Game Day Spillovers: Stadiums and the Geography of Consumer Spending

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Abstract

Using daily credit and debit card transaction data from businesses surrounding 123 professional sports stadiums in the United States, I estimate the effect of game-days on local consumer spending. I find that game-days increase spending within three miles of a venue by 0.95% and transactions by 0.86%, with no significant change in spending per transaction. The small aggregate increase masks a strong spatial concentration as spending rises 54.5% within one-quarter mile of the stadium and by 5.0% over the next quarter mile, but changes little or declines modestly as you get farther away from the venue. The strongest spending response is found for restaurants, bars, and retail establishments, and around venues with relatively little baseline commercial activity. These findings suggest that sporting events generate a modest increase in spending within the surrounding market while sharply reallocating economic activity toward businesses located immediately adjacent to the venue.

Keywords: sports stadiums, consumer spending, spatial incidence, local demand

1 Introduction

Cities across the United States have invested billions of dollars in public funds to construct and renovate professional sports stadiums, often under the premise that these facilities generate local economic development.[1] Proponents claim that games at these stadiums stimulate spending, creating positive spillovers for nearby businesses, and increasing citywide consumption. Yet despite the persistence of this narrative, decades of research finds little evidence that teams or stadiums raise city income, employment, or tax revenues. The

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[1] According to Bradbury et al. (2023), between 1970-2020, state and local governments devoted $33 billion in public funds to major-league sports stadiums in the U.S. and Canada. The *Sports Business Journal* states that in 2025, about $6 billion in construction for new and renovated sports facilities in the U.S. and Canada is expected to be completed, and in 2026 it is expected that $14.2 billion will be spent on the same thing. While the share of costs covered by public funds has been declining over time, the median public contribution between 1970-2020 was 73% of venue construction costs.

contrast between the visible surge in economic activity around stadiums and the limited evidence of broader economic gains motivates a closer examination of the magnitude and incidence of game day spending. The key questions are not just how much additional spending sporting events generate, but also where those gains occur, and which businesses capture them.

This paper answers these questions using daily credit and debit card spending data around 123 professional sports venues in the United States. The sample covers all five major leagues: Major League Baseball (MLB), Major League Soccer (MLS), the National Basketball Association (NBA), the National Football League (NFL), and the National Hockey League (NHL). The data records total spending and spending per transaction at individual establishments, enabling me to distinguish whether game-days raise the number of customers, the size of their purchases, or both. I aggregate establishments into concentric rings extending from the stadium entrance to three miles away. This makes it possible to estimate both the overall change in activity around the stadium and its spatial distribution. The sample spans 2022-2025 and contains 759,843 venue-ring-day observations.

The empirical design compares regular-season game dates with dates on which no observed event occurs at the venue. Fixed effects are used to absorb persistent weekly patterns, common state-level shocks, and local seasonality. I also use weather controls to account for game-day conditions. This design identifies the game-day effect from changes in spending around the same venue relative to its usual spending pattern. This contributes to the literature by measuring how game-day activity translates into consumer expenditures. Prior mobility evidence shows that sporting events sharply increase visits to establishments immediately surrounding venues. Expenditure data make it possible to determine whether those movements generate corresponding changes in merchant spending and along which margins.

The first result is that games produce a positive but modest increase in spending across the surrounding market as a whole. A regular-season game increases spending within three miles of the venue by 0.95% and transactions by 0.86%, while spending per transaction is essentially unchanged. The increase in spending therefore operates primarily through additional transactions rather than larger purchases. This aggregate effect establishes that the visible game-day activity around a venue is not entirely a redistribution of spending within the three-mile catchment, but its magnitude is small relative to the concentration of activity immediately outside the stadium.

The spatial incidence of this increase is much more pronounced. Spending rises by 54.5% within one-quarter mile of the venue and by 5.0% between one-quarter and one-half mile. The estimated effect is essentially zero between one-half and one mile and becomes modestly negative in the next two rings. Transactions exhibit the same sharp gradient, increasing by approximately 37% in the nearest ring. Thus, the small net increase within the three-mile market masks a substantial reallocation of economic activity to-

ward businesses located immediately adjacent to the venue. This distinction between aggregate and spatial incidence is central to interpreting the economic effect of a game.

Looking at the particular businesses affected provides further evidence on the nature of the demand shock. Within one-quarter mile, spending at restaurants and bars increases by about 64%, while retail spending rises by approximately 33%. By contrast, the immediate effects for grocery and liquor stores, lodging, and transportation, parking, and fuel are small and statistically indistinguishable from zero. The game-day response is therefore concentrated in establishments whose goods and services are natural complements to attending a game rather than representing a uniform increase in demand for nearby commerce. The incidence also differs across merchant characteristics. Spending at establishments classified as independent rises by roughly 69% within one-quarter mile, whereas the corresponding estimate for identified chains is close to zero. Effects across predetermined merchant-size quartiles show that the increase is not confined to the largest establishments, but the largest quartile does experience the largest proportional response.

The intensity and context of the event also matter. Within one-quarter mile, a 10% increase in attendance relative to a venue's usual crowd is associated with a 4.75% larger spending response, and the estimated inner ring effect rises from 51.3% at the 25th percentile of a venue's attendance distribution to 58.1% at the 75th percentile. The surrounding commercial environment also matters. Within one-quarter mile, game-day spending increases by approximately 71% around venues with low baseline commercial activity and 66% around medium-activity venues, compared with 26% around venues with high baseline activity. There is also substantial heterogeneity across leagues. NFL games generate the largest near-venue response, increasing spending by approximately 86%, followed closely by MLB games at 81%. The corresponding effects are 53% for MLS, 45% for NHL, and only 13% for NBA games. These differences likely reflect more than crowd size alone. Leagues differ systematically in where their venues are located, the timing and frequency of games, and the extent of pre- and post-game consumption.

Together, these results reconcile the highly visible economic activity surrounding sporting events with the much weaker evidence of broader economic effects. Games generate a measurable increase in spending within the local market, but that increase is small relative to the dramatic concentration of demand immediately surrounding the venue. The economic incidence of a game therefore depends critically on geography and merchant type as nearby restaurants, bars, retailers, and locally oriented establishments can experience large short-run gains even when the effect on the broader stadium market is modest. Distinguishing net local demand from its spatial and sectoral incidence provides a way to connect establishment-level game-day effects with the broader literature finding limited aggregate economic gains from professional sports facilities.

1.1 Related Literature

There is considerable public interest in the economic activity generated from professional sports events, as teams frequently seek public funding for new stadiums on the grounds that such projects will stimulate local economic activity. Early work in this area exploited variation in team arrivals, stadium construction, and public funding to assess the impact of facilities on per-capita income, employment, and business formation (Baade and Dye (1988), Baade (1996), Coates and Humphreys (1999), Propheter (2012), Islam (2019), Agha (2013), Agha and Coates (2015), and Agha and Rascher (2021)). The broad conclusion from this literature is that professional sports facilities rarely generate large increases in aggregate economic activity. One explanation is substitution in the sense that spending associated with sports may come partly at the expense of other local consumption. Consistent with this mechanism, Coates and Humphreys (2003) find positive effects in recreation but offsetting declines in employment and earnings in other service and retail sectors. A separate literature asks whether sports teams generate consumption-amenity benefits that do not appear in measured income or employment. Carlino and Coulson (2004), for example, estimate such benefits using compensating differentials, although the robustness and interpretation of these estimates have been debated (Coates et al. (2006)). Related work also examines whether sports facilities affect local property values. Humphreys and Nowak (2017) study NBA team departures and find that the loss of a team can reduce nearby property values, consistent with sports facilities generating localized amenity benefits. The spending effects studied here therefore represent one particular channel through which sports facilities may affect local economies rather than a complete measure of their welfare effects.

More similar to this paper, another strand of research in this area looks at the impact of stadiums and arenas on surrounding economic activity. Much of this work is motivated by Humphreys and Zhou (2015), who develop a spatial model of urban service consumption in which a professional sports facility can act as an anchor that attracts consumers to a particular location. Their model emphasizes that the effects of a facility depend on the pre-existing commercial environment as placing a stadium within an established consumption center differs from using a facility to generate a new service-consumption center or "arena district." The model developed below adapts this insight to the short-run setting studied here. I hold the surrounding commercial environment fixed and ask how activating the venue for a game changes spending relative to the commercial activity that would otherwise have occurred.

The related empirical evidence supports the conclusions of Humphreys and Zhou (2015) and finds that any facility effects are highly localized and heterogeneous across sectors. Harger et al. (2016) find no evidence that new sports facilities in the 12 U.S. cities they look at increase nearby business openings. Propheter (2019) shows that a new MLS stadium in Denver has no employment effects either in the area

around the new stadium or around where the prior home stadium was, while Propheter (2020) finds that retail establishments within one-half mile of Sacramento's Golden 1 Center experienced substantially shorter survival times than comparable establishments farther away. Using sector-level sales data, Stitzel and Rogers (2019) show that positive effects around NBA arenas are concentrated in particular types of establishments. These findings suggest that stadiums rarely attract new firms, and instead reshuffle existing activity within cities.

This paper is also related to research using local sales or sales tax data to estimate the economic effects of stadiums. While Coates (2006) finds that the Super Bowl in Houston temporarily raised sales in the city, most other studies of sporting events find negligible or offsetting changes (Baade and Matheson (2001) and Depken and Coates (2009)). Even targeted case studies of new publicly funded venues such as Toyota Park in suburban Chicago find no increase in local sales tax receipts (Propheter (2014)). More recently, Bradbury (2024) studies the relocation of the Atlanta Braves to Cobb County using sales-tax collections. He finds a small and statistically insignificant increase in county taxable sales and evidence that part of the activity generated by the stadium development displaced other local spending. Analyses of hotel data find either higher occupancy rates near venues but reduced demand in other neighborhoods (Chikish et al. (2019)), or no discernible effect of sporting events on the hotel market (Depken and Stephenson (2018) and Stephenson (2021)). Even large events generate limited net gains because most hotels operate near capacity year-round and visitors attending games often crowd out other travelers.

The most similar study to the current paper is Abbiasov and Sedov (2023). They use mobile-device foot traffic data from 2018 to map how stadium attendance shocks translate into visits to nearby establishments. Using data on 92 stadiums and arenas across the NFL, MLB, NBA, and NHL, they find sharp, short-lived increases in visits immediately surrounding venues, particularly for MLB and NFL games, together with much smaller or offsetting effects farther away.

My paper builds directly on this work by observing consumer expenditures rather than inferring economic activity from visits. The distinction allows me to ask whether the large movements in foot traffic documented by Abbiasov and Sedov (2023) translate into corresponding changes in spending and, importantly, to identify the margin through which that spending response occurs. The transaction data separately measure total spending, the number of transactions, and spending per transaction. They therefore distinguish between games generating more purchasing occasions and games causing consumers to spend more on each purchase. The data also allow me to examine how the spending response differs across merchant characteristics that are not observed in the mobility data. This paper also extends Abbiasov and Sedov (2023) by covering several post-pandemic seasons from 2022-2025.

