments (Ahlfeldt et al., 2015; Rosenthal and Strange, 2020).

The gambling and tourism literature has generally associated the presence of casinos with successful development policies. This literature on casinos focuses on county-level variation (Rephann et al., 1997; Evans and Topoleski, 2002; Grinols and Mustard, 2006; Wolfe et al., 2012; Humphreys and Marchand, 2013), and lacks a quasi-experimental design. Focusing on city-specific case studies, Scavette (2023) finds that Atlantic

City's Eastern seaboard monopoly on gambling from 1978-1992 drove substantial city-wide employment increases using synthetic controls. In contrast, we estimate null labor market effects using within-city variation in cities that are not a specialized gambling destination.

## 2 Empirical Setting

During the Progressive Era, U.S. states banned nearly all forms of gambling, including state lotteries. In the aftermath of the Great Depression, states slowly re-introduced parimutuel gambling at race tracks and jai alai frontons. Most states continued to prohibit casinos, except for Nevada, whose state legislature introduced a liberal regulatory scheme in 1931. This remained the status quo for 45 years. Then, starting with New Jersey in 1976, states liberalized casino gambling regulation. By 2021, commercial casino operators oversaw 466 casinos in 25 states, often with large Vegas-style resort casinos.[1]

State regulations influenced the locations and types of casinos that opened. Avoiding the stigma of Vegas' organized-crime-connected resort-style casinos, early deregulation legislated nostalgic 'kitsch' gambling venues. Along the Mississippi River, Illinois, Iowa, and Mississippi welcomed back riverboat casinos, while Colorado and South Dakota authorized small casinos in former 'Old West' mining towns. States with strong Tribe-operated casinos limited competition with commercial casinos, as in New York and Oklahoma. States with race tracks such as Florida, New Mexico, and West Virginia authorized video lottery terminals at the tracks, creating 'racinos.' We focus on casinos in states like Illinois, Maryland, and Pennsylvania which legislated large resort-style casinos on non-Tribal land in 'new' locations – not on top of operating race tracks nor the dock of a riverboat casino.

Our data selection process begins with the universe of operating U.S. casinos from the American Gaming Association. We subset this list by manually checking and removing casinos at racetracks ('racinos'), riverboats, or on Native land. Then, doing archival newspaper research, we identify casinos with a plausible counterfactual due to public bidding for a casino license. States deregulated casinos with varying degrees of competition for licenses and transparency in site selection. Some states distributed licenses through a

[1] Per the American Gambling Association https://www.americangaming.org/state-of-play/. With 515 Tribe-operated casinos in 29 states, the tribal-operated casino industry has grown over the same period spurred by the Indian Gaming Regulatory Act of 1988 (see Akee et al. (2015)). These casinos have less flexibility in site selection.

competitive bidding process. For example, Pennsylvania apportioned four licenses for resort-style casinos; two for Philadelphia and Pittsburgh and two for the rest of the state, with at least two operators bidding for each license. On the other hand, some states picked winners or winning locations in non-transparent ways (Martino and Eadington, 2012). For example, Louisiana's selection committee picked the exact location of the New Orleans casino before awarding an operating license.[2] Two states relied on voters to decide whether to license a casino. In Ohio, voters denied licenses to casinos throughout the state several times before finally approving four casino locations in 2009. We use one losing 'bid' in Cleveland from 2006 as a plausible counterfactual site.

For our main specification, we focus on large urban casinos with a relatively transparent alternative site and employment data at the census block level several years before and after opening. This leaves us with six sites: Allentown and Bethlehem, PA; Chicago, IL area (Des Plaines, IL); Washington, DC area (Oxon Hill, MD); Cleveland, OH; Philadelphia, PA; and Pittsburgh, PA. We find several additional casinos with similar site selection processes, but that are located in more remote areas or have limited time series data.

# 3 Data

**Casino opening data** We construct a dataset of U.S. casino openings between 2000 and 2020 using the administrative records of state regulators and news accounts. The dataset includes the date the project wins approval, the date the casino opens, and the locations of the casino and the runner-up sites in the approval process. There are often several years between the date a casino is approved and when it opens. Collecting project approval dates allows us to remove observations where anticipatory effects are not accounted for in the alternative site. We found 17 casinos with at least one plausible alternative site (i.e., sites with competing bids that lost), and restrict our attention on large urban casinos with sufficient labor market data. In Appendix Table A3, we discuss the site selection process in various states and the extent to which we have a credible alternative site.

**Employment data** Our labor force data comes from the Longitudinal Employer-Household Dynamics (LEHD), a partnered data effort between the U.S. Census Bureau and U.S. states, which creates local snapshots of eco-

[2] This was an awkward strategy as one developer won the lease for the land while another won the casino operating license.

nomic conditions by combining Unemployment Insurance records with the Quarterly Census of Employment and Wages data. We rely on their public-use product, the LEHD Origin-Destination Employment Statistics (LODES) which include annual census-block-level profiles of workers and residents. Census blocks partition the United States using visible features like streets and correspond roughly to city blocks within cities. The data covers most states for the years 2002 to 2019. This allows us to measure changes in employment at a granular level. LODES public-use data has limitations, like injected noise and imputation to protect workers' privacy (Graham et al., 2014). Noise attenuates our estimates, making our design conservative. Differences in imputation methods over years may create spurious level effects that our difference-in-difference design nets out.

**Business formation data** To measure business entry and exit, we use Infogroup's (now called Data Axle) historical business census. This data is available from 2000 to 2020 and provides establishment-level address and industry categorization. We aggregate this dataset to produce establishment counts and measures of entry and exit at the census block level.

# 4 Empirical Strategy

Our approach relies on competition for licenses and the site-selection process of casino industry experts. Changes in neighborhoods may be incidental to a casino opening, and we do not know what would have happened in a neighborhood if the casino had not opened. We can temper these concerns by comparing changes in casino entry neighborhoods with 'control' neighborhoods. The empirical challenge is picking credible control neighborhoods. The ideal quasi-experiment uses a control location that is not treated but could have been selected if not for some arbitrary and capricious facts that are orthogonal to neighborhood change. We argue that using 'runner-ups' – sites that narrowly lost a bid to open and their surrounding neighborhoods – satisfies this criterion.

An anecdote illustrates the empirical strategy. After a protracted legal battle with a struggling riverboat operator in the early 2000s, the Illinois Gaming Board opened bidding for the tenth casino license. Several large casino operators undertook a costly site-selection process, which included finding a local government host, paying a design firm to develop the concept, and building support among local constituents. Among

| | Rest of CBSA | Control | Treated | Adjusted differences | | | |
|---|---|---|---|---|---|---|---|
| | | | | Treat vs control | p-val | Sample vs CBSA | p-val |
| *Household characteristics* | | | | | | | |
| Share white | 0.68 | 0.60 | 0.60 | 0.02 | (0.87) | -0.17 | (0.01) |
| Share black | 0.23 | 0.28 | 0.28 | -0.01 | (0.89) | 0.12 | (0.06) |
| Share college degree | 0.33 | 0.26 | 0.24 | 0.02 | (0.55) | -0.06 | (0.01) |
| Share unemployed | 0.07 | 0.09 | 0.10 | -0.01 | (0.59) | 0.03 | (0.02) |
| Share below poverty line | 0.12 | 0.19 | 0.22 | -0.03 | (0.64) | 0.09 | (0.02) |
| Share above nat'l median hh inc. | 0.56 | 0.43 | 0.40 | 0.02 | (0.76) | -0.12 | (0.01) |
| Median household inc. (USD) | 50,553 | 38,814 | 35,261 | 2,795 | (0.66) | -9,852 | (0.01) |
| *Housing market characteristics* | | | | | | | |
| Median home value (1000s USD) | 146.99 | 104.95 | 98.05 | 2.01 | (0.91) | -26.92 | (0.01) |
| Median gross rent | 693.93 | 614.03 | 580.79 | 22.48 | (0.61) | -38.62 | (0.14) |
| Share housing that is occupied | 0.93 | 0.91 | 0.89 | 0.01 | (0.65) | -0.03 | (0.02) |
| Share of units rented | 0.34 | 0.45 | 0.45 | -0.00 | (0.99) | 0.13 | (0.01) |
| Share of units built before 1980 | 0.79 | 0.86 | 0.90 | -0.03 | (0.47) | 0.07 | (0.01) |
| Median housing construction year | 1971 | 1972 | 1972 | -0 | (0.47) | 0.36 | (0.01) |
| Share housing from last 10 years | 0.11 | 0.07 | 0.05 | 0.02 | (0.45) | -0.04 | (0.01) |

Table 1: Neighborhood balance test for tract-level characteristics in the 2000 ACS. 'Adjusted differences' controls for CBSA-level fixed effects. To compute p-values, we cluster standard errors at the neighborhood level.

several competing sites, proposals to open casinos in Des Plaines, Rosemont, and Waukegan became finalists. The Des Plaines proposal won the deal with a 3 to 1 vote of the Illinois Gaming Board.

