How does hosting the Olympic Games impact employment in the host city?

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Abstract

Despite the size and prestige of the Olympic Games, few studies exist to determine whether or not the Games benefit host cities. Existing studies suggest that the Olympics may lead to increased employment, but they reach little consensus on the size or length of that impact. Controlling for the effects of GDP and price levels, I measure the size and shape of the "Olympic effect" with a series of time-period dummies and a fixed-effects model. My study examines all Summer Games from 1984 to 2004 in the first panel study of employment surrounding the Olympics. Using a Prais-Winsten method to correct for heteroscedasticity and AR(1) autocorrelation, I find evidence of a significant employment increase lasting in general from 6 years before the Olympics to 1 year after the Games, with a marginally significant boost lasting up to 8 years afterward. I also find that higher Olympic expenditures are negatively correlated with the size of the Olympic effect, and that the employment impact of the Olympics may be larger in wealthier countries.

Introduction

The modern Olympic Games are among the largest and most complex international conventions ever assembled. Every four years, they provide one host city with an unparalleled opportunity to present itself to the world. Moreover, considerable anecdotal evidence suggests that hosting the Games may have long-lasting positive impacts on a city's economy. This has inspired a strong increase in the demand for hosting the Games, particularly among developing countries that seek an economic boost—the so-called "Olympic effect." Yet putting together an Olympic Games is a daunting task; the Games pose innumerable challenges related to funding, revenue generation, and preparing cities for the fleeting influx of athletes, tourists, and others. If cities hope to capitalize on the promise of Olympic effects, they must be well informed. However, not enough sound econometric analysis exists to be certain that hosting an Olympic Games actually benefits the host city, or to understand the causes and shape of those impacts.

Econometric analyses of the economic impacts of the Olympics are surprisingly scarce. Analyses that attempt to build robust models of the expected impacts by looking at multiple Olympic Games are virtually nonexistent. This reflects several major obstacles to such research. Any analysis must account for the substantial institutional differences between different countries and different Olympic Games. Moreover, one must suppose that there has been enough stability in the process over time that an econometric analysis of this type may have some explanatory power. The Olympics of today are a far cry from the Olympics of the early 20^{th} century, and comparing their effects would yield little information. However, in the past 20 years, stability in the financial structure of the Olympics has finally made this sort of analysis possible.

In spite of their challenges associated with creating them, cross-Games analyses of economic impacts are broadly useful. Many cities can potentially host an Olympic Games. Major national or regional cities that are reasonably developed, industrialized, have open economies and rely heavily on markets are prime candidates. Cities that bear some resemblance to recent hosts may be able to use a cross-Games study to predict the impact of the Games when they formulate bids. Moreover, by knowing what types of bids may generate the maximum economic impacts, planners can craft bids that make more efficient use of the economic boost that the Games may provide.

Most employment directly related to the Games itself is short in duration. However, a substantial part of the promise hosting the Olympic Games is the prospect of an indirect boost in employment surrounding the Games. Data on net employment effects are readily available, relatively easy to compare across locations, and reflective of changes in local economic conditions. They are easily adjusted to control for national economic conditions when examined in time-series. In short, employment statistics may explain the benefits of hosting Olympic Games more clearly than any similar measure. Thus, I have chosen to study the effects of hosting the Summer Olympic Games from 1984-2004 on employment in host cities.

The implementation of the first ever pooled time-series study of Olympic employment produced some surprising results. I found evidence of a positive and significant employment boost from soon after the time the host city was announced until well after the Games had ended. This effect was positive and significant for one year after the Games, although it remained marginally significant for eight years after the Games. This contrasts with the previous findings of others, and suggests that previous studies did not look at enough data to find evidence of a significant impact. I found that countries with higher GDP per capita tend to receive a larger boost from the Olympics. This suggests that they may be better equipped to capitalize on the promise of the Games. Finally, I found that higher expenditures on the Olympics were associated with a decrease in the size of the Olympic effect. This indicates that massive infrastructure spending in the name of urban development may be counterproductive, and also that Organizing Committees may be planning excessively large Olympic Games because the International Olympic Committee has a monopoly on granting hosting rights.

Institutional Background

After a 1500-year hiatus, the modern Olympic era began in 1896 with the Athens Games. Since then, nations of the modern world have met every four years—with a few exceptions—to celebrate this ancient Greek tradition and the spirit of amateur athletics. However, even these "modern" Games bear little resemblance to the Games today. The Games have grown in size, from the number of athletes and events to their now-enormous cost. For most of the 20th century, the Olympic Games were essentially a publicly subsidized undertaking. However, the 1976 Olympics burdened the city of Montreal with a major deficit. This called into question the sustainability of a system that was dependent on domestic altruism. Two years later, Los Angeles submitted the sole bid for the 1984 Summer Olympics. Thus, the Los Angeles Organizing Committee negotiated considerable financial reforms to reduce the public financial burden of the Games. The economic success of the 1984 Summer Games established permanent changes in Olympic planning. Since then, the Games have been marked by increased revenues from television and sponsorships, and the decline of public financing (Preuss 2004). I have chosen to study this modern era of the Olympic Games, since it is of primary relevance to policy makers and potential Olympic host cities today. Accordingly, I examine the cities that hosted the six Summer Games from 1984-2004: Los Angeles, Seoul, Barcelona, Atlanta, Sydney, and Athens.[1]

The process of hosting an Olympic Games begins at the city level. Each city that wishes to host a given Olympic Games creates an Organizing Committee of the Olympic Games (OCOG). This committee is charged with creating a preliminary plan for the organization of the Games, which is submitted as part of the city's Olympic bid. This plan typically includes information on the size and location of newly planned construction, plans for utilizing or repurposing existing venues, other planned construction or expenditures, and plans for financing the Games. Then, seven years before the Games, these bids are considered and voted upon by the International Olympic Committee (IOC), the governing body of the Olympic Games. Once a host has been determined, a public announcement is made and the successful Organizing Committee transitions to begin planning for the Games themselves (Preuss 2004).

The International Olympic Committee has a unique position as the sole governing body of the Olympic Games. Because it is the only organization that can grant the right to host the Games, it acts as a monopoly. Cities compete fiercely for these rights, and their lack of bargaining power with the IOC causes them to make their bids more attractive and grandiose than necessary to host the Games. OCOGs often use unnecessarily lavish construction plans and financing concessions in an attempt to sway the IOC.

The only exception to this rule was the Los Angeles Olympics of 1984. Because Los Angeles submitted the only bid for the 1984 Games, the city's organizing committee successfully countered the IOC's monopoly power. Its construction was sparser than any other modern

[1] I consider the Summer and Winter Olympics to be fundamentally different; the Winter Games are smaller and are hosted by smaller cities. The group of cities that may consider bidding for the Winter Games is generally different from the group that may bid for the Summer Games, and they are not amenable to examination in the same model. Therefore, I do not consider the impact of hosting the Winter Games on employment, nor should one necessarily anticipate a similar effect from hosting the Winter Games as that predicted of the Summer Games by this model.

Olympic Games. Furthermore, the city negotiated an important change in the IOC's rules on Games-related financing: it assumed no financial liability for the Games. This had important lasting impacts on financing of the Games. As Preuss notes, "With the Games of Los Angeles 1984, private financing increased and publicly financed Games became history. The Olympic Movement started to receive high revenues from selling Olympic rights to television networks and sponsors" (Preuss 2004).

Despite the lasting impacts of the Los Angeles Games, the IOC's monopoly is a persistent problem. Other than collusion among bidding cities, there is no realistic way to limit the IOC's monopoly power directly. Only one international organization can reasonably select an Olympic host city; a competitive market is impossible. Some economists have proposed addressing the IOC's monopoly power by instituting an auction process for the right to host the Games. Cities would pay the IOC an amount commensurate with the expected benefit of the Games, and the highest bidder would win the right to host them. This remains a monopolistic model, and the distribution of benefits of auctioning hosting rights is clearly weighted toward the IOC. However, the revenues from the auction could conceivably be redistributed, while excessive Olympic construction or other expenditures become sunk costs once hosting rights are awarded (Stewart and Wu 1997).