The results both confirm and broaden the patterns found in Abbiasov and Sedov (2023). Consistent with

their mobility evidence, I find that economic activity is sharply concentrated immediately around venues, is particularly large for MLB and NFL games, grows with attendance, and weakens substantially with distance. The expenditure data show that the spatial concentration in visits is accompanied by a similarly sharp concentration in spending, while the increase across the broader three-mile market is much smaller. More importantly, I find that the spending response operates almost entirely through an increase in the number of transactions rather than an increase in spending per transaction, and that the response differs substantially across merchant categories and characteristics. These additional findings demonstrate the value of spending data in revealing how stadium events reshape not just where consumers go, but how much they spend and in what sectors.

The paper also relates to a broader literature on the geography of consumer demand. Using credit-card transactions, Agarwal et al. (2020) show that consumer expenditure is geographically localized and that the rate at which spending declines with distance differs substantially across consumption sectors. Su (2022) similarly emphasizes that access to consumption amenities, including restaurants, extends beyond the immediate neighborhood and that the spatial reach of amenities differs across categories. These findings provide a broader urban-economics foundation for studying how a temporary demand shock is distributed across distance and merchant type.

Recent work also highlights the distinction between measuring visits and measuring expenditures. Klopack and Luco (2025) compare payment-card transactions with cellphone location data and show that the two measures are positively related but capture distinct dimensions of local consumption. They also find that responses to local economic shocks decay rapidly with distance and differ across retail categories. This distinction is particularly important in the stadium setting because an event may generate large changes in foot traffic without producing proportionate changes in spending.

The remainder of the paper is organized as follows. Section 2 develops a model of game-day demand. Section 3 describes the data. Section 4 presents the empirical strategy, while Section 5 reports the results and discusses robustness and alternative specifications. Section 6 concludes.

2 A Model of Game-Day Demand

A game at a sports venue can concentrate consumers in a particular location and thereby increase demand for nearby services. This idea is central to the spatial model in Humphreys and Zhou (2015), where sports facilities can serve as anchors for local service consumption. I adapt this mechanism to the short-run setting studied here. Rather than considering how the construction of a facility changes firm location or property values, I take the surrounding commercial environment as fixed and ask how activating the venue for a game

changes spending at nearby businesses.

The key distinction is between stadium districts with relatively little commercial activity on ordinary days and those that are already established consumption centers. A game brings a large group of consumers to either location, but in the latter case there is also more ordinary spending that can be displaced. The incidence of the resulting demand shock therefore depends both on the amount of pre-existing commercial activity and on whether a particular merchant is especially well suited to serving event attendees.

2.1 Game-Day Demand and Local Commercial Activity

Partition the area surrounding a venue into distance rings $r = 1, \ldots, R$ and merchant categories $c = 1, \ldots, C$. Let B_r denote expected spending in ring r on a day when the venue does not host an event. If ω_{rc} is the share of normal spending allocated to category c, where

$$\sum_c \omega_{rc} = 1, \tag{1}$$

then baseline spending in category c is

$$S^0_{rc} = \omega_{rc} B_r. \tag{2}$$

The parameter B_r summarizes the amount of ordinary commercial activity surrounding the venue. A stadium surrounded primarily by parking lots and relatively few businesses will have a low value of B_r, while a venue located in an established downtown or entertainment district will generally have a higher value.

Now consider a game with attendance A. Let h_r denote the exposure of ring r to event attendees. This term captures the fraction of the event crowd that reaches businesses in the ring and can therefore vary with distance from the venue. Let q_c denote expected spending in category c generated by an attendee who reaches the surrounding commercial area. Gross event-generated spending in category c and ring r is therefore

$$A h_r q_c. \tag{3}$$

The parameters q_c need not be equal across merchant categories. Some purchases can be combined naturally with attendance at the game. Restaurants and bars, for example, can be incorporated directly into pre-game or post-game activity, while demand for other goods and services may be less closely connected to the event.

At the same time, the event can displace spending that would otherwise have occurred near the venue. Let $\delta_r(A) \in [0, 1]$ denote the fraction of ordinary commercial activity in ring r displaced by a game with attendance A, with

$$\delta_r'(A) \geq 0. \tag{4}$$

Displacement can arise because traffic, parking constraints, congestion, or the presence of the event crowd causes ordinary consumers to avoid or substitute away from the area.

Game-day spending in category c is consequently

$$S_{rc}^{1} = [1 - \delta_r(A)]\, \omega_{rc} B_r + A h_r q_c. \tag{5}$$

Subtracting equation (2) gives the net game-day effect:

$$\Delta S_{rc} = A h_r q_c - \delta_r(A) \omega_{rc} B_r. \tag{6}$$

The first term is spending generated by the consumers brought to the area by the event. The second is ordinary spending displaced from the local market. Aggregating equation (6) over merchant categories gives

$$\Delta S_r = A h_r Q - \delta_r(A) B_r, \qquad Q \equiv \sum_c q_c. \tag{7}$$

A game can produce substantial gross attendee spending but only a modest increase in total spending if event demand overlaps with an already active commercial market. Conversely, when little commercial activity would otherwise occur near the venue, a larger fraction of attendee spending represents an increase relative to the normal level of activity.

2.2 Comparative Statics

Equation (6) implies that category c experiences a positive game-day spending effect when

$$A h_r q_c > \delta_r(A) \omega_{rc} B_r. \tag{8}$$

Equivalently,

$$B_r < B_{rc}^{*} \equiv \frac{A h_r}{\delta_r(A)} \frac{q_c}{\omega_{rc}}. \tag{9}$$

The threshold B_{rc}^{*} is the maximum level of normal commercial activity for which the event generates a positive net spending effect in category c. It depends on both the strength of event demand and the importance of the category in ordinary local commerce. Define

$$\kappa_{rc} \equiv \frac{q_c}{\omega_{rc}}. \tag{10}$$

A high value of κ_{rc} indicates that category c receives substantial demand from event attendees relative to the amount of spending it normally receives in that market.

Proposition 1 (Commercial Activity Threshold)**.** *For a given event, ring, and merchant category, the game-day spending effect is positive if and only if*

$$B_r < B_{rc}^* = \frac{Ah_r}{\delta_r(A)}\kappa_{rc}. \tag{11}$$

Thus, a positive game-day effect becomes less likely as normal commercial activity increases and more likely as the category becomes more strongly oriented toward event consumption.

Proposition 1 distinguishes a stadium that temporarily creates a consumption center from one located inside an already active commercial market. When B_r is small, relatively little ordinary spending is available to be displaced, so event demand can generate positive net effects across a broad set of nearby businesses. When B_r is large, the same event demand must offset a larger amount of ordinary commercial activity. A large crowd can therefore produce a highly visible increase in activity around the venue without generating an equally large increase in spending relative to what would otherwise have occurred.

The threshold also differs systematically across merchant categories. Consider categories c and j, and suppose that category c is more strongly associated with event consumption:

$$\kappa_{rc} > \kappa_{rj}. \tag{12}$$

It follows immediately that

$$B_{rc}^* > B_{rj}^*. \tag{13}$$

Proposition 2 (Selective Incidence)**.** *Suppose category c is more strongly oriented toward event consumption than category j, so that $\kappa_{rc} > \kappa_{rj}$. Then there exists an intermediate range of normal commercial activity,*

$$B_{rj}^* < B_r < B_{rc}^*, \tag{14}$$

for which the game increases spending in category c but does not increase spending in category j.

This result implies that increasing baseline commercial activity changes not only the magnitude of the game-day response but also its composition. In a relatively inactive stadium district, event demand can generate positive effects across several types of establishments. At intermediate levels of normal activity, the positive response becomes concentrated in categories closely connected to attendance. At sufficiently

high levels of ordinary commercial activity, even these categories need not experience a positive net increase because the spending generated by attendees can be offset by displaced normal demand.

The same threshold logic can generate spatial implications as well. If attendee exposure declines with distance from the stadium,

$$h_1 > h_2 > \cdots > h_R, \tag{15}$$

then, holding other features of the local market fixed,

$$B^*_{1c} > B^*_{2c} > \cdots > B^*_{Rc}. \tag{16}$$

A category can therefore receive a positive effect immediately surrounding the venue but not farther away. Moreover, because categories with larger κ_{rc} have higher thresholds, merchant categories most closely connected to event consumption can continue to benefit under conditions in which less event-oriented businesses do not.

Attendance affects the threshold through both gross event demand and displacement. From equation (9),

$$\frac{\partial \log B^*_{rc}}{\partial \log A} = 1 - \frac{A\delta'_r(A)}{\delta_r(A)}. \tag{17}$$

If displacement rises less than proportionally with attendance, larger crowds increase the range of commercial environments in which a positive spending effect occurs. If displacement rises proportionally, higher attendance scales gross event demand and displaced spending together and leaves the threshold unchanged. The model therefore does not suggest that higher attendance is necessary to generate a larger net effect in every setting.

3 Data

To measure economic activity around professional sports venues, I use SafeGraph point-of-interest (POI) spending data accessed through the Dewey Data Portal.[2] SafeGraph aggregates anonymized credit and debit card transactions from a panel of U.S. consumers and reports total daily spending at individual establishments. Each establishment has a unique Placekey which can be linked to records that contain its name, geographic coordinates, industry classification, and brand affiliation. The primary outcome is total nominal card spending at an establishment on a calendar date, but I also observe daily average spending per transaction and monthly totals for transactions and unique customers. I construct implied daily transaction counts

[2] https://doi.org/10.82551/NSF5-R186

by dividing daily spending by daily average spending per transaction.

Establishments are then matched to sports stadiums using manually collected data on the latitude and longitude of each sports stadium in the U.S. that houses a team in one of the five major North American sports leagues (NFL, NBA, MLB, NHL, and MLS). This results in 123 venues across the five leagues. I then hand collect complete game schedules for all five leagues and record the dates of all home games at the stadiums. I classify each game as either a regular season, postseason, or preseason game. I observe 21,409 regular season games (9,376 MLB games, 1,957 MLS games, 4,338 NBA games, 1,081 NFL games, and 4,657 NHL games).

Treatment is defined at the venue level rather than by team. This distinction matters because several teams share a facility, some teams change venues during the sample, and some games are held at alternative sites. A venue-date is treated whenever a regular-season game occurs at that physical venue, regardless of which team uses the facility. Thus, games played by co-tenants enter the same venue treatment calendar, while a game played at an alternative facility is assigned to that facility rather than to the team's usual stadium. I do not observe concerts, college games, conventions, or other events that don't appear in one of the five league schedules. Unobserved events at the same facility may therefore attenuate the estimated difference between regular season and comparison dates. I test for overlapping events at other nearby venues in a separate robustness check.

From the SafeGraph data, I keep all POIs within a 3-mile radius of each stadium and calculate each establishment's distance from the venue. I then aggregate establishments into five concentric rings: 0–0.25 miles, 0.25–0.5 miles, 0.5–1 mile, 1–2 miles, and 2–3 miles. I exclude businesses located inside the venue, including concessions, team stores, and other stadium operations. The first ring is intended to capture establishments that can be reached within a few minutes of entering or leaving a venue, while the outer rings capture progressively broader commercial areas. Catchments can also overlap in dense sports districts as 101 of the venues have another professional sports venue within three miles. Thus, while I observe 189,455 unique establishments, I observe 302,329 venue-establishment pairs.[3] Overall, Ring 1 contains 4,772 distinct establishments and 4,888 venue-establishment pairs, while the corresponding counts for the outermost ring rise to 96,673 and 126,725. Across the full 3-mile catchment, a venue-day contains an average of 892 observed POIs.

