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Assessing the Economic Impact of College Football Games on Local Economies

Robert A. Baade
Lake Forest College
Robert W. Baumann
Victor A. Matheson
College of the Holy Cross

This article provides an empirical examination of the economic impact of spectator sports on local economies. Confirming the results of other ex post analyses of sports in general, this article finds no statistically significant evidence that college football games in particular contribute positively to a host's economy. Our analysis from 1970 to 2004 of 63 metropolitan areas that played host to big-time college football programs finds that neither the number of home games played, the winning percentage of the local team, nor winning a national championship has a discernable impact on either employment or personal income in the cities where the teams play. An examination of a subset of 42 smaller college towns finds that winning seasons actually reduce the growth rate of per capita personal income. Although successful college football teams may bring fame to their home towns, fortune appears to be a bit more elusive.

Keywords: *sports; football; college sports; impact analysis; mega-event*

College football is among the most popular spectator sports in the United States. Total live attendance at all college football games in 2006 was nearly 48 million fans, which is more than double the attendance of the National Football League (NFL), National Basketball Association (NBA), or National Hockey League (NHL) during recent seasons. Average attendance among the 119 National Intercollegiate Athletic Association (NCAA) Division 1-A football teams, the highest level of collegiate play, totaled more than 46,000 fans per game in 2006 and several teams routinely attract more than 100,000 fans per home game. Outside of

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auto racing and a small handful of golfing events, individual college football games at places like the University of Michigan and the Ohio State University have the largest live paid attendance of any sporting events in the country. Television ratings are equally impressive. The Bowl Championship Series (BCS) championship game is the second-most watched sporting event in the country every year (behind the NFL's Super Bowl) and typically draws a television audience nearly double the size of games during the National Basketball Association finals or baseball's World Series.

The popularity of the sport has led colleges, universities, and occasionally local communities to invest generously in infrastructure for their teams. In 2009, for example, the University of Minnesota will open a new US$288 million stadium, 55% of which was paid for with state funds (although the precise line between what constitutes state and university funds is admittedly unclear for a public university). In similar fashion to the major professional sports leagues in the Unites States, many colleges and universities have also upgraded their playing facilities over the past 15 years adding to both the number and quality of seats. It is not unusual for top programs to have a significant number of high-priced luxury boxes, a far cry from the simple bleachers of yesteryear. As a case in point, the University of Michigan's recent US$226 million stadium renovation plan includes replacing some existing stadium bleachers with 83 suites and 3,200 club seats.

Numerous articles have explored the indirect economic impact of college football and college football success on measures such as applications (McCormick & Tinsley, 1987; Murphy & Trandel, 1994; Toma & Cross, 1998; Tucker, 2005; Tucker & Amato, 1993), graduation rates (Amato, Gandar, Tucker, & Zuber, 1996; Amato, Gandar, & Zuber, 2001; Rishe, 2003; Tucker, 1992, 2004), and alumni giving (Baade & Sundberg, 1994; Grimes & Chressanthis, 1994; Rhoads & Gerking, 2000; Siegelman & Bookheimer, 1983; Siegelman & Carter, 1979), generally reporting minor or mixed effects from athletic success.

Although these type of articles examine the effects of college athletics on their host institutions, this article instead examines the effects of college football on more direct economic indicators such as employment and personal income and focuses on a team's impact on the host community rather than the college or university for which it plays. The results of this article suggest that college football games, as well as a team's success on the gridiron, have a negligible impact on real economic variables in host cities.

Background

Economic impact analyses are divided into two main categories: ex ante studies and ex post studies. Ex ante studies predict the economic effect of an event by estimating the number of visitors to the event as well as their average expenditures. A multiplier is typically also applied to these direct economic impact figures resulting in a total impact number that is often at least twice as large as the direct economic impact. Ex ante studies of college athletics routine ascribe large benefits to major athletic programs. For example, the University of Nebraska estimated that during the 2004-2005 school year, its football program generated US$87.1 million in output, US$31.2 million in worker income, and 2,130 jobs in the Lincoln area. Estimated statewide economic impact of University of Nebraska athletics ranged from US$48 to $155 million (Thompson, 2005). A similar study at the Ohio State University found an estimated US$100 million impact from its athletic program in 2002-2003 (Ohio State University, 2005), whereas football specific studies at Florida State and Pennsylvania State arrived at economic impacts of US$56.2 million and US$40.3 million, respectively (Pearson, 2001). Of course, not every estimate is as optimistic. Pearson (2001) estimated a mere US$2.9 million increase in income in the local economy as a result of the North Carolina State football program. The wide range in economic impact estimates illustrates the ad hoc nature of the assumptions often made in economic impact studies.