However, IOC monopoly is not the only factor that drives massive Olympic spending. Political incentives also tend to favor larger Olympic bids. Proponents of massive infrastructure spending frequently see Olympic bids as a convenient vehicle for their agenda. They use Olympic bids to spur massive investment in public transportation, sports and convention facilities, or affordable housing. Because the Olympics typically require substantial investments already, and because the nature of the Olympic bidding process often favors large expenditures, OCOGs may push through infrastructure investments that would otherwise be too politically controversial. The Olympic Games present a clear deadline, and the need to finish elaborate projects is generally seen as a matter of civic pride. Thus, the Games often propel local infrastructure plans dramatically forward. Traffic infrastructure plans in Barcelona shifted 10 years ahead of their initial schedule as a result of the Games, while the Munich Games of 1972 saw nearly 20 years' advancement in some types of infrastructure (Preuss 2004). I consider the net impact of these expenditures my model.

The recent history of the Olympics says a lot about the type of incentives that drive Olympic bids. It also says a lot about the cities that submit them. The set of Olympic hosts over the past two decades provides a good cross-section of the types of cities that bid for the Games. In general, recent Olympic host cities share a number of similarities. All of the cities are located in moderately or highly developed countries. In fact, all five countries in my sample are OECD members.[2] All hosts were important major cities in their respective countries before the Games were held. In smaller countries, they are typically the primary national urban center.

However, there are also some substantial differences among host cities in the sample. For example, the most populous nation in the sample, the United States, has over twenty times the civilian population of the least populous nation, Greece. In Korea and Greece the average person makes barely a third of what the average American makes. Metropolitan Atlanta includes less than two percent of the population of the United States, while a third of Greeks inhabit metropolitan Athens. A model of Olympic effects must account for these differences if it is to have explanatory power for other potential host cities.

[2] South Korea did not join the OECD until after it hosted the 1988 Games.

Existing Literature

Ex-ante studies of the expected economic effects of the Olympics are plentiful. They are often produced by Organizing Committees to bolster bids. These studies often use complex computer modeling techniques to predict expected changes in income from sources such as tourism and infrastructure investment. Generally, the output of these studies is an expected "multiplier effect," which attempts to describe the long-term expected value of a unit of Olympic-related revenue in terms of income, employment, or wage growth. Both Kasimati and Preuss cite examples of this type of modeling from every Summer Games from Los Angeles to Beijing. However, these models are highly parameter dependent, and they are subject to a number of biases. Many of these biases are intentional; studies overstate the impact of the Games to bolster Olympic bids. They may fail to consider such important factors as crowding out of other economic activity, changes in net flows instead of gross ones, and the range of activities to which their resultant "multiplier" may apply. In sum, they are not a reliable indicator of long-term economic activity (Kasimati 2003; Preuss 2004).

In contrast, the history of post-Olympic economic analyses is much more limited. Yet these analyses provide the best opportunity to accurately examine the economic effects of hosting the Games. Since they deal with empirical data and not expected parameters, post-Games studies are largely free of the biases that plague pre-Games assessments. Moreover, they are much simpler and more transparent. Macroeconomic studies can account for a wide range of factors that impact the economy simply by comparing one region's economy to another's, or by looking at a subset of a region against the broader whole. A well-constructed ex-post model may be much more robust than any pre-Games prediction. However, few of these models exist. Once an Olympic Games is over, the Organizing Committee quickly dissolves. Post-Games impact studies are of little concern to cities that have already hosted the Games; at that point the Games and all Games-related expenditures are sunk costs. Nonetheless, economists have taken on three basic types of analyses of the Games: stock market studies, policy-impact studies, and employment studies. Each of these contributes differently to our understanding of the Games' impact.

Stock market analyses look at changes in the stock market as a result of an Olympic announcement. Although this does not itself measure the long-term economic effects of hosting the Games, any positive stock market reaction may be seen as an attempt to capture rents from additional activity generated by the Games. In "The Sydney Olympic Games and Australian Stock Market Reaction," Berman, Brooks, and Davidson (2000) looked at stock market data from the Australian Stock Exchange surrounding the announcement that Sydney would host the 2000 Olympics. They found that companies with head offices in New South Wales saw a positive impact on stock prices as a result of the Olympic announcement.[3] They also find a significant rise in stocks in several industries related to construction. Building upon this analysis, Veraros, Kasimati, and Dawson investigated the impact of the announcement that the 2004 Olympics would be hosted in Athens (2004). Similarly, they find a significant positive impact on stocks in the construction and industrials sectors.

These studies suggest that the effect of the Games may be largely localized, and that it is primarily confined to industries that benefit from Olympic construction. However, although these conclusions are valuable, they do not give us an adequate picture of the impact of the Games. They are an imperfect measure of present value, since we cannot be sure that no rent-seeking has occurred based on the likelihood that a city's bid will be successful. Moreover, stock market gains can tell us nothing about the nature of future economic gains over time, only

[3] New South Wales is the Australian state of which Sydney is the capital.

their present value. Ultimately, although studies of stock market behavior help us to reach general conclusions about the types of economic impact that may be expected from the Games, they provide little insight into the actual level of the impact over the long term.

Policy-impact studies related to the Olympics inform our understanding of the Games in other ways. Such analyses look not at the benefits of economic activity generated by the Olympics themselves, but at the benefits of policy changes made by host cities or countries in response to the Olympics. The methods of these analyses tend to come not from the realm of macroeconomics—as employment studies do—but from the field of public sector economics. "Blue Skies in Beijing? Looking at the Olympic Effect" considers the economic benefits associated with Beijing's decision to reduce ambient air pollution in advance of the 2008 Olympic Games (Brajer and Mead 2003). The authors have estimated that over the period 1999-2008, reduced pollution will have a net benefit to Beijing citizens of approximately $29 billion (base 2000 U.S. dollars). This public policy benefit is many times greater than Beijing's direct investment in the Games. Public policy analyses give credence to the notion that the unique set of circumstances surrounding the Games may have far-reaching economic effects.

Despite the useful implications of stock market and policy studies of the Games, studies of the employment effect of the Olympics do the best job of explaining Olympic impacts on local economies over time. Existing employment impact studies have attempted to isolate gains in employment or wage growth in a specified region that can be attributed to the Olympics. Time-series regression techniques allow these studies to control for factors such as sector composition and regional or national trends. Moreover, these techniques allow economists to consider the changes in economic impacts over time, including the potential for either temporary or permanent effects. Growth in employment and wages are salient indicators for policy makers or

Olympic planners who care not only about the size of the economic benefits of hosting the Games, but also about their distribution. Thus, studies of this nature allow us to make more useful conclusions about the Olympics' economic impact than other economic impact studies.

Hotchkiss, Moore and Zobay consider the possibility of permanent effects on employment and wages as a result of the 1996 Atlanta Olympic Games (2003). Their analysis considers two possibilities: level effects—permanent upward shifts—and growth effects—permanent increases in the rate of employment and wage growth. Growth effects compound over time, making their long-run effect large. In contrast, level effects become less important over time as their impact becomes a smaller fraction of total employment. Running separate regressions for each type of effect, they compare employment and wages in the Atlanta metropolitan area and other counties with or near Olympic venues against employment in the rest of the state of Georgia. They find evidence of both level effects and growth effects in employment. Their wage results are also positive and significant, although the inconclusive results of a follow-up test for robustness suggest that the evidence for an impact on wages is less clear. Finally, in order to further justify their findings with respect to employment, the authors compare employment in Atlanta with employment in similar Metropolitan Statistical Areas (MSAs) over the same period. Again, they find that the Atlanta Olympics had a positive and significant impact on overall employment levels.

The study of Hotchkiss, Moore, and Zobay provides a useful point from which to examine the impact of the Games. However, the study paints only a preliminary picture of how the Olympic effect might work. The authors never consider the possibility that both level and growth effects exist simultaneously, nor do they justify the use of one result over the other. Because they give no reason for using one model instead of another, Hotchkiss, Moore, and

Zobay make it difficult to use their models to make practical predictions about expected Olympic effects.