We use the runner-up site(s) and surrounding neighborhood(s) as the control in a difference-in-differences design that compares outcomes in treated and control sites before and after the opening of a casino. Our design measures an average treatment on the treated effect, which in our context is the effect of casino entry at a focal site relative to the next best land use. For example, the runner-up site in Cleveland went on to become the Greater Cleveland Aquarium. We identify changes in outcomes at the casino site that are 'net' what is created at the aquarium. In nearby blocks, the spillover estimates recover differences in the local responses to the change in economic activity at the focal site. Our main outcome of interest is job counts, which we interpret as a proxy for overall economic activity. Thus, we attribute positive spillovers to increased economic activity at the casino site. Our estimator relies on a parallel trends assumption: i.e., that employment growth at the casino site and spillover locations would follow a similar trajectory to the runner-up site and comparable locations if not for the casino entry.

While the site selection is not 'as good as random,' the process provides highly vetted sites within the same labor market. Table 1 highlights this. We take tracts for CBSAs in our sample and assign them to treatment, control, or other. From the 2000 American Community Survey (which occurs before any openings in our sample), we compute unweighted means of tract-level characteristics. Tracts in treated and control samples are similar on observables, but compared to the rest of the CBSA, tracts in treated and control units have fewer people with an associate's degree or higher, higher unemployment rates, more households below the poverty line, fewer households above the national median household income, and a much lower median household income. Neighborhoods in treated and control samples have lower median home values and rents, higher vacancy rates, more renter-occupied units, and an older housing stock. Thus, our empirical design selects comparable neighborhoods that differ from the average tract in a CBSA. This is evidence of endogenous site selection. Casino operators select poorer neighborhoods with a less educated workforce and cheaper land. This is intuitive: casinos are a low-skill and land-intensive technology. Note, balance-in-covariates is not required for the empirical strategy, which relies on a parallel trends assumption.

**Definition of distance** We use drive-time as the main distance concept between blocks and the casino. This provides a comparable notion of distance across cities. In our sample, average speed varies considerably across cities. In Appendix Figure A1, we plot average speeds on routes for each city in our sample. Trips are almost twice as fast in Desplaines, IL, and Oxon Hill, MD ($\approx$50km/hr) as they are in Philadelphia ($<$30km/hr).

**Neighborhood definition** We define 'neighborhood' as the census blocks within an 8-minute drive to the site. We compute the distance between blocks based on drive-time (and alternatively, drive distance) between the block centroid and the geo-coordinates of the focal site using Google Maps.[3] We use this drive-time to limit spatial overlap between treated and control sites and maintain comparable sample sizes at each distance. Figure 1 illustrates blocks around each site in Philadelphia, from which we draw the sample.

We choose 8-minute neighborhoods to limit the overlap of census blocks across neighborhoods as alternative sites are often close to the casino. Overlap is an issue in Pittsburgh, Cleveland, and Oxon Hill (Washington, DC), primarily for blocks farther than an 8-minute drive from the casino. We assign blocks to the nearest site. This choice can impact our estimate in either direction; however, supposing that casino entry tends to

[3]To avoid misassigning spillover effects to mismeasured casino locations, we manually verified the census block to which each casino belongs. In only one location does a casino complex span multiple blocks (Bethlehem PA), and in that case, we count all blocks as containing the casino.

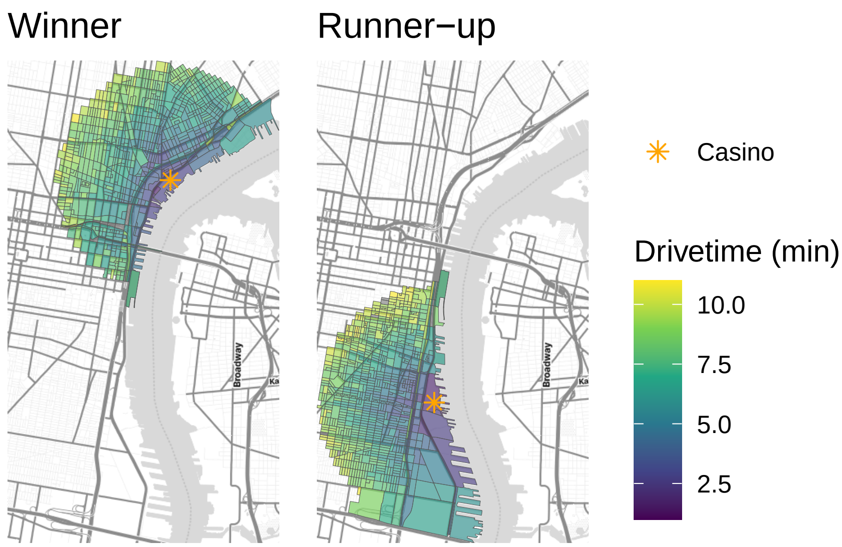


Figure 1: Winner and runner-up sites in Philadelphia, shaded by drive time to the casino. Casino sites are marked with a star.

have larger spillovers than alternative site usage, assigning overlap to the nearest site will put a downward bias on our estimates.

**Estimating spatial spillovers** We estimate the average difference in log-counts of jobs at blocks in the casino neighborhood versus alternative sites, in the years after casino opening relative to the years before the licenses are awarded. We disaggregate effects over space using 'rings', defining spillover regions by drive time in 1-minute bins, starting at a two-minute drive-time to the casino. For a block $b$ in year $t$ in city $c(b)$, we estimate,

$$\log \mathbb{E}[y_{bt}] = \sum_{\tau} \beta_\tau \text{ring}_\tau \times \text{opened}_t \times \text{treated}_b + \sum_{\tau} \phi_\tau \text{ring}_\tau \times \text{opened}_t + \xi_b + \xi_{c(b)t}, \tag{1}$$

where $y_{bt}$ are job counts in block group $b$ at time $t$. The index $\tau$ enumerates drive-time rings to the casino, with $\text{ring}_0$ representing the census block(s) in which the casino opens, $\text{ring}_2$ indicating blocks within a 2-minute drive time to the casino, $\text{ring}_3$ indicating blocks that are a 2-3 minute drive to the casino, and so on, so that every observation belongs in one ring. $\xi_b$ are fixed effects for the census block, while $\xi_{c(b)t}$ are fixed effects for city by time. These fixed effects are saturated for each casino-alternative pair so this is a 'stacked' design, which addresses recent issues in the two-way fixed effects literature (Baker et al., 2022).

The indicator variable $\text{treated}_b$ equals one when block $b$ is in a casino-license winning neighborhood and zero otherwise. Similarly, $\text{opened}_t$ equals one if the casino is open at time $t$. The coefficient $\beta_0$ measures the approximate percent change in employment at the casino compared to the alternative site(s). $\beta_\tau$ for $\tau \geq 2$ measures the spillover effects at distance $\tau$. We estimate equation (1) using a pseudo-Poisson maximum likelihood (PPML) estimator. PPML estimation provides consistent estimates for log-linear models in the presence of heteroscedastic standard errors and allows us to handle block-years with zeros in $y_{bt}$ without dropping rows (Silva and Tenreyro, 2006; Wooldridge, 2010; Correia et al., 2020). We allow for serial correlation in nearby units across time, and arbitrary spatial correlation within a year by two-way clustering our standard errors at the distance ring and neighborhood-year level.