Critics of ex ante economic analysis point out that these studies often fail to account for the substitution effect which occurs when fans spend money at a sporting event rather than at other venues in the local economy. For example, in arriving at the upper bound for the impact of University of Nebraska athletics on the state's economy, the study's author explicitly assumes that all spending by state residents on University athletics would otherwise have been spent out of state (Thompson, 2005). By assuming that the substitution effect simply does not exist, the study significantly exaggerates the true economic impact of the athletic program.

In addition, ex ante studies have difficulty accounting for the crowding out that occurs when the crowds and congestion associated with large sporting events deters nonsports fans from spending money in the local economy during game days. Although hotels, restaurants, and t-shirt sellers may do well on game day, other retailers and service providers may not benefit from the big game and potentially could lose sales.

Finally, many economists are skeptical of the multipliers used in ex ante studies to generate indirect economic benefits. Often the multipliers used are absurdly high, but even more careful estimates of multipliers may be suspicious. Multipliers are calculated using complex input–output tables for specific industries grounded in interindustry relationships within regions based on an economic area's normal production patterns. During game days, however, the economy within a college town may be anything but normal, and therefore these same interindustry relationships may not hold. As there is no reason to believe the usual economic multipliers apply during major events, any economic analyses based on these multipliers may, therefore, be highly inaccurate (Matheson, 2004).

Due to the difficulties associated with ex ante estimation, numerous scholars estimate the effects of stadiums, franchises, and sporting events on local economies by ex post estimation, which examines the actual economic performance of local areas that host franchises or large events. Most ex post studies, including Baade (1996), Coates and Humphreys (1999, 2003), and Baade, Baumann, and Matheson (2008) to name just a few, generally find little or no economic benefits from professional sports teams or new playing facilities. Similarly, ex post examinations of major sporting events such as the Super Bowl (Baade & Matheson, 2006; Coates, 2006; Porter, 1999), All-Star Games (Baade & Matheson, 2001; Coates, 2006) and postseason play (Baade et al., 2008; Coates & Humphreys, 2002) also find no significant economic impact from hosting major sporting events.

Despite the popularity of collegiate sports, however, academic economic impact studies have focused almost exclusively on professional sports and leagues. A handful of credible ex ante impact analyses of collegiate sporting events appear in the academic literature, including Mondello and Rishe (2004). On the ex post side, Matheson and Baade (2004) analyzed the impact of the NCAA men's and women's Final Four basketball tournament and found no statistically significant effect of either tournament on employment in host cities. The most ambitious study comes from Coates and Depken (2006) who researched the effect of college football games and other sports-related variables on taxable sales in individual counties in Texas. They found that each additional game resulted in an increase in taxable sales of between US$281,000 and US$465,000, which resulted in an increase in tax revenues between US$20,000 and US$34,000. They noted, however, that this effect appears limited to the smallest towns hosting college football games and that there is no statistically significant evidence that an NCAA football game has an effect on sales tax revenues in the big conference cities of Dallas, Houston, Fort Worth, Austin, College Station, Lubbock, and Waco.

There are two possible reasons the economic impact of college sports receives less attention. First, colleges and universities have been less explicit in their demands for local taxpayers to foot large bills for new stadiums, and subsequently there is less independent scholarly research that analyzes the economic impact of collegiate facilities. Yet the willingness of local residents to fund college stadium projects at taxpayer expense, the fungibility of public university money, and the tax-exempt status of most colleges and universities means the economic impact of collegiate sports is of crucial interest to the fields of public finance and urban and regional economics. Furthermore, the economic impact of collegiate sports sheds important additional light on the broader question of the impact of spectator sports in general on local economies.

The nature of college sports has also contributed to a lack of significant research. With only a few exceedingly unusual exceptions, large college football programs never relocate and date back several decades. This prevents a before- and-after comparison which is crucial in ex post economic impact studies common in professional sports. Stadium projects for colleges are also distinctly different than those for professional teams. In the NFL, the large majority of aging or economically obsolete facilities have been replaced by entirely new stadiums. In college football, on the other hand, most construction projects involve multiple incremental changes to existing facilities, such as adding capacity, improving existing seating, or providing luxury boxes. These incremental changes are another hurdle in the typical before-and-after comparisons unique to college athletics. Finally, the same problem arises in the analysis of postseason play in college football. Although the location of the NFL's Super Bowl or Major League Baseball's World Series changes from year to year, each of the major college bowls is always in the same city at the same time of the season. Thus, even if Miami's economy always surges around New Year's day, it is impossible to identify whether this spike is due to the annual Orange Bowl or some other annual attribute of the local economy.