More importantly, the authors consider only the existence of permanent Olympic effects. There are a number of practical arguments for the possibility of permanent effects. One is permanent increased awareness of a city for foreign investors, consumers, and tourists. Cities, especially cities in less developed countries, often hope that the Olympic Games will help them find new investors and new markets for their exports. For example, the Spanish region of Catalonia saw some 200 American and Japanese companies make investments in the period surrounding the Barcelona Olympic Games (Preuss 2004). Similarly, foreign airline connectivity to Seoul improved substantially after the Games, which is evidence of increased tourism (Preuss 2004). Cities go to substantial lengths to promote these gains; through measures such as a visiting journalist program, Sydney increased its coverage in foreign media by twelve times over average coverage (Preuss 2004). Tourism effects are most likely to be level effects, while foreign investment may create permanent growth effects. In other words, we cannot discount the possibility of level or growth effects on an a priori basis.

At the same time, there is no conclusive evidence to suggest that permanent impacts must exist, or particularly that they exist in the absence of non-permanent effects. Data is a particular issue here. The potential impact of a permanent effect is huge, especially a permanent growth effect. However, unless one has substantial post-Games economic data, one may easily find a significant permanent effect where only non-permanent effects exist. With only a few years of post-Olympic employment data at which to look, the conclusions of Hotchkiss, Moore, and Zobay must be taken as suspect.

In "Bidding for the Olympics: Fool's Gold?" Baade and Matheson reject the notion of permanent effects in favor of a temporary-effect model (2002). Examining employment in Atlanta and Los Angeles in separate regressions, they represent the effect of the Olympic Games over time using a single dummy variable. They account for numerous internal and external factors that influence employment by controlling for general trends in metropolitan development using similar MSAs. With respect to Los Angeles, they conclude that the Olympic Games had a positive impact on employment, but only in 1984. They attribute this short benefit to comparatively lower spending on infrastructure before the Games in Los Angeles. In Atlanta, they find that the employment effect may have been longer, and may have occurred both immediately before the Games and subsequently as a result of greater infrastructure spending. However, the size of the supposed employment impact in Atlanta varies considerably depending on the years represented by the dummy variable, and is not significant at standard levels of confidence. The authors attribute this difference, as well as the general economic success of the 1984 Games, to the ability of Los Angeles to counteract the IOC's monopoly power in selecting a host city.

Like the study of Hotchkiss, Moore, and Zobay (2003), Baade and Matheson's study provides a useful vantage point from which to study the Olympic effect. It provides a simple, single value for each city that easily encapsulates the supposed Olympic effect. Moreover, their analysis can also point to good theoretical reasons to expect short-term Olympic impacts. Assuming that Olympic-related employment does not completely crowd out other types of employment, we can expect that there may be some employment effect in the very short term simply through Olympic hiring. The Organizing Committees themselves hire substantial numbers of workers before and during the Games.

In spite of these strengths of the model presented by Baade and Matheson, their analysis is similarly incomplete to that of Hotchkiss, Moore, and Zobay. Baade and Matheson consider only temporary effects; they do not examine potential permanent effects. Yet some types of Olympic effects, such as the aforementioned impacts on tourism, may be temporary, permanent, or both. Simply assuming that only temporary or permanent Olympic impacts can exist is insufficient for an accurate analysis of how the Games impact employment. Moreover, their methodology of a single effect over a given time period is theoretically questionable. Any model that attempts to explain variation in employment in host cities must use a functional specification that can capture a complex combination of temporary and permanent effects.

In general, the literature on the economic impact of the Olympics and other "mega-events" involves a variety of predictive methods, which sometimes reach considerably different conclusions. Different studies with different methods have suggested that economic effects may exist, and that they may center in large part on Games-related construction and the construction industry. However, at present the body of literature is too small to firmly establish one reliable method of analysis. The two existing studies of Olympic-related employment choose diametrically opposite functional specifications, and they do little to reconcile this contradiction through theoretical justification. Only one cross-Games econometric analysis currently exists, and it only looks at Games hosted in the United States. No pooled time-series analysis of the Games currently exists. Though many studies consider their impact anecdotally, no study has actually controlled for variables such as levels of infrastructure spending. These types of analyses are needed. In short, the substantial holes in existing literature provide ample opportunities for an improved model.

Theoretical Analysis

One may argue that the employment impact of hosting the Olympic Games is only important if it upholds a simple premise: some short-term events may cause or encourage economic activity that has longer-term benefits. Although organization of the Games themselves involves massive hiring, these jobs quickly dissipate after the closing ceremonies. Policymakers know that this will be the case, and econometric analysis may tell us little about this highly predictable turn of events. However, overall employment may also be affected by the influx of tourism, investment, and other Games-related stimuli. Not only do policymakers care about this possibility, but econometric analysis is essential to assessing long-term impacts accurately.

In order to compare multiple Olympic Games effectively, we must surmount a number of major theoretical and practical obstacles. When looking at a single city over a short time period, we can safely hold many macroeconomic factors to be true. However, when examining multiple cities in different countries over different time periods, few macroeconomic factors are constant. Thus, it is particularly important that such a theoretical model be robust to account for all potential macroeconomic influences.

One potential approach would be to model a full economy, including as many relevant explanatory variables as possible in an attempt to explain all factors that motivate employment. This approach is daunting, and given the small number of cities that have hosted the Olympics, it may fail to account for many other relevant factors. Much like existing ex-ante studies, such models would be of little use to other cities, such as cities that were considering bidding for the Games.

A better approach is to base our model on a fundamental equilibrium relationship that holds true across economies regardless of most economic factors. Then, by analyzing deviations from this relationship, we can separate the impact of the Olympics from expected behavior, implicitly given macroeconomic conditions. For this study, I have made the following assumptions:

Equation 1: Assumption of similar employment ratios

$$\frac{\hat{E}MP_{MSA}}{\%POP_{MSA}} = \frac{EMP_{NOTMSA}}{\%POP_{NOTMSA}}$$

Assuming a city and the surrounding country are subject to the same national macroeconomic influences, we would expect the fraction of the city's population that is employed to be the same as the fraction of the population elsewhere in the country that is employed. That is, we assume that other macroeconomic factors that impact employment levels have similar effects on employment both within an MSA and outside it. Through a simple mathematical transformation and some substitution, I get the following relationship:

Equation 2: Calculation for Expected City Employment

$$\hat{E}MP_{MSA} = (EMP_{NATL} - EMP_{MSA})\left(\frac{POP_{MSA}}{POP_{NATL} - POP_{MSA}}\right)$$

This relationship has proven to be highly robust: A simple OLS regression of expected city employment on actual employment shows that it explains over 98% of the variance in employment levels from city to city. At the same time, since this predictor of expected employment separates MSA from non-MSA employment, it can be treated as a random variable. Therefore, I have chosen to use this relationship to analyze how employment in the period surrounding the Olympics deviates from expected patterns of employment.

If the above relationship holds true, then we can expect the following relationship to be true as well:

Equation 3: Calculation for deviation from expected employment

$$DEV_{EMP} = \hat{E}MP_{MSA} - EMP_{MSA} = 0 + \sum_i \beta_i k_i$$

That is, we expect the difference between expected city employment and actual city employment to be 0, but it may vary based on some exogenous factors, including—but not limited to—the Olympic Games. I use this variable for the deviation from expected employment as the dependent variable in my model, and my model takes this general format. Using this definition for my model, I am able to account for more than 60% of the deviation in employment levels from expected values.[4]

I have focused my search for relevant explanatory variables on theoretical critiques of the aforementioned relationship. The first—and most obvious—critique of this relationship is that not all macroeconomic factors affect major cities and the remainder of the nation equally. Major cities have their own governments, and through government spending, they may independently affect economic conditions in the city relative to the rest of the country. Additionally, some types of economies are more conducive to employment in urban areas than others. An economy that is highly agrarian but lacks an industrial base, for example, may have higher employment outside of the city, while a city that is highly industrialized may have lower employment outside the city.

There are multiple approaches to accounting for localized trends. Again, my goal is a model that is simple to use and robust across cities. Thus I have chosen to use a modified fixed-effects model. For each city in my sample, I have assigned a single dummy variable, which takes a value of 1 if the data point is for that city and a value of 0 otherwise. Including dummy variables for each city allows us to account for the important differences in local conditions, as the coefficients for each city represent a constant level of deviation that we can expect under non-Olympic conditions. This modification substantially improves the explanatory power of the

[4] This 60% value is based on the R-squared value of the regression with Prais-Winsten panel-corrected standard errors. Under the assumptions of Ordinary Least Squares, my model accounts for over 80% of variance.

model, but allows the model to remain easily usable for any comparable city. In order to modify the model for its own needs, a city needs only to remove the dummy coefficients and to add its own mean deviation from expected employment as a constant.[5]

The fixed-effects model goes a long-way toward forming a usable model, because we no longer have to worry about a plethora of potential fixed and long-term factors that impact local employment. However, not all factors change so slowly. Some short-term changes in the economy may impact employment in major cities more or less than employment elsewhere in the country. For example, economic downturns may hit major cities particularly hard. For this reason, I have included national GDP per capita as an explanatory variable. I predict that it will be positively correlated with employment deviation. That is, in countries in which GDP is high, major cities tend to have higher employment levels than other parts of the country, and when national GDP increases, major cities tend to benefit more than other areas in terms of employment.