**Sample restrictions** The license is awarded 3 to 4 years before casinos open. To avoid anticipation effects that occur after the license is awarded, we drop the years between license award and casino opening. We use data 3 years before license announcements and 3 full years after opening to maintain a balanced panel. This is because, in our sample, the DC area casino (Oxon Hill) opens relatively late.

# 5 Results

Casino entry increases employment at the casino site and nearby. However, the net effect is zero within an 8-minute drive as employment decreases at more distant locations. Employment effects are driven by changes in the 'Hospitality and Leisure' industry. We interpret this as evidence that casino entry creates an agglomeration of entertainment jobs in a hyper-local neighborhood around the casino by changing the spatial allocation of employment without affecting aggregate labor demand.

**Employment spillovers** Figure 2 shows estimates of treatment effects, $\beta_\tau$ in Equation (1). Casinos create an over 200% increase in employment at the treated site. Within a 2-minute drive, employment increases by approximately 82%.[4] For blocks within a 2-3 minute drive to the casino, employment increases by about 42%. Effects deteriorate and become modestly negative (about a 20% decline in employment) after a six-minute drive to the casino. At the casino site, job-growth effects are strongest in low-income jobs offering between \$15,000 and \$40,000 per year, while spillover growth is concentrated in higher-income jobs (annual income $\geq$\$40,000; see Appendix Figure A5).

[4] We estimate $\beta_2 = 0.6$, and $\exp(0.6) - 1 \approx 0.82$.

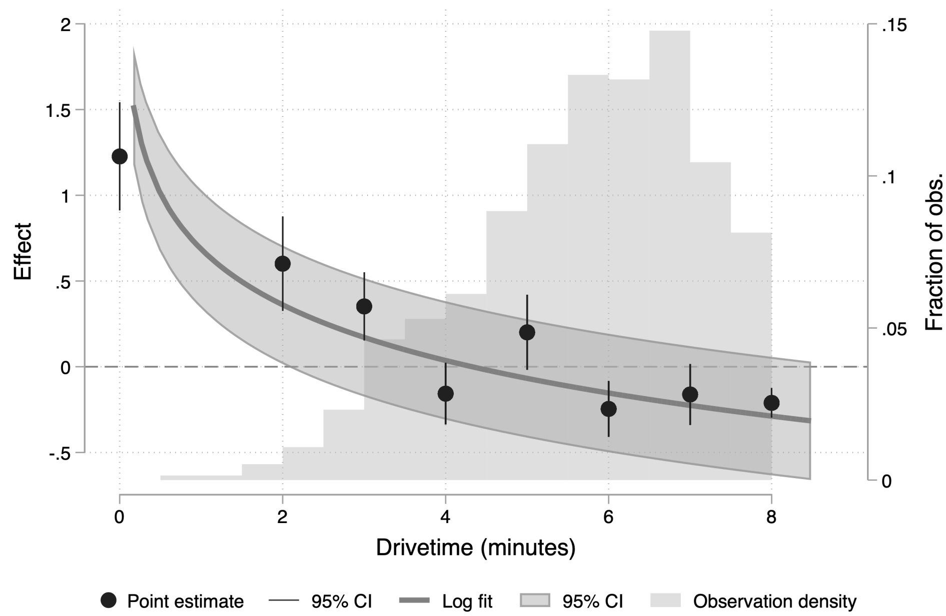


Figure 2: Estimates of $\beta_\tau$, from estimating Equation (1) versus time to the casino. Also pictured: the density of observations (census blocks), as a grey histogram, and an alternative specification in which we interact $\text{opened}_t \times \text{treated}_b$ with the log of time and drop casino sites. Appendix Figure A2 extends the sample to a ten-minute or greater drivetime, and Appendix Figure A3 uses drive distance. Point estimates are in Appendix Table A1

The observed negative effects could reflect the spatial distribution of employment shifting towards the casino: this is an 'agglomeration shadow' where it is profitable for establishments that provide intermediate goods or complementary goods to the casino to relocate closer to the casino to minimize transportation costs (see, e.g., Chapter 9 of Fujita, Krugman, et al., 2001). Other potential mechanisms behind the employment decrease include the casino bidding up land prices or increased output market competition thinning markups. Absent land or goods price data, we cannot disentangle the sources of this 'shadow.'

Appendix Figure A4 shows the positive spillover effects are present across industries, but are largest in the 'Hospitality and Leisure' industry. At the three rings closest to the casino, employment effects are the same order of magnitude or larger when restricted to this industry. In all other industries, we observe large employment growth at the casino site (64%), ring 2 (62%), and ring 3 (28%). Retail establishments co-locate near the casino which likely explains the site effects. Growth effects are closer to 0 for all larger drive times. The decline in employment in rings 6-8 corresponds to a decline in employment in non-entertainment and food service jobs, suggesting the presence of the casino crowded out employment in these industries. Overall, casino entry shifts the spatial pattern of employment within the neighborhood and changes its aggregate industrial composition.

**Effects on establishment entry and exit** Establishments may expand, relocate, or close in response to the casino entry. Using our block-level panel of establishment counts from Infogroup, we estimate Equation (1) on the total number of establishments in a block, as well as counts of exiting and entering establishments; see Appendix Figure A6. Using all establishments, net public services and unclassifiable establishments, we find a treatment effect that is *negative* near the casino, and attenuating to zero farther out. Zooming in on establishments classified as 'Hospitality and Leisure', we observe the same pattern, albeit noisier, with a positive effect on entry and the number of establishments at the block containing the casino itself. This suggests employment growth comes from expansion or the replacement of smaller firms with larger ones. Absent data linking jobs to establishments, we cannot measure how employment growth is distributed across firms.

**Jobs multiplier** Policymakers care about the number of additional jobs created above and beyond those that development projects create. A common statistic to measure this effect is the 'jobs multiplier' (Moretti, 2010), which measures the ratio of excess jobs created for each new job created by the project. To estimate this number, we run regressions of the form,

$$\begin{aligned} \text{total jobs}_{ct} &= \alpha \text{opened}_t \times \text{treated}_c + \xi_{ct} + \xi_n + u_{ct} \\ \text{total casino jobs}_{ct} &= \alpha_0 \text{opened}_t \times \text{treated}_c + \xi_{ct} + \xi_n + v_{ct} \end{aligned} \tag{2}$$

In this estimating equation, $\alpha$ measures the average number of jobs added after the casino opens in the entire neighborhood around the casino, relative to the alternative neighborhood. The term $\alpha_0$ measures the average number of jobs created at the casino site relative to the control site in the 'Hospitality and Leisure' industry.[5] The terms $\xi_{ct}$ and $\xi_n$ reflect city-year and neighborhood fixed effects. We estimate Equation (2) with OLS in a seemingly unrelated regression to allow for cross-equation correlation in $u_{ct}$ and $v_{ct}$. We cluster our standard errors at the neighborhood-time level. We focus on neighborhoods within 3 minutes or 8 minutes of the casino.

[5]Given our results on industry level spillovers, this suggests we will over estimate the number of jobs at the casino and attenuate estimates of the job multiplier towards 0.

The jobs multiplier is $\alpha/\alpha_0$. We report estimates of Equation (2) and the corresponding jobs multiplier for various neighborhood definitions in Appendix Table A2. When restricting the neighborhood definition to a 3-minute drive around the casino, we estimate a jobs multiplier of 0.69 (s.e. 0.21). Using an 8-minute drive to the casino, our multiplier estimate is $-1.37$ (s.e. 0.71).[6] Thus, for every job gained at the casino, over one is lost in the broader neighborhood, but we cannot rule out a jobs multiplier of zero. As spillover effects are hyper-local, our results show that the spatial definition of treatment affects the measured multiplier. While casinos create on average 1,200 jobs at the census block containing the casino, we estimate that around 400 jobs are lost in the broader neighborhood.