This article uses the annual variation in the number of home games for most college football teams to generate economic impact estimates. Unlike the professional leagues, which play a balanced schedule of home and away games, there is more flexibility in the schedule of a college football program. Nearly all college football teams play in a conference, which usually consists of nearby universities of comparable size. The in-conference schedule of a college football team is relatively fixed; each team usually plays the same number of in-conference home and away games, and conference members use an alternating home and away schedule.

Nonconference games, which are usually played before the conference season begins, have considerably more flexibility. When scheduling games, opponents will often agree to a home and away schedule in which the opponents play at one school in first year and at the other school in the succeeding year. Alternatively, the largest and most successful schools will often also schedule nonconference opponents through the use of appearance guarantees. When a guarantee payment is made, the payer is under no obligation to play future game at the opponent's home stadium. For example, a large school like the Ohio State University likely would generate larger home revenues than a smaller opponent such as the University of Cincinnati could expect to receive from hosting Ohio State. Thus, depending on the break-down of a particular year's home-and-away contracts as well as the number of appearance guarantees made or accepted by a school, the number of home games a specific college football program plays during a season will vary from year to year, a deviation from the rule in most professional leagues. Occasionally, teams will also play games at neutral sites where one team may be nominally designated as the home team. In these cases, the contest would not be considered a home game for the institution or the institution's metropolitan area, as any economic activity that results from the contest would occur in the location where the game was played.

The Model

Two types of data have been used most frequently in the existing ex post studies. Coates and Humphreys (1999, 2002, 2003) and Baade and Matheson (2001, 2006) used annual data on employment, personal income, or personal income per capita over a wide number of cities and years to estimate the economic impact of sporting events. Clearly annual data is not ideal when examining relatively small events such as individual college football games. To this end, other studies such as Porter (1999), Baade and Matheson (2001), Coates (2006), Coates and Depken (2006), and Baade et al. (2008) have used taxable sales data that is available at a monthly or quarterly basis. Taxable sales data, however, cannot be used in nationwide panels of sporting events or teams because of cross-state differences in data availability and taxation laws. This leaves two options: examining institutions in a single state using high frequency data or examining a large panel of institutions using annual data. This article uses the panel approach.

Of course, one advantage to studying college sports rather than the major professional leagues is that professional teams are invariably located in the largest metropolitan areas in the country. Regular economic fluctuations of these large diverse economies are likely to obscure the impact of even the biggest sporting events. Although some major college football programs such as University of California at Los Angeles (UCLA) or the University of Miami are located in large metropolitan areas, many others are located in small college towns. In fact, the median population in 2004 of the 63 metropolitan statistical areas (MSAs) examined in this study is 425,000 compared with a median population of more than 2.3 million for MSAs with an NFL team. Identifying any economic changes resulting from spectator sports within smaller cities, should they exist, is likely to be an easier task than performing the same task in a major metropolitan area.

There are several approaches to estimate the impact an event has on a city. Mills and McDonald (1992) provided an extensive summary of these models, which seek to identify changes in economic activity through changes in key economic variables in the short run or the identification of long-term developments that enhance the capacity for growth. Our task is not to replicate explanations of metropolitan economic growth but to use past work to help identify any effects of college football games on economic indicators. To this end, we have selected explanatory variables from existing models to predict economic activity in the absence of the game. Estimating the economic impact of college football programs involves accounting for normal activity and determining whether the number of home games and/or success of the program increases economic activity. Thus, this approach depends on our ability to identify variables that account for the variation in growth in economic activity in host cities.

Given the number and variety of controls found in regional growth models and the inconsistency of coefficient size and significance, any critic can claim that a particular regression suffers from omitted-variable bias. However, it is far more challenging to specify the model that remedies the problem. In explaining regional or metropolitan growth patterns, at least some of the omitted variable problem can be addressed through a careful specification of the independent variables. Representing relevant variables as percentage changes purges the impact of forces that generally affect regional or national MSA growth, which improves identification of any city-specific large events.