I also collect reported attendance for each game.[4] Attendance records are matched to the event calendar using league and game identifiers. When more than one event occurs at the same physical venue on the same

[3]While the baseline treatment is the event status of the focal venue, robustness checks either exclude dates with an event at a nearby venue or control separately for nearby-event exposure.

[4]Attendance for MLB games is collected from the MLB Stats API. Attendance for MLS, NBA, and NFL games is collected from ESPN scoreboards and game summaries. NHL attendance is collected from official NHL Game Summaries, with ESPN used when the official report does not contain an attendance figure.

date, attendance is aggregated to the venue-date level. I also merge the spending and event panels with daily weather data from Open-Meteo using the stadium coordinates. The weather variables include daily temperature, precipitation, snowfall, and maximum wind speed. Weather may affect attendance, travel to the stadium, and normal business activity near the venue.

One limit of the spending data is that it only records card transactions and thus omits cash purchases. Another is that SafeGraph observes a panel of cardholders rather than the universe of cardholders. Thus, for intuition on the dollar-magnitude of some of the changes, I merge the spending data with SafeGraph's monthly panel summary and construct a state-month expansion factor based on the number of cardholders represented and the adult population obtained from the U.S. Census Bureau. This scaling is only used for interpreting economic magnitudes and the main analysis does not use the transformed data.

Finally, I address changes in SafeGraph's POI coverage by requiring establishments to be observed throughout the calendar month in which they contribute to the daily panel. This prevents within-month entry into or exit from the dataset, from being recorded mechanically as a change in spending. Some transactions may also be assigned to the date on which they are processed rather than the date on which they occur. For this reason, all specifications include state-day fixed effects, so that comparisons are between stadiums within the same state on the same day.

Table 1: Summary of Spending Data by Ring

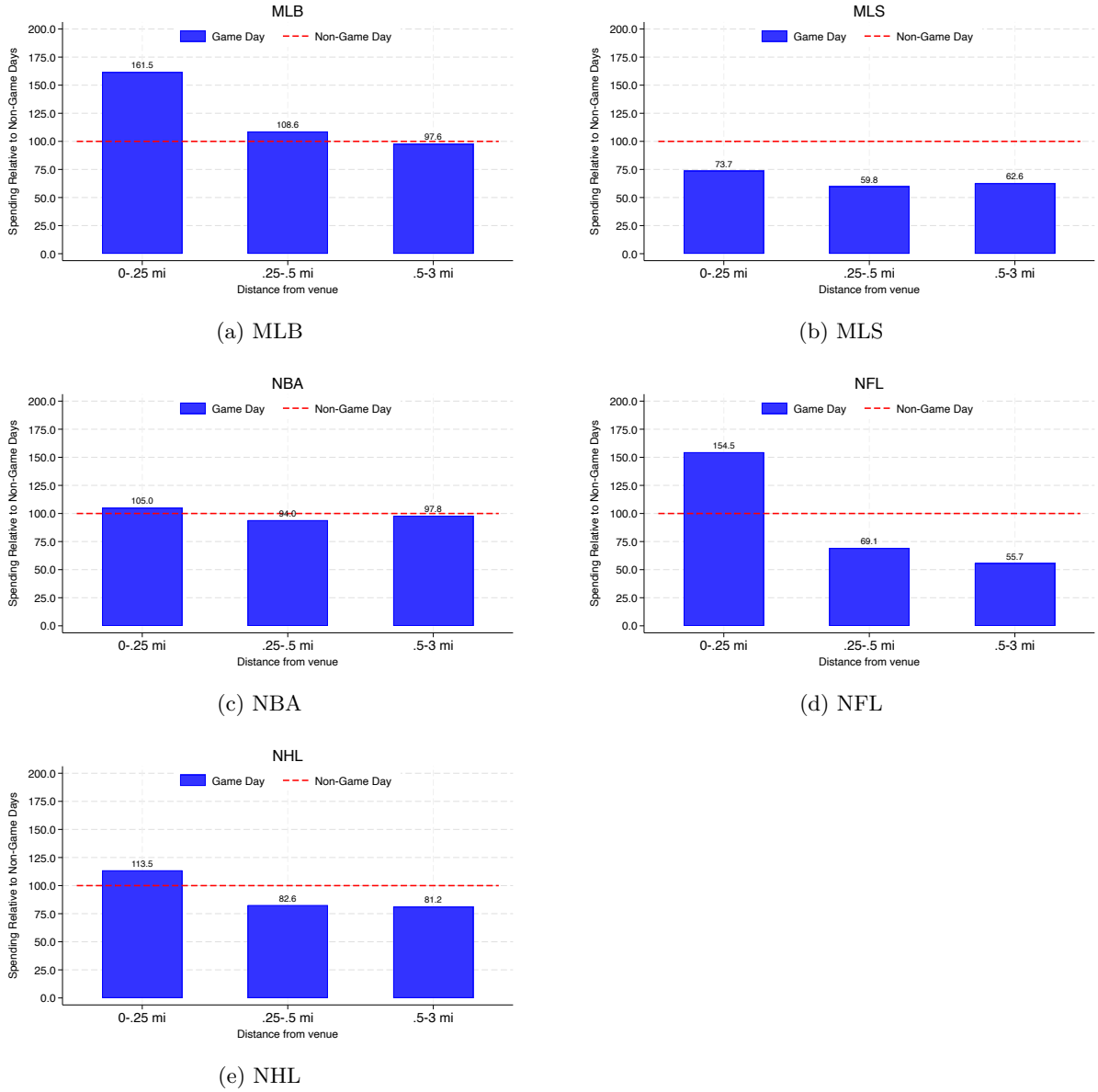
Total	N	Mean ($000)	Median ($000)	SD ($000)	Min ($000)	Max ($000)
All	157,685	174.847	88.572	281.557	0.063	8,523.318
By Ring	N	Mean ($000)	Median ($000)	SD ($000)	Min ($000)	Max ($000)
0–0.25 miles	134,981	3.228	0.573	8.943	0.000	464.220
0.25–0.5 miles	152,503	12.752	3.163	88.376	0.000	5,346.606
0.5–1 mile	157,171	23.114	10.005	43.506	0.000	1,905.944
1–2 miles	157,503	61.014	26.958	133.302	0.000	4,941.322
2–3 miles	157,685	75.768	34.472	147.873	0.000	8,460.425
By Gameday	N	Mean ($000)	Median ($000)	SD ($000)	Min ($000)	Max ($000)
No Game	139,361	173.595	88.223	276.938	0.063	8,487.326
Game	18,324	184.366	91.473	314.320	0.184	8,523.318

Spending data comes from SafeGraph (accessed through Dewey Data). Spending is measured in thousands of dollars and is aggregated across POIs at the day-stadium level. The sample covers all sports venues from January 2022 – July 2025.

The raw spending data cover January 1, 2019 through July 31, 2025, but for the main analysis I exclude observations from before 2022 because of the Covid-19 pandemic. During this time there were many restrictions on attendance that could impact the results. Thus, the main estimation sample covers January 1, 2022 through July 31, 2025. This leads to 157,685 venue-days, of which 18,324 are game-days.

Table 1 summarizes the spending data by ring and by whether or not a game was played at the venue. The number of venue-ring-day observations is 759,843. Because some venue-ring cells contain no observed spending on some dates, the ring panel is not perfectly balanced. Average spending increases the farther one moves away from the venue as mean daily spending within 0.25 miles of a venue is $3,228 and is $75,768 in the ring 2-3 miles from a stadium. This is mostly because the area and merchant count increase sharply with distance. Average spending is slightly higher on game-days ($184,366 versus $173,595 on non-game-days), but there is also more variation and so the difference is not statistically significant.

Figure 1: Spending By League



(a) MLB

(b) MLS

(c) NBA

(d) NFL

(e) NHL

Figure 1 plots relative spending on game-days compared to non-game-days by distance from the stadium

(split into within 0.25 miles of the venue, between 0.25-0.5 miles of the venue, and between 0.5-3.0 miles of the venue) and sports league. Four of the five leagues show a spike in spending in the ring nearest to the stadium, but the magnitude differs quite a bit across leagues. For MLB stadiums, spending in the inner ring increases by 61.5% on game-days and for the NFL the increase is 54.5%. On the other hand, businesses in the inner ring around NBA arenas only see a 5.0% increase in spending on game-days while establishments nearest NHL arenas get a 13.5% boost in spending. For all leagues, this levels off as we move further from the stadium, with outer rings around NFL, NBA, and NHL arenas seeing declines in spending on game-days.

The figure for MLS game-days is the most unusual in that even the ring closest to the stadium sees a 26.3% decline in average spending relative to non-game-days. Part of this is because we are not yet controlling for important factors like day of the week. About 81% of MLS games occur on weekends, while non-game-day spending on Saturdays and Sundays around MLS venues is about 50% and 47%, respectively, of the total average across all non-game-days. The regression analysis done below makes sure to account for day of the week and other observed patterns.

4 Empirical Analysis

The empirical analysis is interested in understanding whether the higher mean spending on game-days observed in Table 1 is a real phenomenon, the geographic extent of any game-day impact, and which businesses are affected the most. Equation (7) shows that spending generated by event attendees can be offset by ordinary commercial activity displaced from the stadium district. I therefore begin by estimating the net change in spending, transactions, and spending per transaction within three miles of the venue.

Let s index a venue and t the day. Then define indicator G_{st} equal to 1 when venue s hosts a regular season game on date t and zero otherwise. In the baseline specification, days where the stadium hosts preseason, postseason, or other observed league events are omitted from the analysis. I then estimate the following specification:

$$Y_{st} = \beta G_{st} + X'_{st}\gamma + \eta_{s,d(t)} + \lambda_{q(s),t} + \tau_{s,m(t)} + \varepsilon_{st} \tag{18}$$

where X_{st} is a vector of weather conditions (mean temperature, precipitation, snowfall, and maximum windspeed) at venue s on day t. The outcome Y_{st} is aggregate activity across all establishments within 3 miles of venue s where activity is measured as either total spending, number of transactions, or spending per transaction. I denote the state containing venue s as $q(s)$, the day of the week as $d(t)$, and the calendar year-month as $m(t)$. Thus, this specification includes venue-day-of-week, state-day, and venue-month fixed effects. The venue-day-of-week fixed effects account for normal spending patterns around a venue for each day

of the week. Ordinary commercial activity generally follows a weekly pattern as does some league schedules (such as most NFL games being on Sundays), and so this controls for correlation in those patterns. The state-day fixed effects absorb any statewide shocks such as holidays, changes in card-panel activity, travel shocks, and weather.[5] Finally, the venue-month fixed effects absorb seasonality effects at the stadiums due to changes in tourism, team seasons, the local merchant environment, and card–panel coverage.