I have chosen to use a lin-log model with respect to GDP because I found it to be a substantially better predictor of variation in my model than raw GDP. Models that predict employment deviation using the log of GDP consistently fit the data better than purely linear models. The theoretical justification for this finding is straightforward. The effect of an increase in GDP of a given size will be larger in a poorer country than in a richer one. A logarithmic scale accounts for this difference by representing equivalent changes in raw GDP as larger when GDP is low and smaller when GDP is higher. Other potential methods for weighting changes in

[5] The other fundamental option for dealing with panel data in this context would be a random-effects model. However, a random-effects model is unsuitable for this model. My panel data is unbalanced, with a different number of observations for each host city. A random-effects model is subject to bias unless the panel is balanced, or additional corrections are made to weight for unequal numbers of cases. In contrast, a fixed-effects model presents no such potential for bias. Additionally, the addition of fixed-effects dummies dramatically improved the goodness-of-fit of my model.

GDP exist, such as the use of percentage change or indexing. These methods allow us to make relative comparisons about the effect of GDP within countries, but they do not allow for differences between more and less developed countries. For this purpose, the use of logarithms is superior to a standard linear specification.[6]

Changes in gross domestic product represent the most theoretically important short-term change in economic conditions that might impact employment. However, there may be other factors whose impact is not fully captured by the fixed-effects model. Likely candidates include wages and prices. To a lesser extent, labor market composition may not be captured by the fixed-effects model, but it changes much more slowly than these other short-term indicators. Comparable data on local labor market composition are not consistently available. Neither are data on wages, although we may expect these to be highly correlated with price levels. However, some international price data are available. Thus, I have chosen to include price levels as an explanatory variable for my model.

In general, comparing prices internationally involves some notion of purchasing power parity, which is distinct from the national exchange rate of a country. The difference between national exchange rates and real purchasing power can therefore be expressed as a "PPP multiplier." When prices are high relative to purchasing power, this multiplier is large, but when prices are low, the conversion factor is small. Since these multipliers represent the conversion to actual exchange rates of local currency units, on which the GDP numbers in my model are based, they represent the best possible way to compare price levels internationally.

[6]One potential concern with this model is the lack of specific theoretical basis for the use of the base-10 logarithmic scale over some other non-linear scale. However, after trying other options such as the natural logarithmic scale, I have concluded that the base-10 log has the highest goodness-of-fit, and is the most appropriate. I also tried to use the percentage change in GDP as an explanatory variable, and I tried using GDP indexed to a value, but neither option fit the model as well as the logarithmic scale. Thus, I am confident that the use of the lin-log model is appropriate.

Prices are theoretically important because their change may represent changes in aggregate demand. The Classical Theory of Economics states that nominal prices will tend to rise when aggregate demand is high, but they tend to fall when aggregate demand is low, since aggregate supply is assumed to be inelastic. If aggregate supply is perfectly inelastic, then aggregate demand will have no correlation with employment. However, if aggregate supply is not perfectly inelastic, then high aggregate demand should correspond with higher demand for labor and therefore increased employment. Therefore, we would expect the general relationship between prices and employment to be positive. However, it is important to note that this is not a sufficient justification for inclusion in the model. My model does not attempt to explain changes in employment levels themselves, but in employment levels in a city relative to expected employment levels. Therefore, only those factors should be included for which there may be a systematic difference in effect between major cities (or Olympic host cities) and other parts of the country. As with GDP, I suspect that changes in aggregate demand will tend to have a greater impact on major cities than on other areas. Therefore, I predict that the relationship between price levels and employment deviation in my model will be positive.

These aforementioned variables should capture most systematic deviation from expected employment that cannot be attributed to the Olympics. Thus, we may now consider how best to capture the employment effect of the Games. Unfortunately, the existing literature on the economic impacts of the Olympics has not agreed on a uniform methodology for measuring employment impacts. While one study simply assumes that employment impacts are permanent (Hotchkiss, Moore, and Zobay 2003), another assumes that they are temporary and assumes that they can be captured by a single dummy variable over a period (Baade and Matheson 2002). Although both of these studies use goodness-of-fit tests to bolster the justification for their model specification, neither has sufficiently shown that other options are either theoretically unsound or empirically improbable. This discrepancy is very troublesome. The question of how long an employment impact lasts is fundamentally inseparable from the level of that impact. One cannot determine the level or the benefits of any potential impact without knowing how it is shaped or how long it will last. If we specify our employment impact variables incorrectly, it may bias our results and lead to incorrect conclusions. Thus, it is important that we have a strong theoretical justification—supported by sound econometric evidence—for any model specification we choose.

I decided that it would be best to begin measuring the size of the effect through the most robust possible means. Then, once I had a sense of the size and timeframe of any employment increase, I could place restrictions on my model and test their viability. Accordingly, I chose to begin with a time-based difference-in-difference approach. By creating dummy variables that correspond to specific periods before and after the Olympics, my model captures any systematic variation from normal Olympic employment levels that cannot be accounted for through fixed effects or the effects of GDP levels on employment. Additionally, visible patterns of dummy variable behavior make it easy to look for the presence of level and growth effects on a more permanent basis.

Figure 1: Stylized representation of statistical model premises

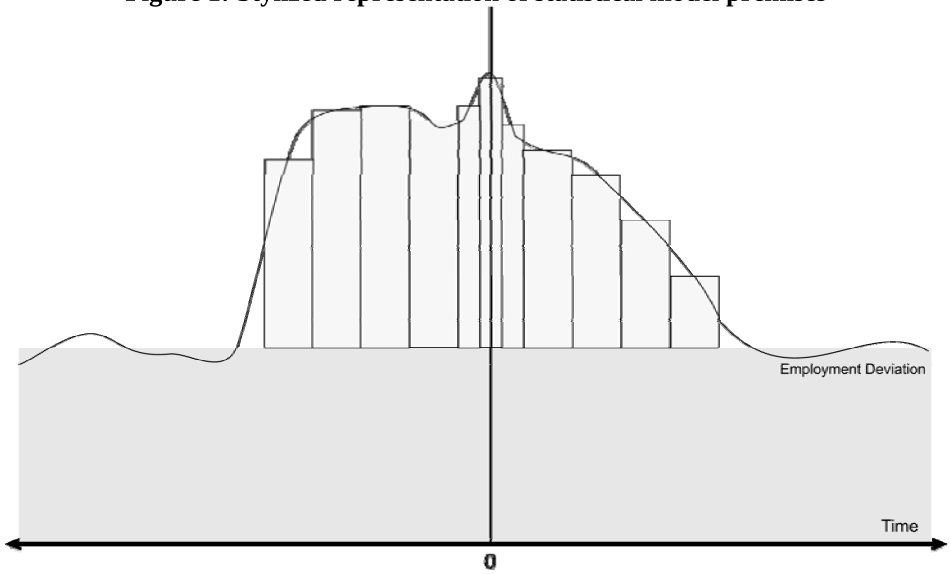

Figure 1 shows a highly stylized representation of this type of model. Note that this is not meant to be an actual representation of the results of the model. Note also that this figure does not show the effect of GDP or prices on employment deviation. However, this figure can be considered to represent possible employment levels for a single hypothetical city surrounding its Olympic Games. The medium-gray shaded area represents the fixed-effect of the city. The lighter gray bars represent the dummy variables for time before and after the Olympics. Despite the relatively complex shape of the change in employment over time surrounding the Games, this type of model can successfully account for most of the variation over the affected period.