The purpose of ex ante studies is to provide a measure of the net benefits a project or event is likely to yield. To our knowledge, there is no prospective model that has the capacity for measuring the net benefits of a project relative to the next best alternative use of those funds. If one assumes that the best use of funds has always occurred, then the growth path observed for a city can be considered optimal. If this optimal growth path, identified by the city's secular growth trend, does not increase during years in which a team plays a higher than normal number of games at home or wins more games than usual, then the evidence does not support the hypothesis that college football contributes positively to a region's economy. In this case, any public subsidization of a college team or its playing facility does not put public monies to their best use.

We use a sample of 63 MSAs over 1969-2004. This includes all cities that are home to a team in one of the six Bowl Championship Series conferences. In addition, three additional universities, Notre Dame, Air Force, and Brigham Young, are added to the sample based on the prominence of their programs both in terms of average attendance and success on the playing field. Two MSAs, Los Angeles and Raleigh-Durham, North Carolina, are home to two football programs. The 63 MSAs in the sample include the home of every national football champion since 1970 and every school whose average attendance typically ranks within the top 50 in college football. Although the choice of 63 cities is somewhat arbitrary, the list includes essentially every university that would generally be considered to have a big-time football program and excludes minor schools with lesser athletic ambitions. Table 1 provides a list of the cities included, whereas Table 2 provides summary statistics for the data. It should also be noted that the variation in the number of home games for most cities is surprisingly large. The difference between the maximum observed number of home games and the minimum number of home games within each MSA averages just over 3 for the 63 cities in the sample, and in 50.2% of all observations the number of home games in a particular season is different than the number of home games in the preceding season.

The following is our baseline model for the estimations:

$$Y_{it} = \beta_0 + \beta_1 \mathrm{POP}_{it} + \beta_j \mathrm{OTHER}_{it} + \beta_k \mathrm{CFB}_{it} + \gamma_t + \alpha_i + \varepsilon_{it} \tag{1}$$

We estimated (1) using three different dependent variables (Y_{it}): growth rates of real personal income, employment, and real per capita income in year t and MSA i. POP_{it} is the log population of city i in time t. OTHER_{it} is a vector of dummy variables that represents identifiable MSA-level deviations of the national business cycle such as the tech boom and bust in Silicon Valley during 1999-2001, the oil boom and bust cycles in the 1970s and 1980s in oil-producing cities, and the effects of natural disasters such as Hurricane Andrew in Miami. The specific variables, cities, and years included in OTHER_{it} is available from the authors on request.

Table 1
Cities and Schools in the Data Set

School	Conference	City	State	Metro Area
BC[a]	ACC	Chestnut Hill	MA	Boston
Clemson	ACC	Clemson	SC	Seneca
Duke	ACC	Durham	NC	Durham
Florida State	ACC	Tallahassee	FL	Tallahassee
Georgia Tech[a]	ACC	Atlanta	GA	Atlanta-Sandy Springs-Marietta
Maryland[a]	ACC	College Park	MD	Washington
Miami[a]	ACC	Miami	FL	Miami
N.C. State[a]	ACC	Raleigh	NC	Raleigh-Cary
North Carolina	ACC	Chapel Hill	NC	Durham
Virginia	ACC	Charlottesville	VA	Charlottesville
Virginia Tech	ACC	Blacksburg	VA	Blacksburg-Christiansburg-Vicksburg
Wake Forest	ACC	Winston-Salem	NC	Winston-Salem
Louisville	Big East	Louisville	KY	Louisville-Jefferson
Pittsburgh[a]	Big East	Pittsburgh	PA	Pittsburgh
Rutgers[a]	Big East	Newark	NJ	Newark-Union
Syracuse	Big East	Syracuse	NY	Syracuse
West Virginia	Big East	Morgantown	WV	Morgantown
Illinois	Big Ten	Champaign	IL	Champaign-Urbana
Indiana	Big Ten	Bloomington	IN	Bloomington
Iowa	Big Ten	Iowa City	IA	Iowa City
Michigan	Big Ten	Ann Arbor	MI	Ann Arbor
Michigan State	Big Ten	East Lansing	MI	Lansing-East Lansing
Minnesota[a]	Big Ten	Minneapolis	MN	Minneapolis-St. Paul-Bloomington
Northwestern[a]	Big Ten	Evanston	Il	Chicago-Naperville-Joilet
Ohio State	Big Ten	Columbus	OH	Columbus
Penn State	Big Ten	University Park	PA	State College
Purdue	Big Ten	West Lafayette	IN	Lafayette
Wisconsin	Big Ten	Madison	WI	Madison
Baylor	Big Twelve	Waco	TX	Waco
Colorado	Big Twelve	Boulder	CO	Boulder
Iowa State	Big Twelve	Ames	IA	Ames
Kansas	Big Twelve	Lawrence	KS	Lawrence
Kansas State	Big Twelve	Manhattan	KS	Manhattan
Missouri	Big Twelve	Columbia	MO	Columbia
Nebraska	Big Twelve	Lincoln	NE	Lincoln
Oklahoma	Big Twelve	Norman	OK	Oklahoma City
Oklahoma State	Big Twelve	Stillwater	OK	Stillwater
Texas	Big Twelve	Austin	TX	Austin-Round Rock
Texas A&M[a]	Big Twelve	College Station	TX	Houston-Sugar Land-Baytown
Texas Tech	Big Twelve	Lubbock	TX	Lubbock
Arizona[a]	Pac-10	Tucson	AZ	Tucson
Arizona State[a]	Pac-10	Tempe	AZ	Phoenix-Mesa-Scottsdale
California[a]	Pac-10	Berkeley	CA	San Francisco-Oakland-Fremont