The identifying assumption is that, conditional on these fixed effects and weather controls, regular season game timing is unrelated to unobserved venue-specific daily shocks to spending. League schedules are not determined randomly. For example, NFL games are concentrated on Sundays and other leagues exhibit systematic scheduling patterns over the course of the season. However, venue-day-of-week and venue-month fixed effects absorb these recurring venue-specific weekly and seasonal patterns. Identification therefore comes from variation in game timing relative to a venue's usual weekly and seasonal spending patterns. Section 5.4 assesses this assumption using specifications that exclude or control for events at nearby venues, test alternative fixed-effect structures, and evaluate placebo assignments of game dates.

Because the fixed effect structure of the estimating equation makes the effective identifying sample less transparent, Appendix Table 6 reports the support and distribution of the identifying weight in the baseline specifications. The aggregate estimate draws on 122 venues in 28 states, while 119 venues contribute to the Ring 1 estimate (below). MLB supplies approximately 41% of the treated coefficient weight in both specifications, with the remainder distributed across the other four leagues. Identification is also widely dispersed across venues with the largest individual venue accounting for less than 2% of the weight, and the ten largest venues together accounting for about 17–18%. Thus, the estimates are not identified by a small set of stadiums or by a single league.

4.1 Where are Game-Day Effects Largest?

The aggregate specification does not reveal where within the stadium market the spending response occurs. Proposition 1 shows that a positive game-day effect depends on event demand relative to the ordinary commercial activity in the local market. If attendee exposure, h_r, declines with distance from the venue, equation (16) implies that the maximum level of normal commercial activity consistent with a positive effect also declines. Positive spending effects should therefore become more difficult to sustain as one moves farther from the venue, holding the surrounding commercial environment fixed. I estimate this spatial profile flexibly

[5] Thus, regressions don't use variation from venues in states with only one stadium.

using the venue-ring-day panel:

$$Y_{srt} = \sum_{j=1}^{5} \beta_j G_{st} \mathbb{1}\{r = j\} + X'_{st}\gamma + \eta_{sr,d(t)} + \lambda_{q(s),t} + \tau_{s,m(t)} + \varepsilon_{srt} \tag{19}$$

The venue-ring-day-of-week fixed effects, $\eta_{sr,d(t)}$, give every ring around each venue its own normal weekday activity pattern. This specification tests where the increase in spending occurs in relation to the sports facility.

Another important factor is likely to be how popular the game is. Higher attendance increases gross event-generated demand, but can also increase the displacement of ordinary activity. Equation (17) shows that a larger crowd expands the set of commercial environments in which a positive effect occurs when displacement rises less than proportionally with attendance. I therefore treat the relationship between attendance and net spending as an empirical question and estimate how the response varies with crowd size separately across distance rings. I measure event intensity using centered log attendance, which is defined as:

$$\widetilde{a}_{st} = \log A_{st} - \overline{\log A}_s \tag{20}$$

where A_{st} is reported attendance for a game on day t and $\overline{\log A}_s$ is average log attendance at venue s over all s's regular season games.[6] I then estimate:

$$Y_{srt} = \sum_{j=1}^{5} \left[\beta_j G_{st} + \theta_j \widetilde{a}_{st}\right] \mathbb{1}\{r = j\} + X'_{st}\gamma + \eta_{sr,d(t)} + \lambda_{q(s),t} + \tau_{s,m(t)} + \varepsilon_{srt} \tag{21}$$

The coefficient β_j measures the effect of a game on spending in ring j when the venue has its average attendance, and θ_j measures how the ring response changes as attendance differs from the usual crowd size. The preferred specification excludes venue-days with more than one event since two games at the same venue on the same day create additional complications.

I also look at heterogeneity in the spending effect along several other dimensions. These include the league (MLB, MLS, NBA, NFL, or NHL), the type of game (regular season or postseason), the day of week, and normal activity level around the venue. For each characteristic H, I estimate an extension of equation 19 that interacts the event indicator, distance ring, and categories of H:

$$Y_{srt} = \sum_{h} \sum_{j=1}^{5} \beta_{jh} G_{st} \mathbb{1}\{r = j\} \mathbb{1}\{H = h\} + X'_{st}\gamma + \eta_{sr,d(t)} + \lambda_{q(s),t} + \tau_{s,m(t)} + \varepsilon_{srt} \tag{22}$$

[6] $\widetilde{a}_{st}$ is set to zero for all non-game-days.

4.2 Who Captures the Game-Day Effect?

To understand which types of businesses benefit the most from game-days, I categorize establishments along several dimensions. The first is based on the broadly defined SafeGraph industry categories where I look separately at restaurants and bars, grocery and liquor stores, lodging, transportation, parking and fuel, and retail. Merchant category provides a direct empirical counterpart to the selective-incidence mechanism in Proposition 2. The model predicts that categories more strongly oriented toward event consumption have higher commercial-activity thresholds and can therefore continue to experience positive game-day effects under conditions in which less event-oriented categories do not. Restaurants and bars provide a natural example of a category likely to be closely connected to pre-game and post-game consumption.

I also categorize establishments on whether they are likely to be a chain based on if its brand identifier is observed at two or more POIs in the U.S. dataset. A business is then categorized as independent if it has no reported brand affiliation and its business name appears at only one POI. Establishments that don't fit either category are left unclassified. A third categorization I make is to group firms based on their size. To measure size I rank merchants within each venue and year according to their average spending on active days during prior non-event years. I then divide them into quartiles that measure a merchant's size relative to other establishments in the same venue market. I also separately measure the commercial scale of the district around each venue using average spending in the closest ring on dates with no observed event. This measure provides the empirical counterpart to B_r in the model. I classify venues as either low, medium, or high activity based on mean Ring 1 spending. These groups allow me to examine the central comparative static in Proposition 1, which is whether the net game-day response becomes smaller as the amount of ordinary commercial activity exposed to displacement increases.

For each classification dimension c, I then estimate the following model:

$$Y^c_{sgrt} = \sum_g \sum_{j=1}^{5} \beta^c_{gj} G_{st} \mathbb{1}\{Group^c = g\} \mathbb{1}\{r = j\} + X'_{st}\gamma + \mu^c_{gj} + \eta_{sr,d(t)} + \lambda_{q(s),t} + \tau_{s,m(t)} + \varepsilon^c_{sgrt} \tag{23}$$

Here $\mathbb{1}\{Group^c = g\}$ denotes membership in group g under classification c, and μ^c_{gj} captures average differences in levels across merchant group-rings. The remaining fixed effects are the same as in the baseline model. The coefficient β^c_{gj} measures group-specific event-day deviations within each ring.

5 Results

Table 2 first asks whether the intense activity observed near a venue translates into a measurable increase in spending for the surrounding market. The estimates from equation (18) show that it does, although the

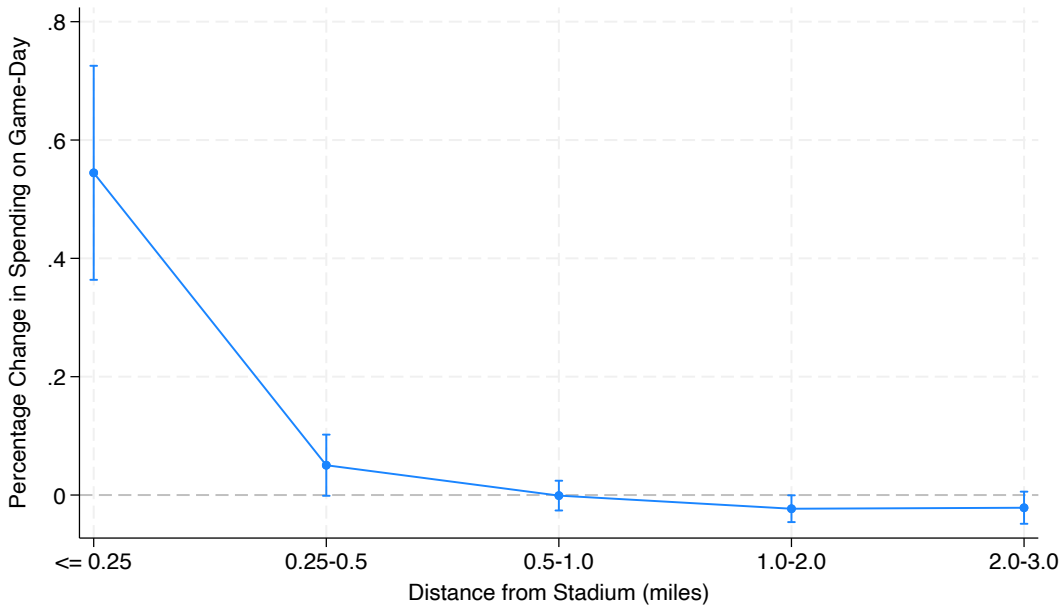
Table 2: Regular-Season Game Effects

	Spending	Transactions	Spending per Transaction
Regular-season event	0.0095***	0.0086***	0.0009
	(0.0020)	(0.0020)	(0.0010)
Observations	154,974	154,974	154,974
R^2	0.985	0.986	0.853

Each column reports the coefficient on a regular-season game from equation (18). All specifications include weather controls, venue-weekday, state-date, and venue-month fixed effects. The standard errors, given in parentheses, are clustered by venue and date.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

magnitude is modest. The results show that a regular season game increases total spending in the 3-mile area around the stadium by 0.95%. If I scale the panel by the state-month expansion, this is roughly a mean increase of $26,042 and a median increase of $16,482. The other results show the increase in spending is mainly due to an increase in transactions, as transactions increase by 0.86% while spending per transaction changes by 0.09% and is not statistically distinguishable from zero.

Figure 2: Regular-Season Game Effects by Distance from the Venue



The small total effect conceals a steep spatial gradient. Figure 2 plots the estimated ring coefficients from equation (19) and Table 3 reports the corresponding estimates for all three outcomes. They show that spending increases by 54.5% within a quarter mile of the venue (with a 95% confidence interval of 36.4% to 72.5%). This amounts to a population-scaled mean increase of about $22,905 and a median increase of

Table 3: Event Effects by Distance

	Spending	Transactions	Spending per Transaction
0–0.25 miles	0.5446***	0.3704***	0.0204
	(0.0923)	(0.0581)	(0.0167)
0.25–0.5 miles	0.0504*	0.0253	0.0068
	(0.0264)	(0.0194)	(0.0073)
0.5–1 mile	−0.0010	−0.0019	0.0055
	(0.0128)	(0.0122)	(0.0049)
1–2 miles	−0.0231**	−0.0191*	0.0042
	(0.0116)	(0.0107)	(0.0041)
2–3 miles	−0.0215	−0.0216	0.0082**
	(0.0139)	(0.0131)	(0.0037)
Observations	759,843	759,843	728,234
R^2	0.898	0.940	0.499

Each column reports average marginal effects from estimating equation (19). All specifications include weather controls, venue-ring-by-weekday, state-by-date, and venue-by-month fixed effects. The standard errors, given in parentheses, are clustered by venue and date.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

$8,415. For the ring that is 0.25-0.5 miles from the stadium, the estimate falls to 5.04% and is significant only at the 10% level. The estimated effect is essentially zero between 0.5-1 mile, and then becomes negative in further distance rings.