The possibility of net job loss is driven by the small negative effect on employment at larger distances from the casino. These small estimates apply to a larger set of blocks, as the number of blocks increases in distance to the casino. This is potential evidence of casinos crowding out employment opportunities more than one-for-one, despite creating positive local spillovers. However, as we cannot rule out a net impact of zero, we interpret our finding as casino entry reshuffling the spatial distribution of employment towards the casino without creating more employment opportunities in the broader neighborhood.

**Parallel trends** Our identification assumption is that parallel trends would hold in treatment and control units absent treatment. While this is a counterfactual, we can test whether parallel trends hold before casino entry. Failing to pass this test would lower our confidence that the parallel trends assumption holds. In Figure 3, we plot estimates of $\beta_\tau$ over time all locations.

We replace the indicator variable opened$_t$ with an indicator for each year $t$, and plot the resulting coefficients relative to effect estimates at $t = -4$, the latest period in the data uncontaminated by anticipatory effects. Time is normalized so that $t = 0$ reflects the year the casino opens. Periods from -3 to -1, denoted by the dashed grey lines, reflect the lag between casino approval and opening. In all three panels, treatment effects are noisily estimated zeros before the announcement of the casino. Anticipatory effects are visible at the casino site itself, where employment rises before opening. Following its opening, employment grows at and around the casino site.

**Randomization inference** Our setting has a limited number of clusters so one might question the validity

[6] Above we raised the concern that total casino jobs may include other 'Hospitality and Leisure' jobs. This attenuates the multiplier estimates towards 0. For example, suppose that $\alpha_0$ includes 200 jobs that are not at the casino. This implies we must add 200 to the estimates of $\alpha$. Then, the 3-minute drive multiplier would be 1.04 and the 8-minute multiplier -1.44.

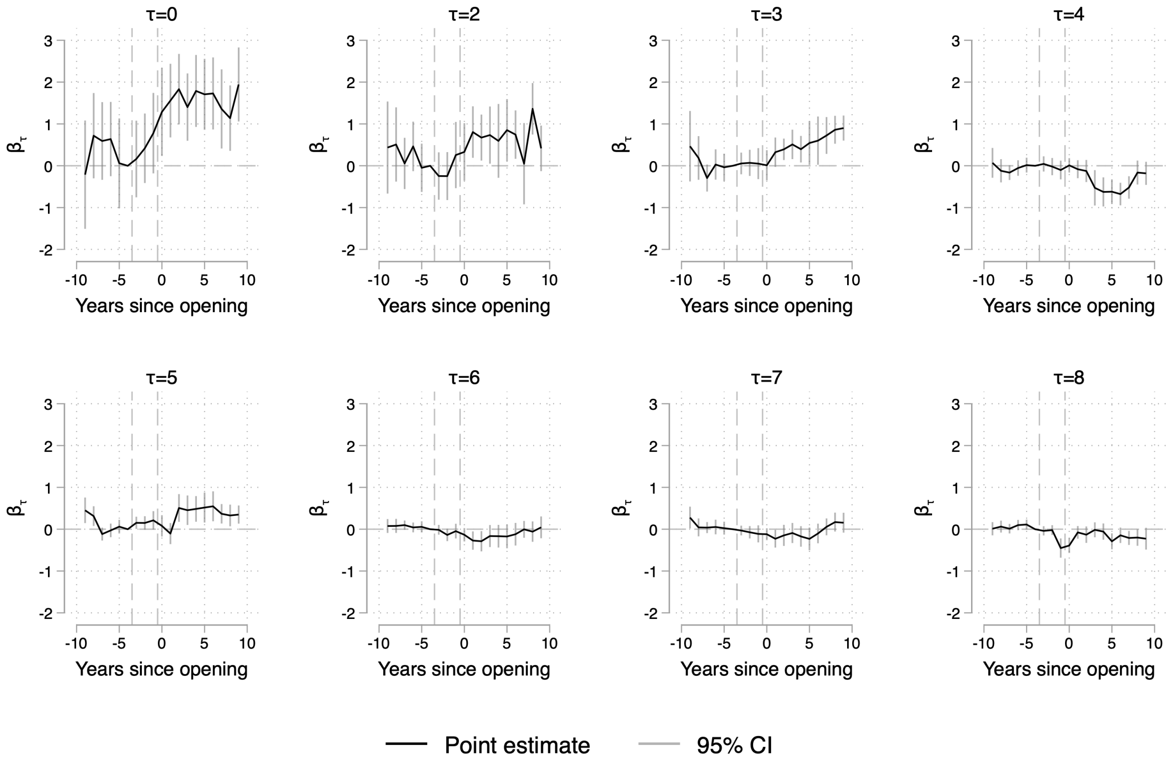


Figure 3: Estimates of $\beta_\tau$ in each year relative to casino opening, where $\tau$ indexes drive-time rings. Dashed lines reflect the widest anticipation period in the data. All six cities are only present in periods -7 through 3.

of cluster-robust t-statistics. To address these concerns, we report results from randomization inference in Appendix C, following MacKinnon and Webb (2020). Overall, these tests concord with our main results albeit with lower confidence.

**Model-based interpretation of local spillovers** We view casino entry as a site-specific productivity shock that may have effects on nearby locations through either urban externalities or the general-equilibrium reshuffling of firms and workers who desire to be closer to the casino to minimize transportation and commuting costs. We remain agnostic as to whether the spillovers are 'revenue productivity' spillovers due to pecuniary externalities (i.e., operating through prices), or are pure technological spillovers. Economists often model these urban externalities by asserting that a component of location-specific productivity depends on some distance-weighted average ('kernel') of nearby productivity (Fujita and Ogawa, 1982; Lucas, 2001; Ahlfeldt et al., 2015). In this section, we develop theory to show how we can use our quasi-experimental variation to estimate components of the productivity kernel.

Let locations be ordered by their distance to the casino, $\tau$, so that $\tau = 0$ is where we assume revenue productivity $A(0)$ has been shocked. Suppose employment at each location is determined by the intersection of local labor supply and local labor demand, and that location-specific productivity depends on a fixed component and a productivity kernel, $K$, so that,

$$\underbrace{L(\tau) = \frac{w(\tau)^{\eta}}{\int_0^{\bar{\tau}} w(s)^{\eta} ds}\bar{L}}_{\text{labor supply}}, \quad \underbrace{w(\tau) = A(\tau)L(\tau)^{-\theta}}_{\text{labor demand}}, \quad A(\tau) = \underbrace{\bar{A}(\tau)}_{\text{fixed}} \times \underbrace{\big(K(\tau, A)\big)^{\lambda}}_{\text{productivity kernel}}$$

where $L(\tau)$ is labor at location $\tau$, $w(\tau)$ is the wage and $A(\tau)$ is (revenue) productivity. The total neighborhood size is $\bar{\tau}$ and $\bar{L}$ is total labor supply to the neighborhood. The labor supply functional form can be microfounded by assuming, e.g., that households draw idiosyncratic workplace shocks from a Fréchet distribution over $[0, \bar{\tau}]$. $\eta$ is the labor supply elasticity, $\theta$ is the (inverse) labor demand elasticity, and $\lambda > 0$ governs the strength of spillovers. In equilibrium, a location-specific productivity shock shifts employment to the first order by,

$$d\log L(\tau) = \frac{\eta}{1+\theta\eta}\left[d\log A(\tau) - \int_0^{\bar{\tau}} \frac{L(s)}{\bar{L}} d\log A(s) ds\right] + d\log \bar{L}.$$

This equation expresses the fact that employment increases at $\tau$ depend on both the location-specific productivity shift relative to the neighborhood average, and the overall shift in labor supply to the neighborhood. Holding fixed $\bar{L}$, this expression captures the 'agglomeration shadow' effect: some locations may receive a positive productivity shock, but their relative productivity declines, resulting in employment decreases.

Under the assumption the kernel takes the following form,

$$K(\tau, A) = \exp\left(\int_0^{\tau} s^{-\delta} \log \bar{A}(s) ds\right),$$

then $d\log A(\tau) = \lambda\tau^{-\delta} d\log A(0)$ when only $\bar{A}(0)$ has been shocked. We seek to estimate, $\delta$, the elasticity of the kernel weights to distance, which informs the rate at which spillovers decay in space.[7] To take this theory to the data, we require a mapping of drive-time distance to the model-relevant notion of distance, $\tau$.