(continued)

Table 1 (continued)

School	Conference	City	State	Metro Area
Oregon	Pac-10	Eugene	OR	Eugene-Springfield
Oregon State	Pac-10	Corvallis	OR	Corvallis
Stanford[a]	Pac-10	Stanford	CA	San Francisco-San Mateo-Redwood City
UCLA[a]	Pac-10	Los Angeles	CA	Los Angeles-Long Beach-Glendale
USC[a]	Pac-10	Los Angeles	CA	Los Angeles-Long Beach-Glendale
Washington[a]	Pac-10	Lake Washington	WA	Seattle-Tacoma-Bellevue
Washington State	Pac-10	Pullman	WA	Pullman
Alabama	SEC	Tuscaloosa	AL	Tuscaloosa
Arkansas	SEC	Fayetteville	AR	Fayetteville-Springdale-Rogers
Auburn	SEC	Auburn	AL	Auburn-Opelika
Florida	SEC	Gainesville	FL	Gainesville
Georgia	SEC	Athens	GA	Athens-Clarke County
Kentucky	SEC	Lexington	KY	Lexington-Fayette
LSU	SEC	Baton Rouge	LA	Baton Rouge
Mississippi	SEC	University Park	MS	Oxford
Mississippi State	SEC	Starkville	MS	Starkville
South Carolina	SEC	Columbia	SC	Columbia
Tennessee	SEC	Knoxville	TN	Knoxville
Vanderbilt[a]	SEC	Nashville	TN	Nashville-Davidson-Murfreesboro
Notre Dame	Independent	Notre Dame	IN	South Bend-Mishawaka
BYU	MWC	Provo	UT	Provo-Orem
Air Force	MWC	Colorado Springs	CO	Colorado Springs

a. Denotes cities with a maximum population in excess of 750,000. These cities are removed from the regression results presented in Table 4.

CFB_{it} represents our vector of college football proxies, which include number of home games, winning percentage, and dummy variables for teams in a national championship season and the year following a national championship. We also included an interaction term between the number of games and population to determine if the effect of hosting a college football game differs by city size. Finally, to account for the panel nature of our data, we included controls for each year (γ_t) and MSA (α_i). Ideally, this specification allows MSAs to have different intercepts and also purges national trends. In other versions of this model, we also included controls for city-specific time trends as well as other dummy variables for specific economic events not included in the $OTHER_{it}$ vector, but these additions added little explanatory power and did not impact our main results.

We used the growth rates of each dependent variable because Dickey–Fuller and Phillips–Perron tests cannot reject the existence of a unit root when the dependent variables are in levels. Although first differencing a dependent variable in levels is often effective in eliminating a unit root, first differencing is essentially analogous to taking the growth rate of the variable. The use of a growth rate is

Table 2
Summary Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
Percent personal income growth	0.0318	0.0325	−0.1278	0.2083
Percent employment growth	0.0228	0.0270	−0.1457	0.1230
Percent personal income per capita growth	0.0171	0.0292	−0.1195	0.2290
ln (population)	12.781	1.337	10.086	16.056
ln (population) squared	165.159	34.866	101.723	257.779
Number of games	5.958	1.412	0	13
Number of games (without Durham and LA)	5.781	1.019	0	8
Winning percentage	0.5512	0.222	0	1
National champs	0.0187	—	0	1

more intuitively appealing than using first differences of levels and is likely to have the same effect of eliminating any underlying unit roots.