Starting from the quarter in which the Olympics were held, I added time-based dummy variables both forward and backward in time for as long as they were positive and significant. As shown in Figure 1, I began by using dummies for shorter periods of time, then used dummy for longer spans of time as I continued. This technique, adopted from Clapp and Hakes (2005), preserves degrees of freedom, while also minimizing the possibility that large standard errors would lead us to conclude that employment effects were insignificant. I established the bounds for inclusion in the model by the premise that significant OLS coefficient estimates were strong grounds for inclusion.[7] Once I had established boundaries for any significant employment effect, I performed a series of F-tests on reasonable restrictions, to end up with a set of dummy variables that explained a high percentage of variance in the simplest way possible. This process of establishing boundaries and making restrictions was repeated once the model was finalized, to ensure that the set of variables presented in the model is the most reasonable set possible.

Once I had set out a time period for non-permanent effects of the Olympics, I attempted to account for possible permanent level and growth effects. I used a dummy variable for the period after the "Olympic effect" began to measure level effects, as well as a dummy variable multiplied against a time counter to measure growth effects. I found both to be highly insignificant. This suggests that the previous study of Hotchkiss, Moore, and Zobay (2003), which found positive and significant growth and level effects, may not have looked at enough data to recognize what was actually a shorter-term effect. Accordingly, I have omitted dummy variables from my model that would consider these types of effects.

The approach I have described easily encapsulates the size of effects related to the Games over time. However, it says nothing about what causes some Olympics to be more successful than others. Thus, I have included two additional variables in my model. The first is an additional variable for the log of per capita GDP, set to 0 during periods that are not subject to the Olympic effect in my model. This variable should test whether the Olympics impact more developed and less developed countries differently. Since no previous studies have looked at employment surrounding Olympic Games outside the United States, we have no strong basis for expecting the sign of this variable to be either positive or negative.

The second variable takes on the indexed value of the total costs (in year 2000 U.S. dollars) of the Olympic Games held in that city during the period of the Olympic effect. During

[7] Note, however, that I do not make the assumptions of standard OLS in reporting my results.

the unaffected period, this variable takes on a zero value.[8] This variable tests whether or not additional Olympic expenditures due to IOC monopoly power or political pressure may affect the size of employment impacts. In their analysis of the Atlanta Games, Baade and Matheson (2002) suggest that increased infrastructure spending may have had a positive effect there. However, the theory of IOC monopoly suggests that the impact of increased spending is negative, since cities are forced to spend more than is economically efficient. Therefore, the expected sign of this variable is also indeterminate. Based on this result, we may shed some light on these competing theories.

As a final consideration for my model, I attempted to examine any historical events that could potentially bias my model. I attempted to identify specific criteria for inclusion of such an event. Any such event must uniquely impact the employment situation in a city, but not the rest of the surrounding country. Furthermore, one must be able to isolate its effect to a specific period of time. Since my model should account for standard macroeconomic activity, I considered only economic and political events of particular regional importance. Potential examples would include localized violence, local political turmoil, a catastrophic event, or another non-macroeconomic event that would tend to impact local employment without similarly impacting national employment. The only event that I found to meet this criterion was the 1992 Los Angeles "Rodney King" riots.

On April 29, 1992 large parts of the city of Los Angeles erupted in looting and violence in reaction to the acquittal of four police officers accused of using excessive force against

[8] The cost variable has been indexed such that the cost of the least expensive Olympics (Los Angeles in 1984) is zero, and all other Olympics take on the value of the percentage difference between their costs and the cost of the 1984 Olympics. I purposely indexed the variable in this way to ensure that my dummy variable estimates would remain unaffected; if the Games were put on cheaply, we would expect the size of the Olympic effect in a given period to be simply the size of the dummy variable in that period plus the log of national GDP. However, if the Games were put on at higher cost, then costs will affect the size of the impact as well. Since costs are still linearly defined, this method of indexation does not affect the goodness-of-fit of the model. However, it improves the readability of the model under the assumption that extra costs are, in general, avoidable.

Rodney King. Several days of chaos resulted not only in nearly 60 deaths and 2,300 injuries, but also in over 10,000 businesses reporting damage. Property damage estimates range from $446 to $750 million—not much less than was spent on the Los Angeles Olympic Games. The net employment losses from the riots have been previously estimated at approximately 75,000 jobs (Spencer 2004). An initial look at the saved residuals from my model suggested that the Rodney King race riots did have a substantial negative impact on employment in Los Angeles. Additionally, other data supported the notion that the riots had important effects on the city: the Census Bureau estimated a decline in the population of Los Angeles in 1994 and 1995, contrary to both the national trend and the trend in Atlanta. Moreover, I found that the overall level of employment in Los Angeles in the fourth quarter of 1996 was lower than it had been over four years earlier, just before the riots occurred. Based on this evidence, I have concluded that the 1992 riots may have had a significant negative impact on employment levels in Los Angeles. Thus, I have defined an additional dummy variable for the effect of these riots.

Determining the period over which the race riots may have impacted employment has proven to be a difficult issue. Although we know when the riots may first have begun to impact employment, there is no clear historical indicator for when the city returned to expected employment levels relative to the rest of the country. In order to find a reasonable value for this end period, I made the assumption that no similar localized events impacted employment in Los Angeles over this time period. That is, all systematic changes in employment over the impacted period that cannot be accounted for by other variables in the model must be the direct or indirect result of the riots. Then I defined the dummy variable over a series of possible periods, extending it at regular intervals and performing F-tests to determine when it no longer significantly improved the model to extend the dummy variable further. I discovered that for every extension of the dummy interval up to the last quarter of the year 2000, I significantly improved the goodness-of-fit of my model relative to any more restrictive assumption. This length for the negative effect of the race riots was substantially longer than I had expected. However, because I lack solid theoretical evidence to restrict the dummy variable to a shorter period of time, I have chosen to report my model using this particular definition.[9]

Based on the theoretical arguments that I have outlaid here, I propose the following general model to capture the impact of the Olympic Games on employment, and to determine how Olympic expenditures and development influence the employment effect:

Equation 4: Model Specification

$$DEV_{EMP} = \beta_0 + \sum_i \beta_i D_{MSAi} + \sum_{j=2}^{4} \beta_j D_{Qj} + \beta_1 \log(y) + \beta_2 PPP + \sum_k \beta_k D_{Ak} + \beta_3 \log(y) D_A$$
$$+ \beta_4 COST * D_A + \beta_5 D_{Riot} + \varepsilon_i$$

Table 1: Description of variables

Variable	Description
DEV_{EMP}	Deviation of actual MSA employment from expected MSA employment level
D_{MSAi}	Fixed effect of membership in ith MSA (Los Angeles is excluded)
D_{Qj}	Seasonal adjustment dummies (Quarter 1 excluded)
$\log(y)$	Log of GDP per capita
PPP	Purchasing power parity multiplier against actual exchange rates
D_{Ak}	Time-period dummy for period *k* in period of Olympic effect *A*
$\log(y)D_A$	Effect of Log GDP/capita on employment in period of Olympic effect *A*
$COST * D_A$	Effect of indexed total expenditures of Organizing Committees on employment in period *A*. (Percentage of L.A. expenditures – 1)
D_{Riot}	Dummy for period of effect from 1992 Los Angeles riots

I submit that this model should explain the primary causes of variance in employment levels generally, and the shape, size, and contributing causes of any Olympic effect.

[9] Although the goodness-of-fit of my model was significantly worsened by restricting or omitting this dummy variable, my coefficients and overall patterns of significance were not generally impacted by it. Therefore, we may conclude that the model is relatively robust to alternative definitions of the variable.

Data Considerations

In order to perform a truly international analysis, substantial data collection from several data sources using different definitions was necessary. Most of these data came from national statistical agencies. This posed a number of obstacles, since comparable international data are not always available, particularly on the city level. However, every possible effort has been taken to ensure that data are as consistent as possible and to eliminate bias.