[7] We have picked this functional form for the kernel for its tractability, and use $\bar{A}(s)$ in its argument rather than $A(s)$. Under this assumption, we trace out the direct effect of a shock in one location on another, abstracting from 'reflection' (Manski, 1993) – that a productivity increase at one location is reflected back onto itself through the kernel. If endogenous effects are present, our estimate of $\delta$ will be biased upwards. Reflection magnifies the effect of local shocks, and we will confound endogenous amplification with a slow spatial decay. Thus, we view our estimate as an upper bound on the decay of productivity spillovers.

| | Point estimate | 95% CI |
|---|---|---|
| $\kappa\delta$ | 0.371 | $[0.194, 0.562]$ |
| $\int_0^{\bar{\tau}} \frac{L(s)}{\bar{L}} d\log A(s)ds$ | 0.225 | $[0.083, 0.427]$ |

Table 2: Estimate of the spatial decay parameter $\kappa\delta$ as well as the average change in productivity, estimated using nonlinear least squares. 95% confidence intervals are constructed using 600 bootstrap estimates using draws of $\beta_\tau/\beta_0$ from the estimated variance-covariance matrix of the point estimates of equation 1.

We follow the literature in assuming that $\tau = \exp(\kappa \text{drivetime}_\tau)$, so that distance is 1 when the drive-time is zero and decays exponentially at rate $\kappa$. Equation (1) estimates $d\log L(\tau)$ with $\beta_\tau$. Then, when there is no change in the neighborhood labor supply (which we cannot reject in the data) such that $d\log \bar{L} = 0$,

$$\frac{\beta_\tau}{\beta_0} = \frac{\exp(-\kappa\delta \cdot \text{drivetime}_\tau) - \int_0^{\bar{\tau}} \frac{L(s)}{\bar{L}} d\log A(s)ds}{1 - \int_0^{\bar{\tau}} \frac{L(s)}{\bar{L}} d\log A(s)ds}.$$

This equation describes how the productivity spillovers from a $\bar{A}(0)$ shock impact relative employment changes across space according to the kernel. We use this as an estimating equation, where the average productivity changes is a quantity to be estimated. We report estimates of $\kappa\delta$ and average productivity in Table 2 using the estimates of $\beta_\tau$ from Equation (1) and average ring drive-time. We find $\widehat{\kappa\delta} = 0.37$ and estimate average productivity as $0.23$, which implies that employment effects become negative beyond a 4-minute drive-time. The model visually fits the data well (see Appendix Figure A7). We bootstrap the estimation procedure using draws of $\beta_\tau/\beta_0$ to construct confidence intervals and reject zero for both parameters.

To put this number into perspective, this implies that doubling productivity at location $\tau = 0$ translates into about 15.7% growth in productivity at a drive time of 5 minutes away – a rapid decline of productivity spillovers in space. For comparison, Ahlfeldt et al. (2015)'s estimates of a similar parameter imply a productivity decrease of 16.4% at this same travel distance horizon.

# 6 Conclusion

How diffuse are the employment spillovers from a large development policy? Using information from competition for casino licensure coupled with spatially granular employment data, we estimate the labor market impacts of casino openings with 'almost-treated' counterfactual sites via a difference-in-differences strategy.

We find a positive employment multiplier that attenuates sharply in space: at the treated location, employment increases by a factor of nearly three. These employment gains spillover to sites within a three-minute drive-time, but cause employment declines farther away. Employment gains are concentrated in the same industry as the casino, suggesting that intra-industry spillovers, or demand-side forces like trip-training drive the results. While we do not observe new business entry, there is some evidence of changes in turnover rates, suggesting that employment growth is driven larger firms replacing smaller ones. Interpreting our results through the lens of a simple model of an urban labor market with productivity spillovers, we show that our estimates imply a rapid spatial decay of productivity spillovers.

For every three jobs created at the casino, a little over two are created nearby, but over four are lost in the overall neighborhood, defined by a eight-minute drive to the casino. We measure a negative effect on net employment in the casino's local neighborhood that is statistically indistinguishable from zero. This suggests that the positive agglomeration effects associated with opening a casino are outweighed by the 'agglomeration shadow' cast at sites further away. Therefore, as a jobs policy, incentivizing casino entry does not effectively promote local urban development despite creating hyper-local employment spillovers. These results are relevant both for urban policymakers seeking to understand the impact of casino opening – a high-stakes and increasingly popular urban development strategy – as well as economists interested in understanding the spatial decay of employment spillovers.

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# Online Appendix

## A Additional tables and figures

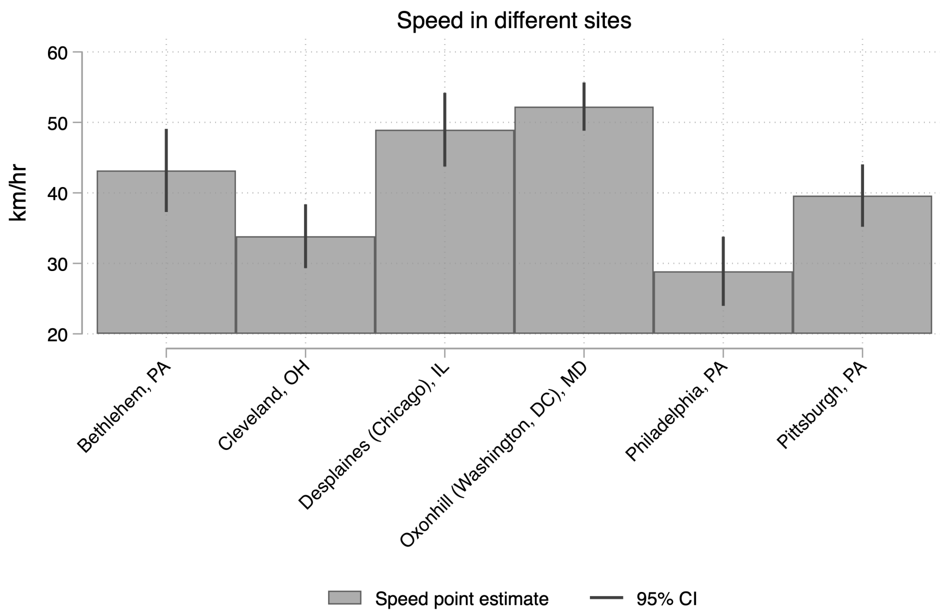


Figure A1: Average speed on routes in our sample, computed from a regression of distance (measured in kilometers traveled on the road network) on trip duration. Standard errors reflect clustering at the city level.

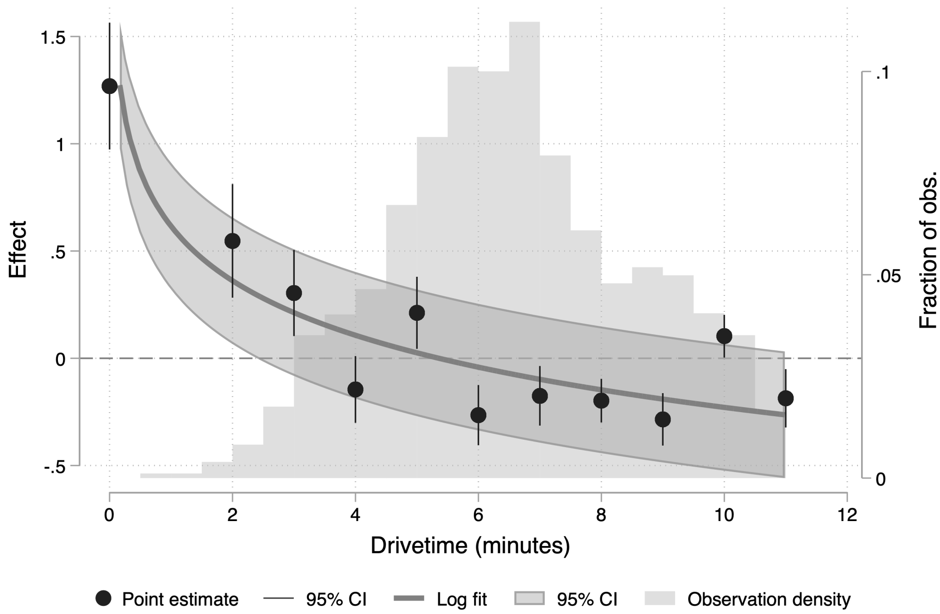


Figure A2: Robustness of results to neighborhood definition. Here we estimate equation 1 using the 900 nearest census blocks to the sites, treating overlap as described in the text. The histogram shows the distribution of blocks over rings. Sample counts start to decline after 8 minutes for three reasons: overlap of census blocks across sites, census blocks becoming larger outside of dense urban areas, and the restriction to 900-nearest blocks.