In fact, several tests reject the existence of a unit root in the growth rates of the dependent variables. First, we performed Dickey–Fuller and Phillips–Perron tests for each city. For all three dependent variables, 61 of the 63 cities passed both tests at 5%. The other two cities (Tallahassee and Washington, DC) passed both tests at 10%. We also performed unit root tests on the entire panel using tests from Levin, Lin, and Chu (2002) and Im, Pesaran, and Shin (2003), which allow for panel-specific attributes, such as differing time trends and autoregressive paths. Both tests rejected the existence of a unit root in all three dependent variables.

Given the time-series nature of the data, the error term in equation (1) is likely to be autocorrelated. Although ordinary least squares regressions will produce consistent estimates, the standard errors will be incorrect. We used a test suggested by Wooldridge (2002) for autocorrelation within each panel, which estimates $\hat{\varepsilon}_{it} = \rho\hat{\varepsilon}_{i,t-1} + u_{it}$. Under the null hypothesis no autocorrelation, $\rho = -0.5$, and all three dependent variables rejected the null. This is not surprising as there are likely to be carry-over effects of each dependent variable from 1 year to the next.

One method to account for the autocorrelation is to include an autoregressive component, which changes our estimation model to

$$Y_{it} = \beta_0 + \beta_1 Y_{i,t-1} + \beta_2 \text{POP}_{it} + \beta_3 \text{TECH}_{it} + \beta_4 \text{CFB}_{it} + \gamma t + \alpha_i + \varepsilon_{it}. \tag{2}$$

Introducing a lagged dependent variable requires the Arellano and Bond (1991) estimation technique, which is sometimes referred to as a difference GMM model. This model is described in several works, including Bond (2002) and Roodman (2006). This model begins by differencing equation (1), which purges α_i. Once the city-specific effect is removed, the model uses higher-order lags of Y_{it} to instrument for $\Delta Y_{i,t-1}$. Any other independent variables that are believed to be endogenous or predetermined (i.e., variables independent to the current error but not previous errors) can be handled in the same way.

Given $T = 35$, there are 34 observations of the differenced dependent variable (ΔY_{it}) for each city. Given that the first lag of the differenced dependent variable is endogenous ($\Delta Y_{i,t-1}$), all of the remaining 32 higher-order lags can be used as instruments for ΔY_{it}. Although the higher-order lags should create missing values in practice, Holtz-Eakin, Newey, and Rosen (1988) showed that each instrument produces a useful moment condition. In other words, consider the moment condition $E\left[Z'_{it}\Delta\varepsilon_{it}\right] = 0$, where Z'_{it} contains the instruments (i.e., the higher-order lags) and $\Delta\varepsilon_{it}$ is the differenced error term. For the second-order lag instrument, the moment condition is $\sum_i y_{i,t-2}\Delta\varepsilon_{it} = 0$ if $t \geq 3$; for the third-order lag instrument, the moment condition is $\sum_i y_{i,t-3}\Delta\varepsilon_{it} = 0$ if $t \geq 4$, and so on.

Consistency of this approach requires the error terms to be independently and identically distributed, which is typically cannot be assumed in dynamic panel models. For example, it is plausible that the variance of the error term (original or differenced) may differ across cities. A weighting matrix W asymptotically corrects the moment condition: $W = \frac{1}{N}\sum_i (\overline{Z'_i}\Delta\overline{\varepsilon_i}\Delta\overline{\varepsilon'_i}\overline{Z_i})$, where $\overline{Z_i}$ and $\Delta\overline{\varepsilon_i}$ are city-specific (T–2) vectors. Using this weighting matrix, GMM minimizes $(\frac{1}{N}\sum_i \Delta\overline{\varepsilon'_i}\overline{Z_i})W^{-1}(\frac{1}{N}\sum_i \Delta\overline{Z'_i}\overline{\varepsilon_i})$.

To obtain the weighting matrix, it is necessary to have consistent estimates of $\Delta\overline{\varepsilon_i}$, which can be obtained using a different weighting matrix $W_1 = \frac{1}{N}\sum_i (\overline{Z'_i}H\overline{Z_i})$, where H is a (T–2) square matrix with 2 on the diagonal, -1 on all of the immediate off-diagonals, and 0 elsewhere. Thus, the first-step estimates the model using W_1 to produce the estimates $\Delta\widehat{\varepsilon}_{it}$, which the second step uses in the weighting matrix W. Although this correction produces the desirable asymptotic properties, several works (Arellano & Bond, 1991 and Blundell & Bond, 1998, to name only two) have suggested that the standard errors in the second step are downward biased. We used the Windmeijer (2005) finite-sample correction to adjust the standard errors. Finally, one concern with the Arellano and Bond (1991) technique is the overidentifying restrictions, especially given the relatively longtime period for each city in our data. We used a Hansen (1982) test to determine the number of overidentifying restrictions.