These results suggest that a game produces a sharp increase in spending at businesses that are nearest to the venue, a much smaller response over the next quarter-mile, and little to modestly negative activity farther away. The negative outer-ring estimates are consistent with equation (6). As attendee exposure declines with distance, event-generated spending can become too small to offset displaced ordinary activity.

5.1 Attendance

As for attendance, Table 4 shows that crowd size affects the magnitude of the spending response immediately surrounding the venue, but much less so as you get further away. Within one-quarter mile, the coefficient on centered log attendance is 0.475, implying that a 10% increase in attendance relative to the venue's typical crowd is associated with a 4.75% larger game-day spending response. Consistent with this, the estimated inner ring effect rises from 51.3% at the 25th percentile of attendance to 54.6% at the median and 58.1% at the 75th percentile. By contrast, the attendance gradient is close to zero in the second ring and remains small and statistically indistinguishable from zero at greater distances. Larger crowds therefore appear to amplify the near-venue demand shock rather than broaden it spatially.

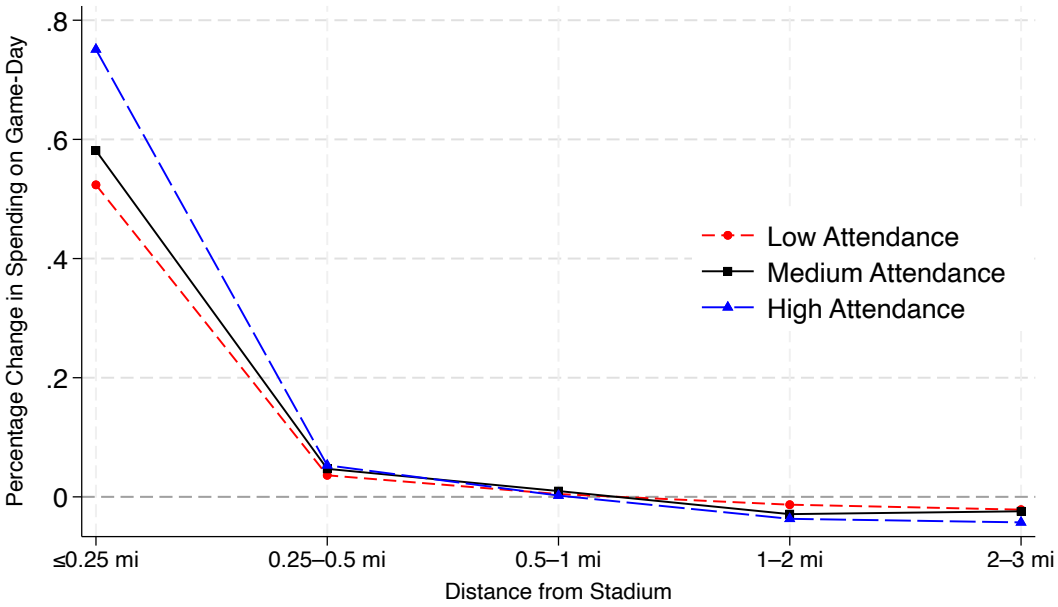
I also break down games into attendance categories based on whether the game was in the bottom third, medium third, or top third of attended games at the venue for that season. I then run a regression that

Table 4: Event Effects by Attendance

	Attendance	Event Effect at Attendance Percentile		
Distance from Venue	Gradient	25th	Median	75th
0–0.25 miles	0.475***	0.513***	0.546***	0.581***
	(0.162)	(0.087)	(0.092)	(0.098)
0.25–0.5 miles	0.029	0.048*	0.050*	0.052*
	(0.081)	(0.026)	(0.027)	(0.029)
0.5–1 mile	−0.021	0.001	−0.001	−0.002
	(0.049)	(0.013)	(0.013)	(0.014)
1–2 miles	−0.065	−0.019*	−0.023**	−0.028**
	(0.043)	(0.011)	(0.012)	(0.013)
2–3 miles	−0.050	−0.018	−0.021	−0.025*
	(0.031)	(0.013)	(0.014)	(0.014)

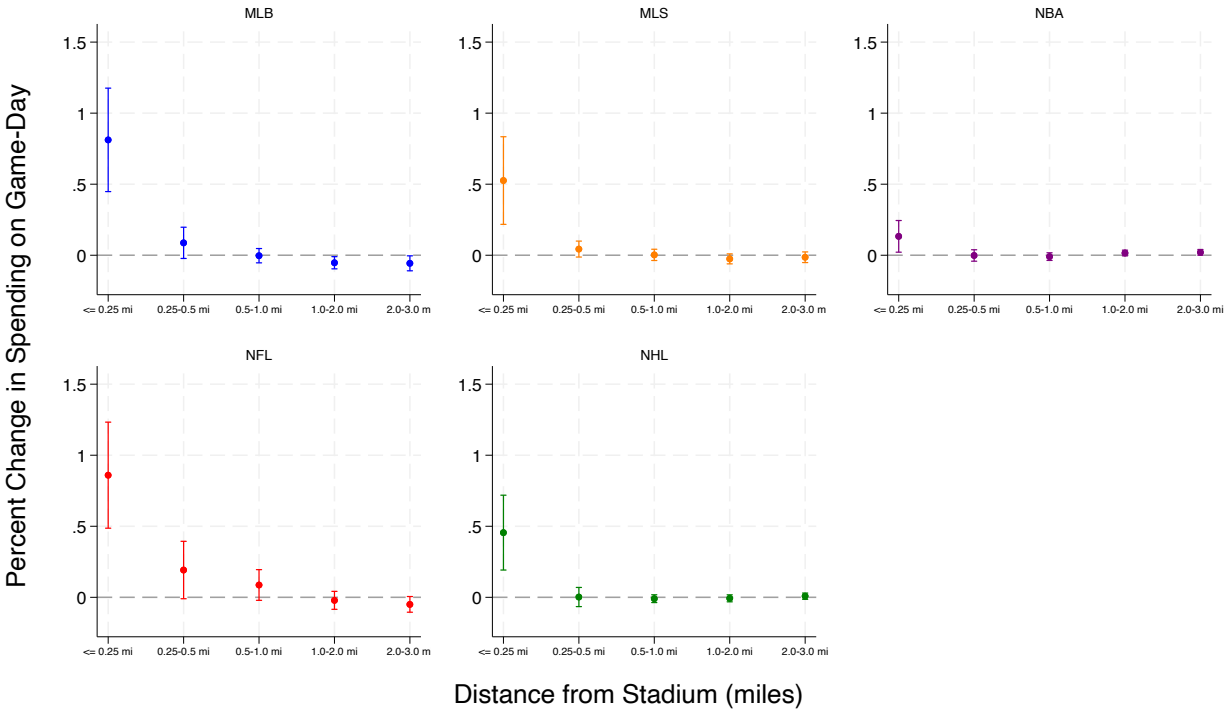
Column 1 reports the ring-specific coefficient on centered log attendance. Columns 2–4 report the estimated effect on spending evaluated at the 25th percentile, median, and 75th percentile of within-venue attendance. All specifications include weather controls, venue-ring-by-weekday, state-by-date, and venue-by-month fixed effects. The standard errors, given in parentheses, are clustered by venue and date.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 3: Game-Day Effect by Attendance



interacts the ring-game-day interaction with these categories. The results are displayed in Figure 3. They show that high attendance games increase spending in the closest ring by 75.1% compared to a 52.4% increase for the lowest attended games.

Figure 4: Game-Day Effect by League



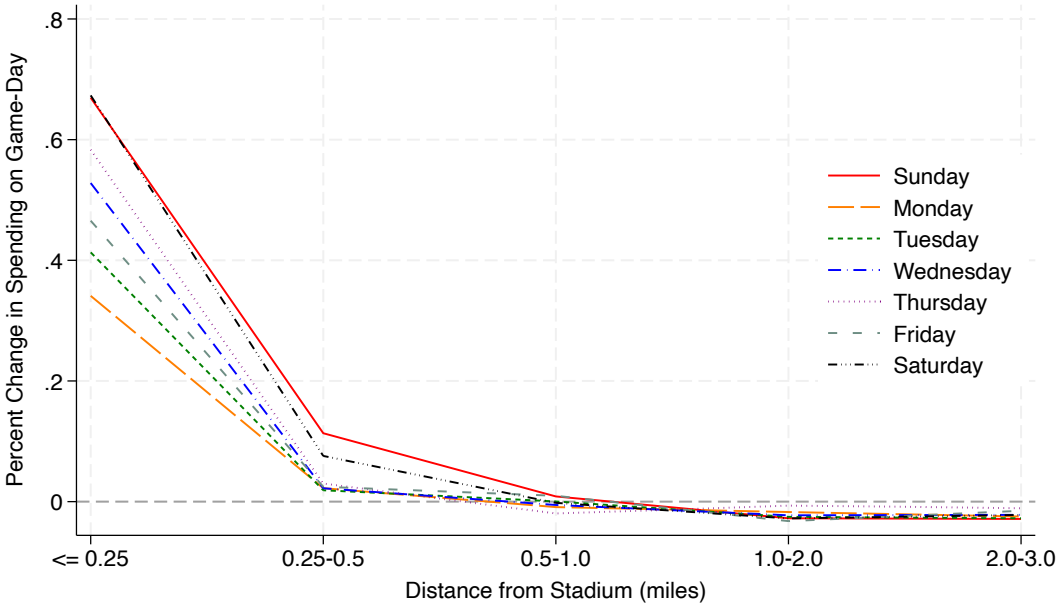
5.2 Which Events Have the Largest Effect?

I also look at how the game-day spending effect varies across the five leagues. Figure 4 shows the results broken down by league. It shows that each league produces a positive inner ring effect, but the magnitudes differ substantially. NFL games have the largest effect with an 86.0% increase in the inner ring on game-days, followed by MLB at 81.2%. The effects are much smaller for the other three leagues with an estimated inner ring effect of 52.6% for MLS, 45.4% for the NHL, and 13.3% for NBA games. NFL games also have the broadest spatial profile. Spending increases by 19.2% in ring 2 and 8.67% in ring 3, although these estimates are only significant at the 10% level. MLB spending is 8.73% higher in ring 2, but returns to zero in ring 3 and falls by about 5.5% in each outer ring. MLS, NBA, and NHL estimates are close to zero beyond the first quarter mile.

The differences across leagues likely reflect both fan behavior and the environments surrounding different types of venues. NFL and MLB games often generate substantial pre- and post-game activity, with fans arriving early or remaining nearby after the event and spending at restaurants, bars, and other businesses close to the stadium. NBA and NHL arenas, by contrast, tend to be smaller and are typically located in the dense downtown cores where many fans already reside or work, implying less incremental demand relative to baseline downtown activity. While MLS games generally draw smaller crowds, their stadiums are more likely

than their counterparts to be in suburban neighborhoods with less economic activity on non-game days.