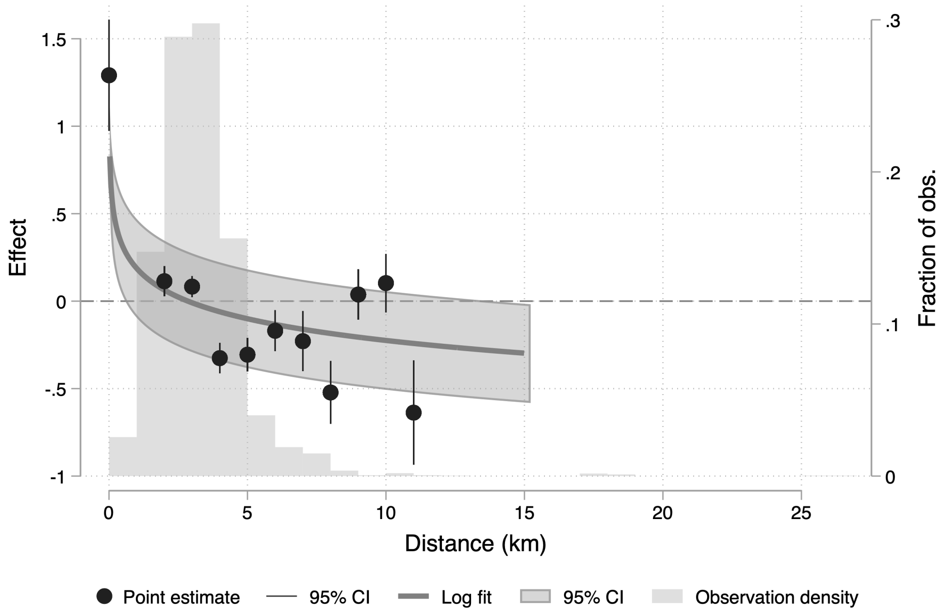


Figure A3: Main specification, using drive-distance bins, instead of drivetimes

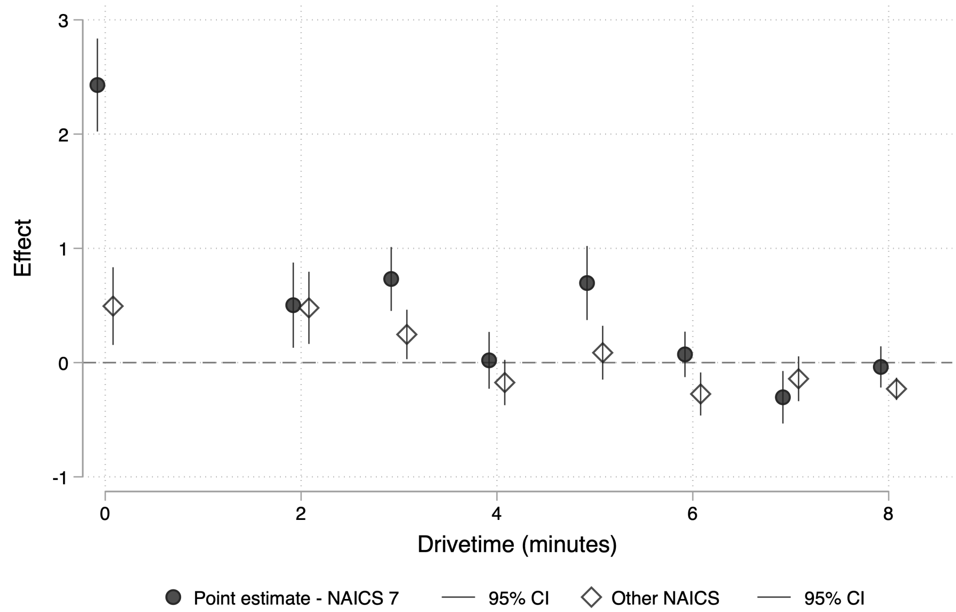


Figure A4: Estimates of $\beta_\tau$ from Equation (1), split for NAICS-7 jobs and all other jobs.

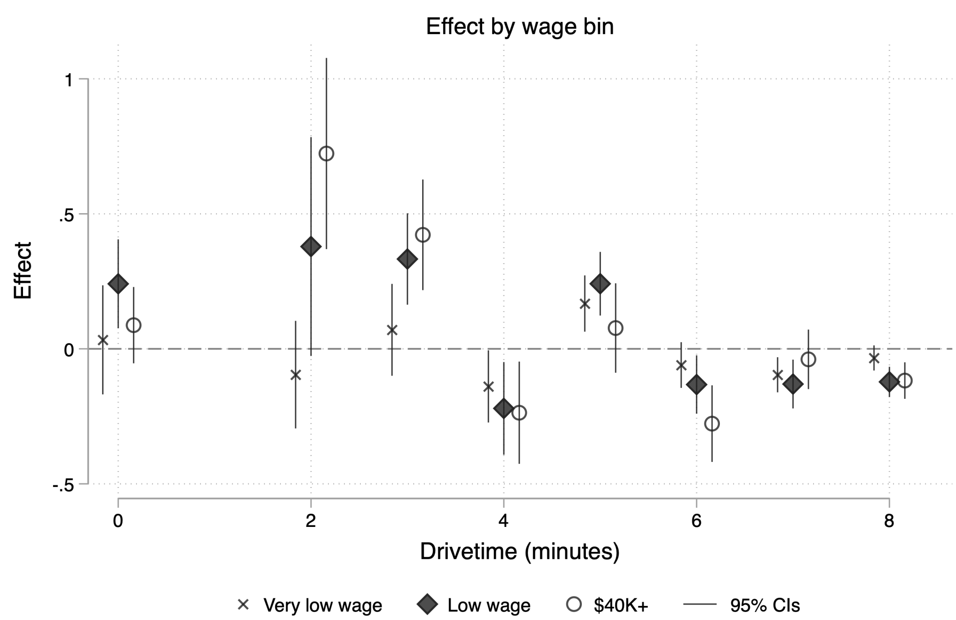


Figure A5: Estimates of $\beta_\tau$ from Equation (1), split by wage bins in the LEHD-LODES.

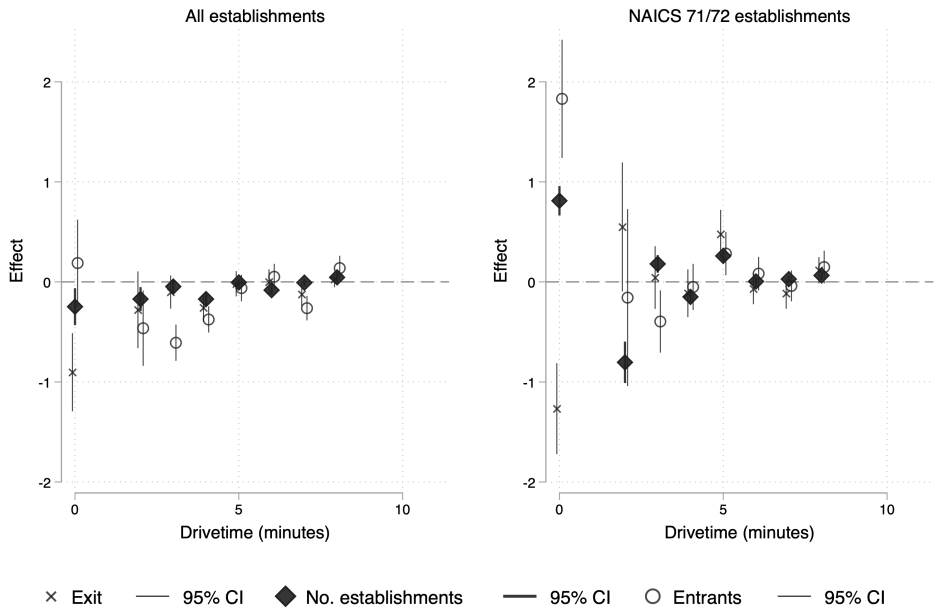


Figure A6: Estimates of $\beta_\tau$ from Equation (1), where the dependent variable is either total establishment counts, counts of exit, and counts of entry. Left: All establishments. Right: NAICS 71/72 establishments.