Table 3 presents the Arellano–Bond estimation results of equation (2) using each of the three dependent variables. For brevity, we omit the estimates for the year dummies, city fixed effects, and the vector of OTHER_{it} variables. None of the college football controls are statistically significant, and winning percentage and the year following a championship season have a negative effect on all three dependent variables. The Arellano–Bond tests for autoregressive errors produce the expected result. For personal income and employment growth, these tests suggested that autocorrelation exists in the first lag (which is expected), but not in the second.

More importantly, none of the college football variables are statistically significant in any of the models. In fact, less than half of the estimates have the expected

Table 3
Arellano–Bond Estimation Results (Standard Errors in Parentheses), All Cities

Dependent Variable	Personal Income Growth	Employment Growth	Personal Income Per Capita Growth
Dependent variable$_{t-1}$	0.6617*(0.3542)	0.3452***(0.1000)	−0.3559(1.0102)
ln (population)	−0.1593***(0.0447)	−0.0875(0.0595)	
ln (population)* number of games	−0.0003(0.0009)	−0.0001(0.0006)	
Number of games	0.0026(0.0123)	0.0008(0.0082)	−0.0005(0.0006)
Winning percentage	−0.0079(0.0066)	−0.0011(0.0028)	−0.0048(0.0069)
National champs	0.0037(0.0040)	0.0032(0.0026)	0.0015(0.0043)
National champs$_{t+1}$	−0.0047(0.0037)	−0.0020(0.0028)	−0.0033(0.0046)
Arellano–Bond test for AR(1)	$z = -1.67$ $p = .096$	$z = -3.41$ $p = .001$	$z = -0.24$ $p = .808$
Arellano–Bond test for AR(2)	$z = 1.10$ $p = .270$	$z = 0.51$ $p = .613$	$z = -0.32$ $p = .748$
Instruments (lags of differenced dependent variable)	2,3	2,3,4	2,3,4
Hansen test for overidentification	$\chi^2 = 0.35$ $p = .551$	$\chi^2 = 2.68$ $p = .261$	$\chi^2 = 4.76$ $p = .092$

Note: For brevity, we omit the year dummies, city fixed effects, and the coefficients on the vector of OTHER_{it} variables. Full results are available from the authors on request.
*Statistically significant at the 10% significance level.
**Statistically significant at the 5% significance level.
***Statistically significant at the 1% significance level.

positive sign (under the assumption that college football increases economic activity). Therefore, we can find no benefit of additional games or a winning program on per capita income, employment, or personal income. Given that all of the schools have had college football programs throughout the sample frame, we cannot test whether the existence of a large football program helps or hurts an area. Rather, our results suggest that additional games or a winning program, conditional of already having a team, does not impact employment or personal income. It is important to note that most ex ante economic impact studies expect additional home games to significantly increase local economic activity. For example, the previously cited University of Nebraska study predicted that each additional home game would bring in US$5 million extra for the local economy (Thompson, 2005).

One possibility is that the effects of our college football variables vary by city size. For example, the effect of a college football game is likely to be obscured in the large economies in which some of the institutions in our data reside. Although we attempted to capture this phenomenon with the interaction between number of games and population, another method is to estimate the model separately for smaller cities.

Unfortunately, the Arellano–Bond is only appropriate when $n > T$, so no more than 25 cities can be cut from the sample without resorting to an alternative

Table 4
Arellano–Bond Estimation Results (Standard Errors in Parentheses), Cities With Population Less Than 750,000