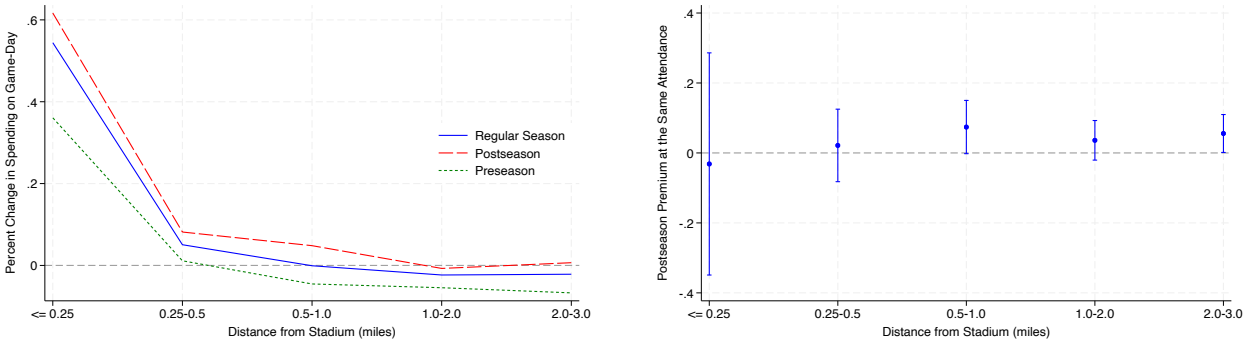
Figure 5: Game-Day Effect by Day of Week



The day of the week of the game can also matter even after controlling for each venue-ring's normal weekday patterns. Thus, I also run a specification that looks at heterogeneity across the day of the week. Figure 5 shows that the near-venue spending response is present throughout the week but does vary somewhat by day of the week. The Ring 1 effect is largest on Saturday and Sunday, at roughly 67%, compared with about 34% on Monday and between 41% and 58% on Tuesday through Friday. Because the specification absorbs venue-by-ring-by-day-of-week fixed effects, these differences do not simply reflect higher baseline spending on weekends. Rather, they compare game days with the usual spending level for the same venue, ring, and weekday. One possible explanation is that weekend games give attendees more time for pregame and postgame consumption, while also making it easier for non-attendees to travel to the stadium district to check out restaurants, bars, or other event-related activity. At the same time, part of the weekend pattern reflects league composition, particularly the concentration of NFL games on Sundays.

Figure 6 examines whether the magnitude of the local spending response varies between regular and postseason games. Panel (a) shows a clear ordering near the venue. Spending within one-quarter mile increases by approximately 62% for postseason games, compared with 54% for regular-season games and 36% for preseason games. The postseason response also appears somewhat more persistent spatially, remaining positive through the next two distance rings, whereas the preseason effect falls rapidly and becomes negative farther from the venue. This broader spatial footprint may reflect behavior beyond the ticketed crowd

Figure 6: Game-Day Spending Effects by Game Type and Attendance



(a) Effects by season type

(b) Postseason premium conditional on attendance

Panel (a) reports the estimated game-day spending effect for regular-season, postseason, and preseason games by distance from the venue. Panel (b) reports the postseason-minus-regular-season difference evaluated at the same centered attendance level. Vertical lines denote 95% confidence intervals. All specifications include weather controls, venue-ring-by-weekday, state-by-date, and venue-by-month fixed effects. Standard errors are clustered by venue and date.

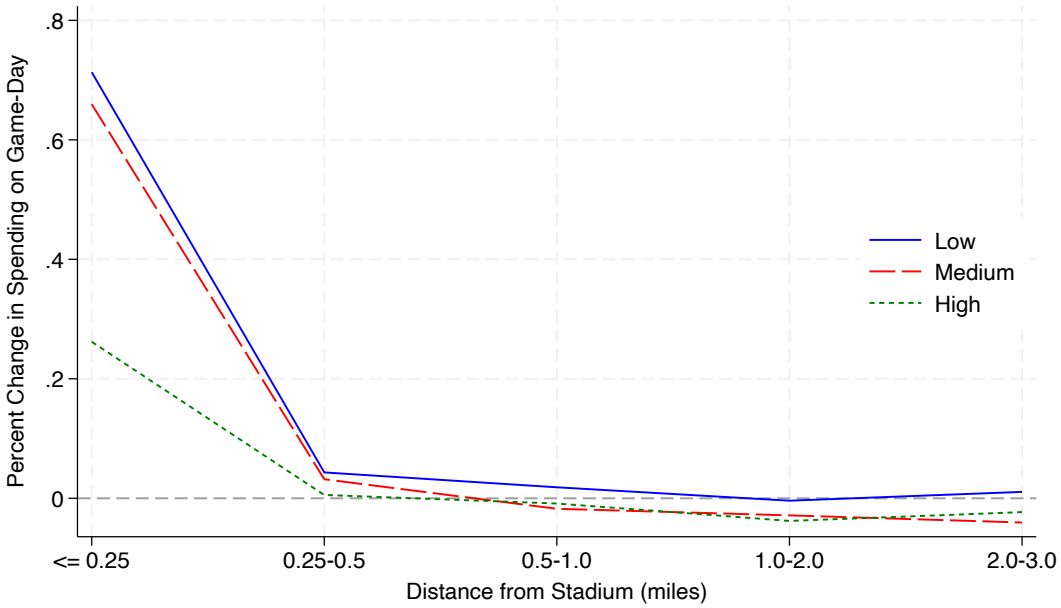
itself. Postseason games can draw non-attendees toward the stadium district (to watch at nearby bars and restaurants, participate in pregame or postgame activity, or simply be near the event), so some of the additional spending may occur at greater distances even among consumers who do not enter the venue.

Panel (b) separates the postseason comparison from one of its most important correlates by allowing the spending response to vary with attendance. Once attendance is held fixed, the apparent postseason premium disappears in the immediate stadium area. Within one-quarter mile, the postseason-minus-regular-season difference is -3.1 percentage points. The confidence interval is wide and comfortably includes zero. By contrast, the postseason-minus-regular-season differences are positive in several outer rings, including statistically significant estimates farther from the venue. This pattern suggests that the larger unconditional Ring 1 effect for postseason games is largely associated with the larger crowds they attract, but that postseason games are more likely to increase spending in outer areas due to the excitement they create in the city, even among non-attendees.

Figure 7 examines whether the game-day response depends on the amount of commercial activity normally present around the venue. I classify venues using average spending within 0.25 miles on non-game days during the sample period. Figure 7 shows that the proportional response is substantially larger around venues with less baseline commercial activity. Within one-quarter mile, game-day spending increases by approximately 71% in low-activity districts and 66% in medium-activity districts, compared with 26% in high-activity districts. The difference attenuates rapidly with distance as effects are small across all three groups beyond the first quarter mile, and the medium- and high-activity groups exhibit modest negative estimates in the outer rings.

This pattern is consistent with the commercial-activity threshold in Proposition 1. In the model, a stadium

Figure 7: Game-Day Effect by Baseline Commercial Activity



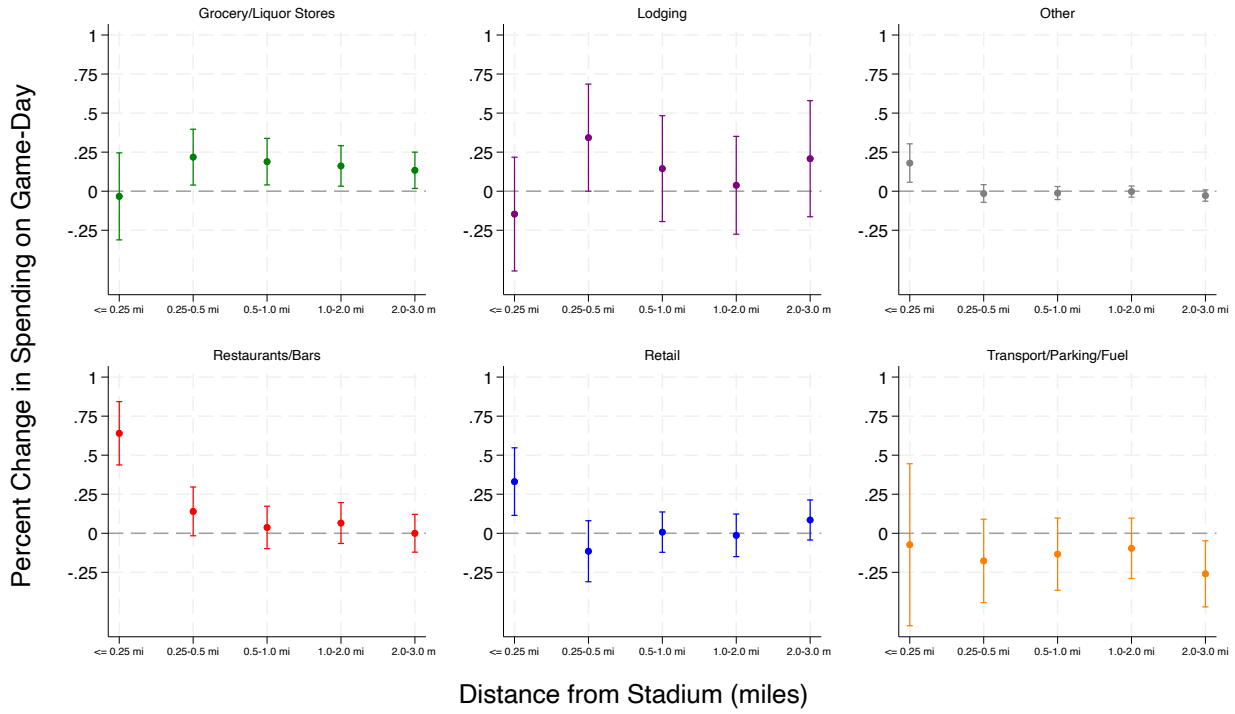
district with little ordinary commercial activity has relatively little baseline spending available to be displaced, so event-generated demand is more likely to produce a positive net increase. In a commercially dense district, the same influx of event consumers interacts with a much larger amount of spending that would have occurred even without the game. The smaller proportional effect in high-activity markets therefore reflects more than a mechanical difference in baseline scale as it is also consistent with a greater role for ordinary commercial activity in offsetting gross event demand.

5.3 Which Businesses Are Most Affected?

Merchant category provides the clearest test of whether the game-day shock changes the composition of local demand. Figure 8 shows substantial heterogeneity across categories in both the magnitude and spatial distribution of the response. Within one-quarter mile of the venue, spending at restaurants and bars increases by approximately 64%, and retail spending rises by 33%. In contrast, the corresponding estimates for grocery and liquor stores, lodging, and transportation, parking, and fuel are small and statistically indistinguishable from zero. In terms of Proposition 2, these patterns indicate that game attendees allocate spending differently from the consumers normally patronizing the stadium district.