The first major hurdle that I encountered involved the definition of a city. Although I wish to examine the employment impact of hosting the Olympics on the host city, it is not immediately clear what this involves. A commonsense definition of the term "city" is usually taken to include surrounding suburban areas, comprising a metropolitan area with strong economic and social ties. City boundaries fall far short of this definition; many cities have long outgrown their historical bounds, and political definitions are often largely arbitrary. Any study that looked only at city boundaries would be both prone to bias and would largely miss the point of the study. In response to this issue, statisticians have developed the concept of the Metropolitan Statistical Area (MSA). MSAs generally include both urban and suburban areas, and are meant to be a tool for performing comparable analyses across cities and over time. Since I wish to gauge the effect of the Olympics on the entire metropolitan area, I have opted to use these definitions where available.[10]

Unfortunately, clear-cut metropolitan definitions are not always available. Moreover, each country largely chooses how to define the boundaries of its metropolitan areas. In some cases, these definitions are largely arbitrary. Even where specific criteria for MSA boundaries are outlined, they are often defined in very inexact fashion. In the United States, for example,

[10] Throughout this paper, I use the terms "city" and "metropolitan area" to refer to this concept of the overall metropolitan area. These terms should in all cases be interpreted as equivalent to the definitions used in my model.

MSA definitions fall along county boundaries based on the percentage of county residents that commute to a city center (Office of Management and Budget 2000), even though large areas of the county may have few or no commuters. Conversely, portions of a county that have high commuter populations may not be counted as part of an MSA because of the presence of large outlying areas. The fixed-effects model is robust to slightly different metropolitan definitions so long as those differences remain constant. Fixed-effect dummies control for all constant differences between cities. However, if substantial Olympic-related employment occurs outside of the metropolitan boundaries defined in the model, then our estimates of the Olympic effect will be downwardly biased. Therefore, it is important that we be confident in the relative consistency of metropolitan definitions used in the model.

In the cases of Atlanta and Sydney, sufficient data on employment were available based on MSA definitions that were consistent across time. Although there may be some questions about the comparability of MSA definitions, in these cities a clear MSA definition exists for which data was available. However, in the other four cases this definition became more troublesome. In Seoul, a consistent MSA definition was available, but employment data were unavailable for any period before 1989. This means that we have no data on employment in Seoul prior to or during the Olympic Games. At that time, South Korea was undergoing a transition to democracy; a lack of statistical records may indicate that the previous autocratic regime was unconcerned with localized employment. Since no comparable data were available, only available data was included in the data set.

In Los Angeles, data for the MSA as it is currently defined is available only from 1990 forward. Given that the Los Angeles Olympics took place in 1984, this is clearly insufficient for our purposes. However, a second statistical definition exists for Los Angeles, the "Los Angeles-

Long Beach-Glendale, CA Metropolitan Division." Data for this area were available to 1978. Further research on the history of MSA definitions in Los Angeles has led me to conclude that the use of the Los Angeles Metropolitan Division in my model is reasonable, and is unlikely to cause substantial downward bias.[11]

In Barcelona and Athens, in contrast, no employment data based on MSA definitions are available at all. Spain and Greece break their employment data down by region and province, but not by metropolitan area. Fortunately for the purposes of this model, strong proxies exist that can be used in place of standard MSA definitions. In Greece, the periphery (state) of Attica closely matches the boundaries of the metropolitan area. Although there is some dispute over the exact population of the Athens metropolitan area, we can safely conclude that the state of Attica encompasses Athens and its suburbs, and that it matches the metropolitan area closely enough to avoid substantial bias.[12] Similarly, the Spanish province of Barcelona—of which the city Barcelona is capital—encompasses the Barcelona metropolitan area and matches it closely enough to avoid substantial bias.[13]

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[11] The Los Angeles MSA currently includes two counties in Southern California: Los Angeles County and Orange County. The Los Angeles Metropolitan Division consists of Los Angeles County only. In the 1980 Census, the Los Angeles MSA was defined along the same boundaries as the current Metropolitan Division. Orange County, along with other counties in Southern California, was considered part of the Los Angeles Consolidated Metropolitan Statistical Area, a separate definition subject to less stringent standards of economic ties. The current definition of the Los Angeles MSA was adopted only in 2000 (U.S. Census Bureau 2005).

[12] The *Wikipedia* entry for Attica reports that "about 3,750,000 live in the periphery, of which more than 95% are inhabitants of the Athens metropolitan area" (2006). Similarly, the *World Gazetteer* estimates the 2006 population of Athens at approximately 3.77 million (Helders 2006). However, Greece's own population estimates are somewhat more conservative; it reports the estimated population of "Greater Athens" in 2004 as 3.37 million, or approximately 86% of the overall population of Attica (General Secretariat of National Statistical Service of Greece 2006).

[13] The Spanish Municipal Register reports the official population of Barcelona province as of January 2005 to be approximately 5.23 million (National Statistics Institute 2006). However, the Spanish government releases no figures on the population of metropolitan areas. As with Athens, independent population estimates vary; the *World Gazetteer* pegs the current population of the Barcelona metropolitan area at 4.86 million, but German demographer Thomas Brinkhoff estimates it to be only 3.8 million (Helders 2006:; Brinkhoff 2006). The United Nations, seemingly splitting the difference, estimates the 2003 population of the metropolitan area to be 4.4 million (United Nations 2004).

In general, no single definition of a metropolitan area will be perfect or consistent, especially across countries. In my research, I found that where official definitions of Metropolitan Statistical Areas did not exist, unofficial estimates of metropolitan populations from independent sources sometimes varied significantly. Even where metropolitan definitions exist, these demographers sometimes report metropolitan populations that are substantially larger than official figures. However, the fixed-effects model is relatively robust to differing definitions. As long as the specified definition of the metropolitan area covers the region in which we expect Olympic employment to occur, differences in metropolitan definitions that lead to systematic differences in employment levels are accounted for by fixed-effects dummies. Therefore, the time-period dummies remain unbiased.

Unfortunately, not all potential sources of bias are unavoidable. To the extent that any metropolitan definition does not capture Olympic-related employment outside its bounds, estimates will be biased downward. As Hotchkiss, Moore, and Zobay note, Olympic venues sometimes occur some distance from the host city; during the Atlanta Olympics, some events were held as far away as Savannah, Georgia (2003). No model that uses metropolitan area data can fully account for this type of situation. Conversely, the dummies for the effect of GDP and prices may be biased (toward 0) if the definition of a metropolitan area is too broad, since these variables will capture some effects that are not associated with the city itself. These are the most likely sources of bias in my model. However, given limited statistical data on employment, the use of metropolitan areas remains the most consistent way to model the effect of the Olympic Games on employment. Moreover, since both of these potential biases are downward, significant findings are likely to be more significant than suggested by regression results.

Despite this fact, I have taken all reasonable measures to ensure that metropolitan definitions are as consistent as possible.

Table 2: Information on unbalanced panel data[14]

City	Data Start Period	Quarters Relative to Games	Metropolitan Definition
Los Angeles	1978 Q1	-26 to +84	Los Angeles-Long Beach-Glendale, CA Metropolitan Division
Seoul	1989 Q1	+3 to +69	Seoul Metropolitan Area
Barcelona	1979 Q1	-54 to +52	Province of Barcelona
Atlanta	1990 Q1	-26 to +36	Atlanta-Sandy Springs-Marietta, GA Metropolitan Statistical Area
Sydney	1979 Q1	-86 to +20	Sydney Metropolitan Area
Athens	1998 Q1	-26 to +4	Periphery of Attica
Total	N = 486		

In addition to data on metropolitan and national employment levels, we need data on metropolitan and national population to calculate expected employment, and therefore the deviation from expected employment. In the interest of consistency and regular availability, I use civilian (working-age) population. National statistical agencies typically release reports that specify numbers for civilian population, civilian labor force, and civilian employed and unemployed. The lower-bounds of working age are variably defined as 15 or 16, depending on the country. This is not a problem; the fixed-effects model can account for this difference. It can also account for most other differences in statistical definitions and measurements, so long as those statistical definitions are applied consistently over time.

However, the United States does not release data on civilian population with its employment numbers. Rather, the United States relies on population data from the decennial Census, which as the name suggests, is compiled only every 10 years. Decennial Census data break down the population of each MSA by age. Additionally, the Census Bureau releases

[14] Additional information on sources is available in the Data Appendix.

annual estimates of MSA populations, which take into account both decennial data and proxy data such as tax return and Medicare data (U.S. Census Bureau 2004). From these estimates, I was able to interpolate estimates of civilian population.[15]

In summary, ensuring comparable data for employment and population levels required several assumptions and transformations. These assumptions were made with particular care to minimize potential sources of bias. In particular, they rely heavily on the nature of the fixed-effects model to account for persistent differences in definitions and measuring techniques.