Dependent Variable	Personal Income Growth	Employment Growth	Personal Income Per Capita Growth
Dependent variable$_{t-1}$	0.5187(0.3857)	0.4287***(0.1398)	−0.8160(0.8136)
Number of games	−0.0008(0.0014)	−0.0001(0.0013)	0.0013(0.0008)
Winning percentage	−0.0079(0.0049)	−0.0024(0.0040)	−0.0089**(0.0039)
National champs	−0.0011(0.0032)	0.0045(0.0026)	0.0010(0.0040)
National champs$_{t+1}$	0.0052(0.0046)	−0.0020(0.0045)	−0.0055(0.0038)
Arellano–Bond test for AR(1)	$z=-1.98$ $p=.048$	$z=-3.18$ $p=.001$	$z=-0.16$ $p=.875$
Arellano–Bond test for AR(2)	$z=1.21$ $p=.227$	$z=0.86$ $p=.387$	$z=-0.94$ $p=.348$
Instruments (lags of differenced dependent variables)	2,3,4,5,6,7	2,3	2,3,4
Hansen test for overidentification	$\chi^2=5.23$ $p=.389$	$\chi^2=0.55$ $p=.458$	$\chi^2=0.85$ $p=.356$

Note: See note to Table 3.

estimation method. Therefore, we removed all MSAs with a population of greater than 750,000 at any point in the timeframe, which eliminated all cities with an NFL or other major league teams as well as a handful of cities with professed big-league ambitions. Removing these MSAs left 42 cities with a median population of 221,000 and a mean population of 276,000, roughly one fifth the mean population of the full sample.

Table 4 presents the estimates for this reduced sample of cities. In the restricted models only three of the coefficients for the 12 football variables even have a positive sign, much less a reasonable level of statistical significance. Winning percentage is statistically significant in the personal income per capita model, but the sign is negative. Thus, the data indicate that in smaller towns and cities dominated by local universities, winning seasons actually reduce the growth rate of per capita personal income. Therefore, our main conclusions are only strengthened by an examination of small towns. We do not observe any strong evidence that the economic effect of college football differs between small and large cities, and even for the very smallest cities in the sample, those cities where a home football game can literally double or triple the population of the city on game days, college football games still do not appear to have any positive effect on the economic growth rates of local economies and may actually have negative effects.

Point estimates for personal income from the second model suggest that an additional home football game actually reduces local personal income in smaller cities by US$5 million, although a 95% confidence interval extends the estimate to between US$13.3 million and negative US$24.0 million in additional personal income. Although the confidence interval on the coefficient is large enough such that an ex ante estimate of US$5 million per game cannot be rejected, these results certainly do little to bolster claims of large positive economic impacts from sporting events.

Conclusions

This article provides another empirical examination of the economic impact of spectator sports on local economies. Confirming the results of other ex post analyses of sports in general, this article finds no statistically significant evidence that college football games contribute positively to a host city's economy. Our analysis from 1970 to 2004 of 63 metropolitan areas that play host to big-time college football programs find that neither the number of home games played, the winning percentage of the local team, nor winning a national championship has a discernable impact on either employment or personal income in the cities where the teams play.

Of course, it can be argued that any attempt to discern the effects of sporting events within the context of large, diverse metropolitan areas is like searching for a needle in a haystack. In contrast to most other ex post studies of the economic impact of sports, however, this article examines big events in many small communities thereby making the haystack as little as possible. If such a needle exists, it should certainly be easier to find within a typical college town than in a major league city.

Given the fact that the only identifiable economic impact detected in even these smaller communities was negative, this is all the more evidence that spectator sports in general do not have a large positive net economic impact on host cities. Although successful college football teams may bring fame to their home towns, fortune appears to be a bit more elusive as big plays and big crowds inside the stadium do not seem to translate into big money outside the stadium.

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Robert A. Baade has been the James D. Vail professor of economics at Lake Forest College since 1986. He earned his PhD in economics from the University of Wisconsin at Madison. He has published approximately 40 scholarly articles, book chapters, and monographs on a variety of topics relating to the economics of professional and intercollegiate sports. In addition to his work as a scholar, he has been involved in numerous state, county, and city deliberations relating to large public projects including stadiums, arenas, and convention centers. He currently serves as the president of the International Association of Sports Economists.

Robert W. Baumann is an assistant professor in the Department of Economics at the College of the Holy Cross in Worcester, Massachusetts. He received his PhD from the Ohio State University. His scholarly work, which ranges from the economics of education and poverty to the impact of natural disasters and professional sports on local economies, has appeared in *Urban Studies, Growth and Change*, and the *Southern Economic Journal*.

Victor A. Matheson, PhD, is an associate professor in the Department of Economics at the College of the Holy Cross in Worcester, Massachusetts. He has published extensively in the field of the economics of collegiate and professional sports including studies of the economic impact of the Super Bowl, World Cup, and Summer Olympics. He also works as a referee in the top professional and intercollegiate soccer leagues in the United States.