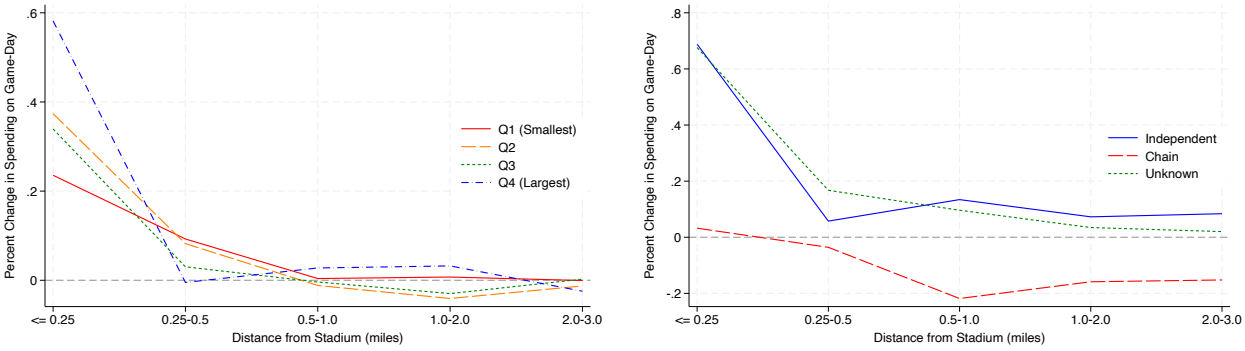
The spatial patterns also differ across categories. Restaurant and bar spending remains about 14% higher in the 0.25–0.5 mile ring but is close to zero beyond that distance. Grocery and liquor store spending, by contrast, is unchanged immediately adjacent to the stadium but increases by roughly 13%–22% in the outer

Figure 8: Game-Day Effect by Business Category



rings. Lodging also peaks outside the first ring, with a 34% estimated effect between 0.25 and 0.5 miles, although the confidence interval is wide. Transportation, parking, and fuel spending is negative throughout most of the catchment and falls by about 26% in the outermost ring. These differences are consistent with distinct spatial complementarities across sectors. Food, drink, and event-related retail are most valuable in the immediate vicinity of the venue, whereas purchases such as groceries or lodging need not occur at the stadium entrance itself and may instead take place along the broader trip to the event or in nearby commercial areas.

Figure 9: Game-Day Effects by Merchant Characteristics



(a) Merchant Size

(b) Chain Status

Figure 9 graphs the results of regressions that interact the game-day indicator with the size classification and the chain classification. The size classification places firms into quartiles with Q1 containing the smallest establishments and Q4 the largest. Merchants are then classified as chains if their brand is observed at two or more locations and independent if they have no reported brand affiliation.

Panel (a) shows that the spending response is stronger for the largest establishments. Within one-quarter mile of the venue, spending increases by roughly 23% for merchants in the smallest quartile, 37% in the second quartile, 35% in the third quartile, and 58% among the largest merchants. The largest establishments therefore exhibit the strongest proportional response, but meaningful increases occur throughout the size distribution. The spatial pattern also differs somewhat by size. For the largest merchants, the effect is almost entirely concentrated within one-quarter mile, whereas smaller establishments continue to exhibit modest positive estimates in the 0.25–0.5 mile ring. Beyond one-half mile, the effects for all four size groups are generally small.

Panel (b) reveals considerably sharper heterogeneity by chain status. Spending at independent establishments rises by approximately 69% within one-quarter mile of the venue, while the corresponding estimate for chains is close to zero. The unclassified group exhibits a similarly large near-venue response of approximately 68%. The divergence persists outside the first ring as spending at independent establishments remains modestly positive across much of the catchment, whereas the chain estimates become negative beyond one-quarter mile. These patterns suggest that the immediate game-day demand shock is captured disproportionately by non-chain establishments. Taken together, these results imply that the game-day shock is concentrated not only in complementary sectors such as restaurants and retail, but also disproportionately among the largest nearby merchants and non-chain establishments, while still reaching businesses throughout the size distribution.

5.4 Robustness Exercises

The main identifying assumption of this paper could fail if game dates coincide with unusual calendar patterns, if nearby venues contaminate treated or comparison dates, or if the estimated response reflects spurious correlation rather than spending associated with actual games. Table 5 organizes the main robustness checks around these concerns. Across the non-placebo specifications, the Ring 1 estimates range from 0.489 to 0.593, compared with 0.545 in the preferred specification. Thus, none of the alternative samples or fixed-effect structures materially change the magnitude of the near-venue response.

I first examine whether the result is sensitive to recurring calendar patterns, the specification of the outcome, and the choice of comparison dates. Professional sports schedules are highly structured across both

Table 5: Robustness of the Ring 1 Spending Effect

Specification	Ring 1 Effect	Standard Error	Observations
Preferred baseline	0.5446***	0.0923	759,843
Venue-ring-month-weekday fixed effects	0.4894***	0.0780	759,784
City-by-date fixed effects	0.5365***	0.0940	759,843
State-date cells with treated and untreated venues	0.5890***	0.1090	396,154
Matched comparison dates	0.5568***	0.0889	110,865
Exclude adjacent event dates from controls	0.5926***	0.0990	674,065
Exclude multiple events at focal venue	0.5428***	0.0910	758,709
Exclude another event within 3 miles	0.5588***	0.0920	664,354
Control for another event within 3 miles	0.5687***	0.0930	759,843
Exclude another event within 6 miles	0.5418***	0.0820	643,065

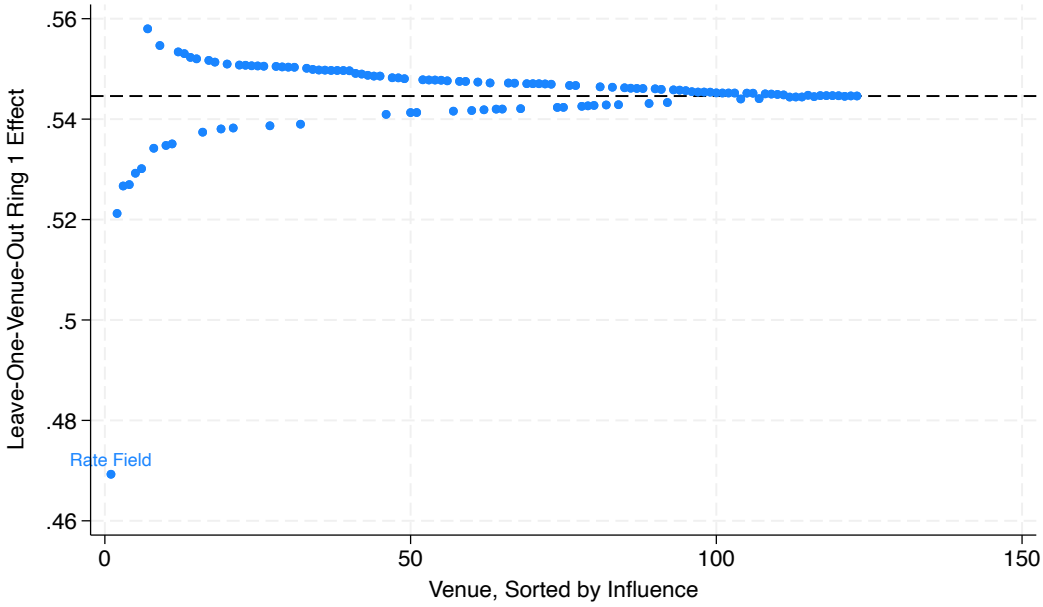
The table reports the Ring 1 spending coefficient from each specification. Unless otherwise specified, regressions include weather controls, venue-ring-by-weekday, state-by-date, and venue-by-month fixed effects. The city-by-date specification replaces state-by-date fixed effects with city-by-date fixed effects, the richer fixed-effects specification absorbs venue-ring-by-month-by-weekday effects, and the matched specification includes match-by-ring fixed effects. The standard errors are clustered by venue and date.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

the week and the season. NFL games, for example, are concentrated on Sundays, while baseball schedules vary systematically across the summer months. If spending around a venue has its own month-specific weekday pattern, the baseline venue-ring-by-weekday and venue-by-month fixed effects may not fully absorb these recurring differences. I therefore estimate a more demanding specification with venue-by-ring-by-month-by-weekday fixed effects, which compares game and non-game dates within the same venue, distance ring, calendar month, and day of the week. This substantially narrows the identifying variation while allowing each part of the stadium catchment to have its own weekly spending pattern in every month. The inner ring estimate falls from 54.5% in the preferred specification to 48.9%, but remains economically large and precisely estimated. Thus, recurring venue-specific scheduling and seasonal spending patterns account for some of the baseline estimate, but they do not explain the sharp increase in spending immediately surrounding the venue.

While the baseline specification takes into account demand conditions common to venues within the same state and date, this may still leave more localized shocks that vary across cities. I therefore run a test where I replace the state-by-date fixed effects with city-by-date fixed effects, which absorb any same-day shock shared by venues in the same city, including local events, disruptions, or other changes in economic activity. The Ring 1 estimate is 53.7%, essentially unchanged from the preferred estimate of 54.5%. The small difference is not especially surprising because many states contain only one city with a professional sports venue, so the state-by-date and city-by-date controls are effectively similar in those cases. The specification is most informative in states with venues in multiple cities, where it removes within-state differences in local economic conditions. The stability of the estimate therefore provides some reassurance that the baseline result is not driven by city-specific same-day shocks in those multi-city states.

As a further check on the source of identifying variation, I restrict the sample to state-date cells containing both a venue hosting a regular-season game and a venue without an event. This provides a particularly transparent subset in which treated and untreated venues are observed contemporaneously within the same state. The resulting Ring 1 estimate is 0.589, compared with 0.545 in the preferred specification. The corresponding aggregate spending effect is 1.18%, compared with 0.95% in the baseline. Thus, restricting the analysis to state-dates with direct treatment overlap produces very similar conclusions.

Figure 10: Leave One Venue Out Ring 1 Spending Effects



I also examine whether the near-venue effect is driven by any individual stadium. Figure 10 re-estimates the baseline Ring 1 specification 123 times, each time excluding one venue. The estimates are generally tightly concentrated around the full-sample coefficient of 0.545. The largest change occurs when Rate Field is excluded, which lowers the estimated effect to 0.469. This likely reflects an unusually large game-day spending response around the venue rather than disproportionate weight in the regression, since no individual venue accounts for more than 2% of the identifying weight (according to Appendix Table 6). Rate Field is also an MLB venue with a large number of regular-season home dates, and game-day demand may represent a particularly large proportional shock relative to ordinary commercial activity immediately surrounding the stadium. The next-largest changes are considerably smaller, and the remaining estimates converge rapidly toward the full-sample coefficient. Together with the dispersed identifying weights in Appendix Table 6, this exercise indicates that the near-venue effect is a broad feature of the sample rather than the product of a small number of stadiums.

For another robustness check, as an alternative to using the full set of non-game dates as controls, I construct a matched sample in which each regular-season game is compared with non-event dates at the same venue that fall on the same weekday, in the same calendar month, and in the same year. This restricts the comparison group to dates that are close to the event in both season and weekly spending patterns. The resulting Ring 1 estimate is 55.7%, nearly identical to the preferred estimate.