However, most other data, including data on GDP and prices, are simply unavailable on the local level. While some countries, such as the United States, release ample data on GDP and prices at the metropolitan level, other countries do not compute local GDP or prices at all. For this reason, I have chosen to use national GDP and price data as explanatory variables in my model. Most countries release data on GDP quarterly. It is typically expressed in local currency units adjusted to different time bases, and sometimes with no clear time base. I had hoped to use these data, but without a clear way to adjust into a uniform currency at a uniform time base, these conversions were likely to be inexact, and therefore prone to bias. I opted instead to use annual data on GDP per capita and prices from a consistent source, interpolated quarterly. I also extrapolated these data forward for the few quarters between the most recently reported annual data points (2004 values) and the most recently reported quarterly employment data (3rd Quarter 2005 in all cases). Since GDP and prices tend to change in relatively linear fashion, this is not a

[15] Annual population estimate data are available for both the Atlanta MSA and the Los Angeles Metropolitan division, from which I could interpolate quarterly values. However, annual Census Bureau estimates do not break down the population by age. To compute civilian population estimates, I linearly interpolated the percentage of the population of civilian age from decennial Census data. As one would expect, these values changed little from Census to Census, and they should reasonably be expected to change in a highly linear fashion. Then I multiplied these percentages against the estimated overall MSA population numbers to get quarterly estimates of MSA civilian population that would be comparable to the statistics released by other countries. Although this represents a combination of multiple estimations, these transformations should not introduce any systematic bias into the model. A more precise explanation of this method is available in the Data Appendix.

major potential source of bias. However, it may be a slight source of inefficiency and autocorrelation, since linearly interpolated data may fit the model somewhat less well than true quarterly data. My GDP data are expressed in base 2000 U.S. dollars and come from the World Bank's World Development Indicators database.

My data on Olympic expenditures comes from Preuss (2004). His recent work to compile data in a consistent fashion across Olympic Games finally makes inter-Games comparisons possible. According to Preuss, officially stated numbers on Olympic expenditures are often subject to a number of biases. In particular, Organizing Committees typically attempt to minimize their officially stated profit margins, to avoid paying a portion of the profits to the IOC. To this end, they frequently make unrealistic assumptions, such as assuming that the value of Olympic construction depreciates fully over the span of the Games. This increases the officially stated cost of the Games, even though Organizing Committees are often able to sell assets such as Olympic villages. They also engage in many projects that are not strictly necessary for the Games, for auxiliary reasons such as urban renewal, public infrastructure development, and civic pride. Preuss has subtracted these costs from his figures for operational expenditures, and he has corrected officially stated profit margins to account for these differences. However, these corrections are not suitable for the purposes of our study. I am interested in the effect of all Organizing Committee expenditures on employment, not just those that are most directly related to the Games. Therefore, I have chosen to use the uncorrected figures, computed by adding the difference between the officially stated profit margin and Preuss' corrected profit figures to his corrected expenditures figures. In fact, I found that the use of these uncorrected figures in my model was a significantly better predictor of variation in employment deviation than the use of Preuss' corrected figures. Thus, I believe that these figures provide the most accurate possible picture of how the cost of an Olympics influences its employment effect.

Results

I have used the assumptions of Ordinary Least Squares when defining my model, specifically when determining the bounds of the Olympic effect period. However, we have strong theoretical reasons to suspect that two primary assumptions of the Ordinary Least Squares do not hold in this model. In particular, we have reason to believe that both heteroscedasticity and autoregressive autocorrelation will be present. Heteroscedasticity occurs when the values of the error term do not exhibit a constant variance throughout their distribution. When the assumption of homoscedastic error terms is violated, Ordinary Least Squares produces estimates that are unbiased but inefficient. We may reach incorrect conclusions based on the confidence intervals that are too large or too small. Autocorrelation occurs when the values of the error term exhibit non-zero covariance. Autoregressive autocorrelation occurs when this covariance is related to the relation between terms in a time-series. That is, autocorrelation is autoregressive when one can predict the error term for a given observation with better-than-random accuracy by examining the error term at some other point. Like heteroscedasticity, autocorrelation leads to Ordinary Least Squares estimates that are unbiased but inefficient.

The theoretical basis for the existence of heteroscedasticity in my model is straightforward. The dependent variable in the model, the deviation from expected city employment, is a function of actual city employment levels, national employment levels, city population, and national population. The nature of this definition and of the fixed-effects model controls for most non-localized macroeconomic effects. However, in larger cities, employment levels will naturally exhibit greater variance over time than in smaller ones, due to business cycles and similar events. While most of these cycles are national in scope, and therefore eliminated from our model, some local cycles and effects do exist. To the extent that localized effects are not captured by the model, we should again expect that they will vary more widely in larger cities. Therefore, we can expect that the variance of error terms in this model will be larger in larger cities, introducing heteroscedasticity.[16]

Similarly, we can establish a strong theoretical likelihood of autoregressive autocorrelation. Typically, autoregressive autocorrelation occurs in time-series models because of business cycles and other short-term macroeconomic effects. When these are not fully accounted for in a model, they cause the error terms of that model to exhibit positive covariance between successive values, the condition known as AR(1) autocorrelation. Simply put, this means one may be able to predict the error term of a given observation by looking at the error term of the observation before it. As with heteroscedasticity, we should expect this in our model because the model is inherently unable to account for localized business cycles.[17]

In a pooled time-series data set such as this one, correcting for heteroscedasticity and autocorrelation present special challenges. Correcting for autoregressive autocorrelation in multiple panels requires software that can separate panels as distinct time-series, while still estimating coefficients collectively. Moreover, we cannot assume that the degree of autocorrelation in the model will be the same across countries. Different cities will have more or less prominent local macroeconomic cycles; for example, in small countries, local and national macroeconomic cycles may be much less distinct. Fortunately, modern statistical packages such

[16] For example, by saving the residuals my regressions on individual cities, I found that the standard deviation of the residual values for Atlanta (7.90) was less than one-sixth the standard deviation of residual values for Los Angeles (50.80).

[17] Consistent with this explanation, a Durbin-Watson *d* test performed on the aggregated OLS regression returned a value of .925, which is indicative of a high likelihood of positive autocorrelation.

as Stata have adapted correction procedures to address these issues. I have chosen to use the Prais-Winsten estimation method, which corrects both for heteroscedasticity and AR(1) autocorrelation. This method is the only generally available method that is unbiased, efficient, and that allows one to correct for AR(1) autocorrelation without dropping the first observation in each panel, as well as the first observation for which each dummy variable is defined. Given the importance of dummy variables to this model, the Prais-Winsten method is clearly the best estimator for this situation.[18]

Table 3: Model with GDP effect and cost effect omitted (Prais-Winsten Panel-Corrected Standard Errors

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
Constant	-388.236	152.3676	-2.55	0.011	-686.871	-89.6012
D_{Seoul}	-36.0833	56.90546	-0.63	0.526	-147.616	75.44935
$D_{Barcelona}$	56.16977	53.36214	1.05	0.293	-48.4181	160.7577
$D_{Atlanta}$	5.312273	25.89967	0.21	0.837	-45.4501	56.07469
D_{Sydney}	-108.772	33.54385	-3.24	0.001	-174.516	-43.0269
D_{Athens}	-31.6812	52.72672	-0.60	0.548	-135.024	71.66131
D_{Q2}	-35.929	3.454301	-10.40	0.000	-42.6993	-29.1587
D_{Q3}	-36.783	4.191797	-8.78	0.000	-44.9988	-28.5673
D_{Q4}	-34.7018	3.512921	-9.88	0.000	-41.587	-27.8166
$\log(y)$	313.4613	83.41797	3.76	0.000	149.9651	476.9575
PPP	169.8259	55.76635	3.05	0.002	60.52583	279.1259
$D_{\Pr e1724}$	22.66884	10.52223	2.15	0.031	2.045655	43.29202
$D_{\Pr e1316}$	41.5291	13.25442	3.13	0.002	15.55091	67.50728
$D_{\Pr e912}$	51.04048	14.22558	3.59	0.000	23.15886	78.9221
$D_{\Pr e58}$	51.77813	14.84426	3.49	0.000	22.68392	80.87234
$D_{\Pr e34}$	55.08608	16.49894	3.34	0.001	22.74875	87.42341
$D_{\Pr e12}$	51.80605	16.84581	3.08	0.002	18.78887	84.82322
D_{Post0}	60.7326	18.67437	3.25	0.001	24.13151	97.3337
D_{Post12}	42.84612	18.30851	2.34	0.019	6.962094	78.73015
D_{Post34}	47.45921	20.87757	2.27	0.023	6.539917	88.37851
D_{Post58}	21.22429	19.87195	1.07	0.285	-17.724	60.1726
$D_{Post912}$	31.19619	19.6946	1.58	0.113	-7.40451	69.79689

[18] More information on the methods used is available in the Data Appendix.