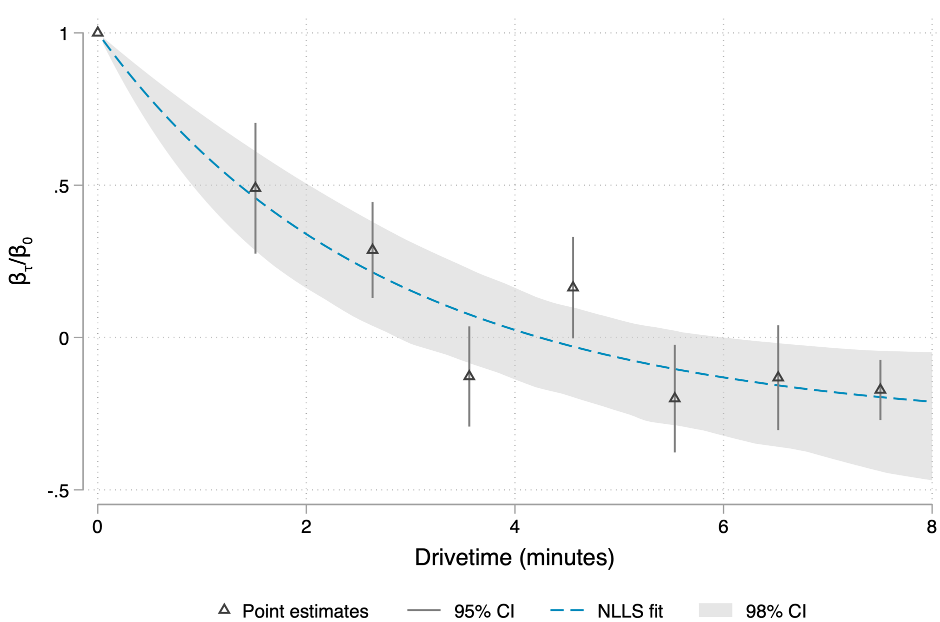


Figure A7: Point estimates of relative employment effects $\beta_\tau/\beta_0$ against the model-implied relationship. Confidence intervals constructed using 600 bootstrap estimates.

| | Drivetime | Drive distance |
|---|---|---|
| $\beta_0$ | 1.27 | 1.29 |
| | (0.15) | (0.16) |
| $\beta_2$ | 0.55 | 0.11 |
| | (0.13) | (0.04) |
| $\beta_3$ | 0.30 | 0.08 |
| | (0.10) | (0.03) |
| $\beta_4$ | -0.15 | -0.33 |
| | (0.08) | (0.04) |
| $\beta_5$ | 0.21 | -0.31 |
| | (0.09) | (0.05) |
| $\beta_6$ | -0.27 | -0.17 |
| | (0.07) | (0.06) |
| $\beta_7$ | -0.18 | -0.23 |
| | (0.07) | (0.09) |
| $\beta_8$ | -0.20 | -0.52 |
| | (0.05) | (0.09) |
| $\beta_9$ | -0.28 | 0.04 |
| | (0.06) | (0.07) |
| $\beta_{10}$ | 0.10 | 0.10 |
| | (0.05) | (0.09) |
| $\beta_{11}$ | -0.19 | -0.64 |
| | (0.07) | (0.15) |
| Observations | 54,864 | 54,864 |
| Pseudo R-squared | 0.92 | 0.92 |
| Ring $\times$ Opened control | ✓ | ✓ |
| Block FE | ✓ | ✓ |
| City-year FE | ✓ | ✓ |

Table A1: Estimates of $\beta_\tau$ for specifications of Equation (1) using both drivetime (column 1) and drive distance (in km, column 2). In parentheses: standard errors two-way clustered at the ring and neighborhood-year level.

| | Casino | All jobs w/in 3 min | All jobs w/in 8 min | Total effect |
|---|---|---|---|---|
| Treated $\times$ Post | 1184.02 | 821.19 | -1619.79 | -435.76 |
| | (299.27) | (202.85) | (1158.72) | (1168.16) |
| $\alpha/\alpha_0$ | | 0.69 | -1.37 | |
| | | (.21) | (.71) | |
| Within R-squared | 0.27 | 0.24 | 0.03 | 0.00 |
| R-squared | 0.75 | 0.98 | 0.99 | 0.99 |
| Observations | 124 | 124 | 124 | 124 |

Table A2: Estimates of the jobs multiplier for varying neighborhood definitions. Standard errors in parentheses clustered at the neighborhood-time level. All regressions include neighborhood and city-year effects. Standard errors for estimates of $\alpha/\alpha_0$ are computed using the delta method.

# B Data appendix

**Casinos** Our treatment and control casinos are available in Table A3 and reflect a manual data collection process described in the main text.

**Spatial data** We use the U.S. Census Bureau's TIGER/Line shapefiles to define 2020 census blocks. We remove from these blocks with zero land area (blocks entirely in water), and then proceed to crop out bodies of water from the remaining shapefiles for display purposes. We define "neighborhood" by first pulling the set of 900 nearest contiguous census blocks to each potential winner. Then we compute drive time and subset the data to minimize overlap and maintain roughly the same number of observations across sites within each distance bin.

**Employment data** Our employment data are the "Workplace area characteristics" (WACs) using the LEHD-LODES 8 data. The data covers most states for the years 2002 to 2019. The Census Bureau has released data some years beyond 2020. We omit this data from our analysis due to the effect the Covid-19 pandemic may have had on local labor markets surrounding casinos.

The data have drawbacks. First, it is aggregated at the block level, which means we cannot view establishment-level changes. Second, industry-level employment is binned at the two-digit NAICS code. Finally, covariates are coarsely binned: the income measures are binned based on annual income which top codes at $40,000 per year, and do not allow us to study effects on wages.

**Drivetimes and distances** We use drive distances and times between block centroids by querying Google Maps API, inputting the regular traffic model, and assuming a Monday, 26 February 9 am CST arrival.

**Business formation and churn** Our data on local establishments comes from the Infogroup Business/Academic data provided by the University of Chicago Library. The business census is essentially a digitization of the 'yellow pages': public listings of business phone numbers and addresses. We treat the provided 'ABI' code as a unique business identifier in the dataset. To form the panel, we assume a business' NAICS-2 code is the modal NAICS-2 code in the panel. We remove blank rows, duplicate observations, and observations without a geocoded address. This filtering process leaves some duplicate ABI-coded observations in the data. As businesses move addresses over time, we treat each ABI $\times$ census block as a unique business, so a move is counted as an exit in one block and an entrance in another. We drop "establishments" associated with public services (e.g., playgrounds) that appear in the data with NAICS 2-digit codes 92 ("Public Administration") and 99 ("Nonclassifiable establishments"). While Infogroup collects additional variables such as the number of employees, these numbers appear to be imputed and may be unreliable.