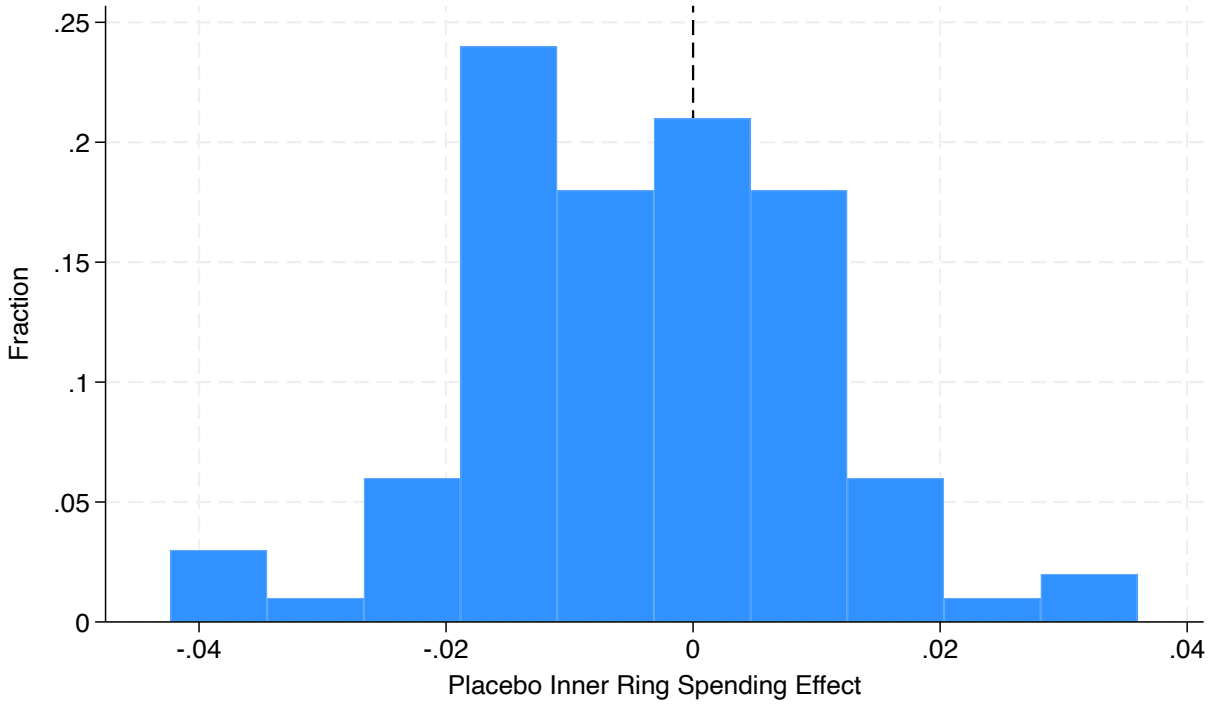
Non-event dates immediately before or after a game may be imperfect counterfactuals if event-related spending spills across calendar days. This could occur because visitors remain in the stadium area before or after the event, or because some card transactions are posted with a short delay. I therefore re-estimate the model after removing non-event dates adjacent to an event from the comparison sample. According to Table 5, the Ring 1 estimate rises from 54.5% to 59.3%. Thus, adjacent non-event dates do not generate the baseline result, and, if anything, including them attenuates the estimated game-day effect.

A second set of concerns is over treatment interference arising from simultaneous events at the focal venue or at nearby sports venues. Shared sports districts can place the same merchants within the catchments of multiple venues, so an event at a nearby stadium may affect spending attributed to the focal venue. The first check is to exclude dates with multiple events at the focal venue. Table 5 shows that this has almost no effect, changing the inner ring coefficient from 54.5% to 54.3%.

Another potential concern in this vein is that spending generated by an event at a nearby venue could be incorrectly attributed to the focal stadium when the two catchment areas overlap. I therefore drop every focal venue-date on which another professional sports venue within three miles also hosts an event. This restriction removes roughly 95,000 venue-ring observations, but the Ring 1 estimate increases slightly to 55.9%. The corresponding venue-day specification also continues to show a positive aggregate effect within three miles, with spending increasing by 1.36% compared with 0.95% in the preferred baseline, while spending per transaction remains essentially unchanged. These results indicate that the near-venue and broader catchment effects are not driven by contamination from events at neighboring venues.

In another robustness check, rather than dropping dates with overlapping nearby events, I first retain the full sample and include an indicator for whether another professional sports venue within three miles is hosting an event on the same date. This specification allows nearby events to have their own effect on local spending while separately identifying the effect of an event at the focal venue. The estimated Ring 1 own-event effect is 56.9%, again very close to the preferred estimate. The nearby-event indicator is itself positive, consistent with spillovers across overlapping sports districts. As a further check, I broaden the exclusion radius and drop focal venue-dates with another event within six miles. The Ring 1 estimate is 54.2%. Together, these results show that overlapping event markets matter for local spending, but they do not account for the main near-venue game-day response.

Figure 11: Distribution of Placebo Ring 1 Spending Effects



As a final robustness check, I perform a falsification exercise that tests whether the preferred specification produces similar effects on dates that resemble game dates in their scheduling patterns but on which no game actually occurred. Within each venue, year, and day-of-week cell, I randomly assign the same number of placebo game dates as actual regular-season games to dates on which the focal venue hosted no event. I then re-estimate the baseline inner ring specification for each of the 100 placebo schedules. Figure 11 shows that the resulting estimates are centered near zero. The mean placebo coefficient is -0.0043 and the median is -0.0040. The 95th percentile is 0.018 and the largest placebo estimate is 0.036, compared with the preferred Ring 1 estimate of 0.545. The placebo distribution also contains both positive and negative estimates. Thus, the large near-venue spending response is not a mechanical consequence of the scheduling structure or the fixed-effects specification.

6 Conclusion

This paper uses daily transaction data around 123 professional sports venues to measure both the net local spending response to a game and the incidence of that response across space and businesses. Regular-season games increase spending within three miles of a venue by approximately 0.95% and transactions by 0.86%, with essentially no change in spending per transaction. Games therefore generate a measurable increase in

economic activity within the surrounding market, but the average effect across the full catchment is modest.

The spatial distribution of this response is considerably more pronounced. Spending rises by approximately 54.5% within one-quarter mile of the venue and by about 5% over the next quarter mile, but changes little or declines modestly farther away. The largest near-venue increases occur at restaurants and bars and at retail establishments, while grocery, lodging, and transportation-related businesses show little immediate response. Larger crowds generate larger spending responses immediately surrounding the venue without producing a comparable expansion of the effect farther away. Differences across leagues, season type, day of week, and baseline commercial activity similarly show that the magnitude of the shock depends on both the event and the surrounding commercial environment.

Taken together, these patterns are consistent with viewing a sporting event as a temporary consumption anchor. Games generate the largest net increases where event demand is large relative to ordinary commercial activity, while in established commercial districts more of the spending associated with the event can be offset by activity that would otherwise have occurred. These results point to a distinction that is easy to miss in discussions of the economic effects of professional sports. Sporting events do generate additional spending in the market surrounding a venue, but the much larger effect is in how sharply that activity is concentrated in space. The large crowds visible outside a stadium therefore reflect a combination of modest net local demand creation and substantial redistribution of spending toward businesses positioned to serve event-related consumption. This helps reconcile the large effects often observed immediately around sports venues with the much smaller aggregate effects found in the broader literature.

This distinction is important for evaluating claims about the economic benefits of stadiums. A large increase in spending immediately outside a venue should not be interpreted as an equally large increase in economic activity for the surrounding market. At the same time, a small aggregate effect does not imply that the local incidence is unimportant, as restaurants, bars, retailers, and other businesses close to the venue can experience substantial gains on game days, and those gains are distributed unevenly across establishments. Stadium policy therefore involves questions of incidence as well as aggregate effects. The relevant economic question is not simply whether a venue generates spending, but how much of that spending is additional, where it occurs, and which businesses capture it. The results here speak directly to the short-run consumer-spending channel, but they do not provide a complete welfare analysis of stadium investment. They do not measure benefits to fans, changes in land values, fiscal revenues, construction effects, or longer-run changes in the location of businesses and economic activity.

These limitations also suggest several directions for future research. One is to connect the short-run spending responses documented here to longer-run changes in commercial development, business entry and survival, property values, and local tax revenues. Another is to examine the origins of game-day spending

more directly. Distinguishing spending by local residents from that of visitors would help determine how much of the measured increase represents new expenditure entering the local economy rather than changes in when and where residents spend. Future work could also examine how stadium design, transportation access, surrounding land use, and the composition of nearby businesses shape the ability of stadium districts to capture event-related demand. More broadly, the same framework can be applied to other temporary urban demand shocks, including concerts, conventions, festivals, and other large events, where highly visible local activity may similarly differ from the net effect on the surrounding market.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work, the author used ChatGPT to proofread the manuscript for clarity, grammar, and presentation. The author reviewed and edited all suggested output as needed and takes full responsibility for the content of the published article.

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A Sources of Identifying Variation

Table 6: Sources of Identifying Variation

	Aggregate Effect	Ring 1 Effect
Observations	154,974	759,843
Venues contributing	122	119
States contributing	28	28
Dates contributing	1,283	1,281
Regular-season game venue-dates	18,324	16,413
Share of treated coefficient weight by league		
MLB	40.8%	40.9%
MLS	10.1%	8.7%
NBA	22.5%	25.2%
NFL	5.8%	4.4%
NHL	20.5%	20.6%
Concentration of treated coefficient weight across venues		
Largest venue	1.8%	1.9%
Largest 5 venues	8.6%	9.0%
Largest 10 venues	16.9%	17.7%

The table describes the identifying support for the preferred aggregate and Ring 1 spending specifications. For the aggregate specification, the game-day indicator is residualized with respect to the weather controls, venue-by-day-of-week, state-by-date, and venue-by-month fixed effects. For the Ring 1 specification, the Ring 1 game-day indicator is residualized with respect to the same controls, the venue-by-ring-by-day-of-week fixed effects, and the game-day treatment indicators for the other four rings. Coefficient weights are the normalized Frisch–Waugh–Lovell weights among treated observations and sum to one across treated observations. "Contributing" venues, states, and dates are those containing treated observations with nonzero residualized treatment. Rare game dates involving multiple leagues account for the remaining 0.2–0.3% of treatment weight and are not reported separately.

The fixed effects in the preferred specifications substantially restrict the variation used to estimate the game-day coefficients. This appendix makes that variation explicit. For each coefficient, I use the Frisch–Waugh–Lovell representation of the regression to partial out the corresponding treatment variable with

respect to the controls and fixed effects in the preferred specification. For the Ring 1 coefficient, I additionally partial out the treatment indicators for the other four distance rings. I then use the resulting residualized treatment variable to calculate normalized coefficient weights across treated observations. These weights describe the observations that identify the coefficient.

Table 6 shows that the preferred estimates draw on broad support across venues and leagues. The aggregate coefficient is identified from treated observations spanning 122 venues, 28 states, and 1,283 calendar dates, while the corresponding Ring 1 coefficient draws on 119 venues in the same 28 states. MLB games provide the largest single share of identifying weight, at approximately 41%, but nearly 60% comes from the other four leagues. More importantly, the identifying weight is not concentrated in a small number of venues. The largest individual venue contributes only 1.8% of the aggregate weight and 1.9% of the Ring 1 weight. Even the ten largest contributors account for only 16.9% and 17.7%, respectively. Thus, the headline estimates reflect variation across a broad set of professional sports markets rather than being driven mechanically by a handful of heavily weighted venues.