| State | Winning Location | Alternative Location | Opening | Approval | Story |
|---|---|---|---|---|---|
| IL | Des Plaines (41.9974, -87.8643) | Waukegan (42.3428,-87.8989) | 2011 | 2008 | License Competition |
| MA | Everett (42.3954,-71.0694) | Revere (42.3980, -70.9945) | 2019 | 2014 | License Competition |
| MA | Springfield (42.0985, -72.5875) | Palmer (42.1739, -72.318) | 2018 | 2014 | License Competition |
| MD | Oxon Hill (38.7951, -77.0089) | Fort Washington (2 locations) (38.7977, -76.9626) + (38.7557, -76.9943) | 2016 | 2013 | License Competition |
| PA | Philadelphia (39.9093, -75.1647) | Philadelphia (2 locations) (39.9603, -75.1628) + (39.9514, -75.1536) | 2021 | 2017 | License Competition |
| PA | Philadelphia (39.9644, -75.1326) | Philadelphia (39.9286, -75.1421) | 2010 | 2006 | Second casino won license but company could not complete the project |
| PA | Pittsburgh (40.4481, -80.0222) | Pittsburgh (40.4368, -80.0106) + (40.4396, -79.9895) | 2009 | 2006 | License Competition |
| PA | Bethlehem (40.6142, -75.3569) | Allentown (40.631, -75.4526) | 2009 | 2006 | License Competition |
| PA | Mt Pocono (41.1135, -75.3217) | Pocono Manor (41.1, -75.3918) | 2007 | 2006 | License Competition |
| ME | Oxford (44.1153, -70.446) | Lewiston (44.0968, -70.2192) | 2012 | 2010 | Ballot Initiatives |
| KS | Mulvane (37.469, -97.3285) | Wellington (37.2798, -97.3509) | 2011 | 2010 | License Competition |
| KS | Pittsburg (37.344, -94.7092) | Cherkokee County (37.0134, -94.628) | 2017 | 2015 | License Competition |
| OH | Cleveland (41.499, -81.6932) | Cleveland (41.4961, -81.7032) | 2012 | 2009 | Ballot Initiatives |
| OH | Cincinnati (39.1084, -84.5068) | Clinton County (39.4874, -83.9491) | 2013 | 2009 | Ballot Initiatives |
| NY | Monticello (41.6596, -74.649) | Wawarsing + Thompson (41.6989, -74.4028) + (41.6745, -74.6594) | 2018 | 2014 | License Competition |
| NY | Schenectady (42.8233, -73.9372) | Rensselaer, Cobbleskill + East Greenbush (42.646, -73.7427), (42.7025, -74.398) + (42.6404, -73.6989) | 2017 | 2014 | License Competition |
| NY | Watterloo (42.9701, -76.8434) | Johnson City (42.1238, -75.9979) | 2017 | 2014 | License Competition |

Table A3: Casinos identified as having a potential viable alternative location. License competition includes locations that were in the final deliberation round for a license. In two cases, Philadelphia (2017) and Illinois (2010) a candidate site shares a block group with the winning site and so we excluded it. Monticello (2014) had a number of additional alternative sites closer to New York City which the site selection committee ruled out. Ballot Initiatives includes statewide ballot initiatives that failed. In Maine, the Lewiston site attempted to join the 2010 ballot, but began organizing too late. The Cleveland alternative site and the Cleveland casino site were together on a failed 2006 ballot. The 2009 ballot only included one Cleveland license. In 2009, Ohioans "authorize[d] only one casino facility at a specifically designated location within each of the cities of Cincinnati, Cleveland, Columbus, and Toledo" (Ballotpedia, 2020a). Penn National Gambling controlled two locations and Cleveland Cavaliers owner Dan Gilbert the others. Ohioans rejected similar proposals in 1990, 1990, 1996, 2006, and 2008. Penn National Gambling funded the opposition to the 2008 proposal, which provisioned one resort-style casino to southwestern Ohio run by a Minnesota-based company. (Ballotpedia, 2020b). The 2006 bid authorized racinos and two sites in Cleveland (one of which won the licensure in 2009), so we use the losing 2006 site as if it were a 'runner-up.' The Clinton County site lost in a 2008 ballot initiative, which would have provided a casino monopoly to their location. We believe the losing versus winning ballot initiatives won or lost for reasons orthogonal to our outcomes of interest.

| coefficient | RI-t | RI-$\beta$ |
|---|---|---|
| $\beta_0$ | 0.069 | 0.042 |
| $\beta_2$ | 0.264 | 0.306 |
| $\beta_3$ | 0.069 | 0.056 |

Table A4: Randomization Inference results based on t-stats and coefficients. For the 6 casino sample, there are 144 potential treatment vectors. MacKinnon and Webb (2020) recommend t-stat-based Randomization Inference based on simulations and theory.

# C Randomization Inference

Randomization inference provides a design-based approach to inference. We follow the method of MacKinnon and Webb (2020) and study the effectiveness of randomization inference in difference-in-difference settings. They suggest using t-statistics based on clustered standard errors, which they call RI-t.

Randomization inference tests the sharp null that $E[Y_i(1) = Y_i(0)] = 0$ and relies on an assumption of random assignment–or that the investigator understands the assignment mechanism (MacKinnon and Webb, 2020). The random assignment assumption is stronger than parallel trends. MacKinnon and Webb (2020) argue that RI-t provides a better test in settings with few clusters compared to randomization inference based on coefficients (RI-$\beta$) or the wild-clustered bootstrap. However, the cost is that RI-t may be underpowered relative to these other tests. We conduct one-sided tests because we expect positive job growth.

Our randomization inference procedures treat the 6 casino license competitions as experiments. There are 143 unrealized potential treatment assignments.[8] For each potential treatment assignment, we run the regression model and collect the resulting distribution of statistics. The p-value is then the fraction of unrealized t-statistics that are larger than the realized one. We visualize this information in the figure A8 for t-statistics for $\beta_2$, and $\beta_3$. We reject the sharp null at the 10% level for $\beta_0$ and $\beta_3$ (see table A4). The p-value on $\beta_2$ suggests the results are not statistically significant.

[8] $143 = 2^4 \times 3^2 - 1$

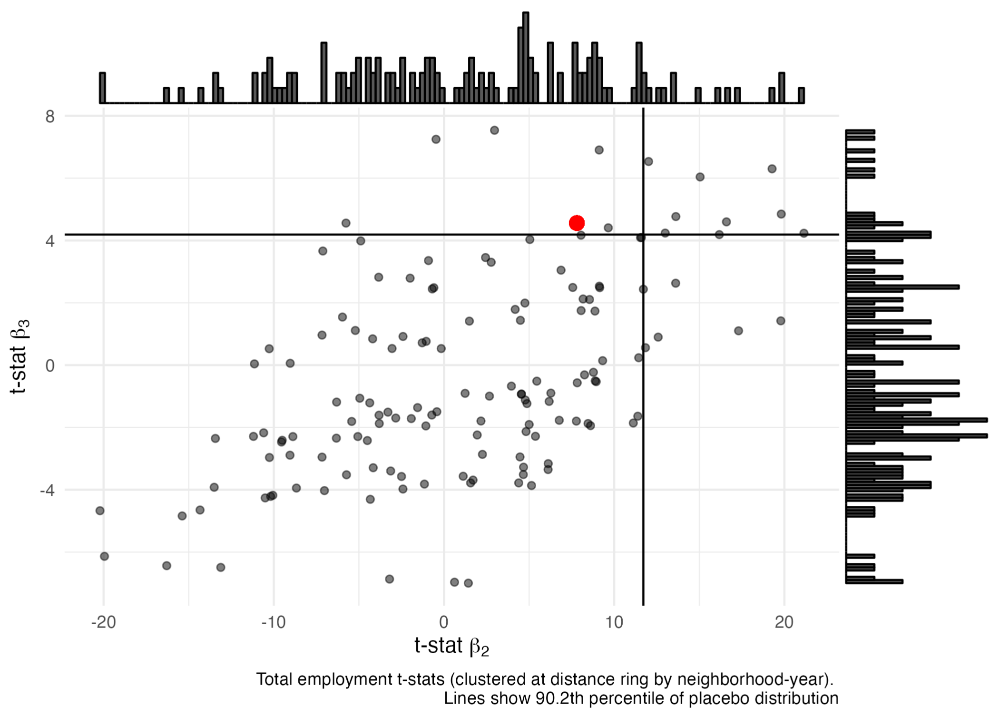


Figure A8: Randomization Inference based on t-statistics. We plot t-statistics for $\beta_2$ on the x-axis and $\beta_3$ on the y-axis. Black lines show values that correspond to an exact p-value of .098.