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
$D_{Post1316}$	41.13529	19.58279	2.10	0.036	2.753718	79.51686
$D_{Post1724}$	30.58315	18.10299	1.69	0.091	-4.89806	66.06436
$D_{Post2532}$	23.34651	16.8522	1.39	0.166	-9.6832	56.37621
D_{Riot}	-142.34	28.34425	-5.02	0.000	-197.894	-86.7866
rhos = .8389917 .1643236 .855826 .5534502 .6426289 .6648179						
R-squared = .5578						

Table 3 shows the results of the Prais-Winsten regression with all time-series dummy variables included in the model. In this regression, I have removed the variables for the effect of GDP per capita and the effect of Olympic costs on employment. That is, this model assumes that those variables have no impact on employment in the Olympic period. Note that although the coefficients for all time-series dummies are positive, they are only significant after the Olympics for a period of one year. Despite the findings of the Ordinary Least Squares model, on which basis these time-series variables have been selected, these findings may suggest that the effect of the Olympics diminishes soon after the Games. In contrast, the coefficients on all dummy variables for the period before the Games are significant. This suggests that employment levels may increase long before the Games, perhaps in response to construction and increased international visibility.

In addition to these findings about the general length of the Olympic impact, we can reach two other important conclusions. This model suggests that higher prices and higher levels of income are correlated with higher than expected employment in cities. When prices rise—presumably because of an increase in aggregate demand—major cities may benefit disproportionately. Similarly, major cities benefit disproportionately when GDP per capita levels are higher. These findings are robust to the alternative specification of the model presented in Table 4. However, the coefficient of the effect of GDP is not consistent in the regressions of individual cities. This suggests that most of the employment benefit accorded to major cities may be the effect of being located in a wealthier country, not the effect of short term changes in income. In other words, a small change in income may not improve employment in the city much more than it would elsewhere. However, higher development may be correlated with higher employment levels in major cities relative to other areas.

The model presented in Table 3 begins to tell us some important things about the Games. It suggests that there is a positive impact on employment associated with the Games. However, it says nothing about why some Games may be more successful in spurring employment than others. I have chosen to examine two potential issues: whether GDP impacts the size of the Olympic effect, and whether the amount that the Organizing Committee spends on the Games impacts the size of the Olympic effect. We can begin to examine these questions by incorporating these variables into the model. I present my results as Table 4.

Table 4: Overall model results (Prais-Winsten panel-corrected standard errors)

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
Constant	-520.3918	163.1773	-3.19	0.001	-840.2135	-200.5702
D_{Seoul}	40.13243	57.41041	0.70	0.485	-72.3899	152.6548
$D_{Barcelona}$	136.727	53.37239	2.56	0.010	32.11904	241.335
$D_{Atlanta}$	8.986809	18.55552	0.48	0.628	-27.38134	45.35496
D_{Sydney}	-20.0207	40.67061	-0.49	0.623	-99.73363	59.69223
D_{Athens}	74.8317	58.24061	1.28	0.199	-39.3178	188.9812
D_{Q2}	-37.20984	3.717097	-10.01	0.000	-44.49522	-29.92447
D_{Q3}	-39.54565	4.472246	-8.84	0.000	-48.3111	-30.78021
D_{Q4}	-34.62402	3.781163	-9.16	0.000	-42.03496	-27.21307
$\log(y)$	395.4272	102.9304	3.84	0.000	193.6873	597.1671
PPP	130.6622	40.69414	3.21	0.001	50.90311	210.4212
$D_{\mathrm{Pr}e1724}$	15.35567	68.79982	0.22	0.823	-119.4895	150.2008
$D_{\mathrm{Pr}e1316}$	35.6631	69.82348	0.51	0.610	-101.1884	172.5146
$D_{\mathrm{Pr}e912}$	44.38181	69.8816	0.64	0.525	-92.58361	181.3472
$D_{\mathrm{Pr}e58}$	43.54311	69.88026	0.62	0.533	-93.41969	180.5059

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
D_{Pre34}	45.80051	70.26715	0.65	0.515	-91.92058	183.5216
D_{Pre12}	43.16358	70.0091	0.62	0.538	-94.05173	180.3789
D_{Post0}	53.6068	70.46648	0.76	0.447	-84.50496	191.7186
D_{Post12}	32.94675	70.04449	0.47	0.638	-104.3379	170.2314
D_{Post34}	38.21886	70.47711	0.54	0.588	-99.91373	176.3515
D_{Post58}	9.099189	69.76511	0.13	0.896	-127.6379	145.8363
$D_{Post912}$	17.72987	69.96366	0.25	0.800	-119.3964	154.8561
$D_{Post1316}$	21.30803	70.34426	0.30	0.762	-116.5642	159.1802
$D_{Post1724}$	3.005768	70.10033	0.04	0.966	-134.3883	140.3999
$D_{Post2532}$	-12.34345	70.22797	-0.18	0.860	-149.9877	125.3008
$\log(y)*D_A$	99.83055	47.37676	2.11	0.035	6.973802	192.6873
$COST*D_A$	-74.13723	16.94452	-4.38	0.000	-107.3479	-40.92658
D_{Riot}	-179.5499	25.30006	-7.10	0.000	-229.1371	-129.9626
rhos = .7241404 .0739413 .7677738 .5223441 .4213575 .6767149						
R-squared = .6099						

Table 4 shows my complete model. This model includes all of the theoretically important variables described in Table 1. One may quickly note that all of the time-period dummy variables in the model are insignificant under this model specification. This, however, does not imply that the Olympics have no positive and significant employment effect. The variables for Olympic costs and the effect of GDP on Olympic employment are defined over the same period as the time-period dummies; the time-period dummy variables in this model describe only that variance which cannot be attributed to these other factors. To compute the expected employment effect for a given city for a given period of time using this model, one must use the following formula, where costs are indexed as described in the data appendix:

Equation 5: Transformation for Computing Olympic Effect at time period k

$$\Delta EMP_k = D_{Ak} + \beta_3 \log(y) + \beta_4 COST$$

By testing this model with some sample cities at sample time periods, one will find that for most cities and most Games, the expected change in employment levels will be positive, in keeping with the findings in Table 3. In fact, the coefficients of the time-based dummies are fairly similar to those in the previous model. The fact that the standard errors of the dummy estimators have substantially increased indicates the possible presence of multicollinearity introduced by specifying a variable for income over the Olympic effect period in addition to the variable for income that is defined generally.

The model in Table 4 suggests that in general, higher income levels are associated with a larger Olympic employment effect. The Olympics may induce more employment in wealthier countries than in less wealthy countries. This runs contrary to the notion that a boost of a fixed size will impact poorer countries more than wealthier countries. However, it suggests that in the case of the Olympics at least, wealthier countries may be able to take advantage of the opportunities presented by the Games more effectively. For example, cities in more developed countries may be better positioned to take advantage of the increased international exposure their cities receive as a result of the Olympics. Cities in more developed countries may be more attractive tourist destinations. Alternatively, cities in more developed countries may be better able to plan the Games to maximize employment, for reasons such as better existing infrastructure. However, these results clearly suggest that developing countries seeking to host the Games may not get as large of boost as they expect. These issues warrant substantial further research.

Perhaps more importantly, the findings of the model in Table 4 also indicate that higher levels of infrastructure expenditures have a strong negative effect. This last finding is particularly noteworthy. The cities that seem to receive the greatest employment benefit from the Olympic Games are those that spend the least. This finding is highly significant, and proved to be highly robust. This suggests that although the Games have a positive impact, that impact is largest when Olympic expenditures are kept as small as possible. For example, employment gains may be diminished when politicians use the Games as a cover to engage in expensive projects that would not otherwise be feasible.

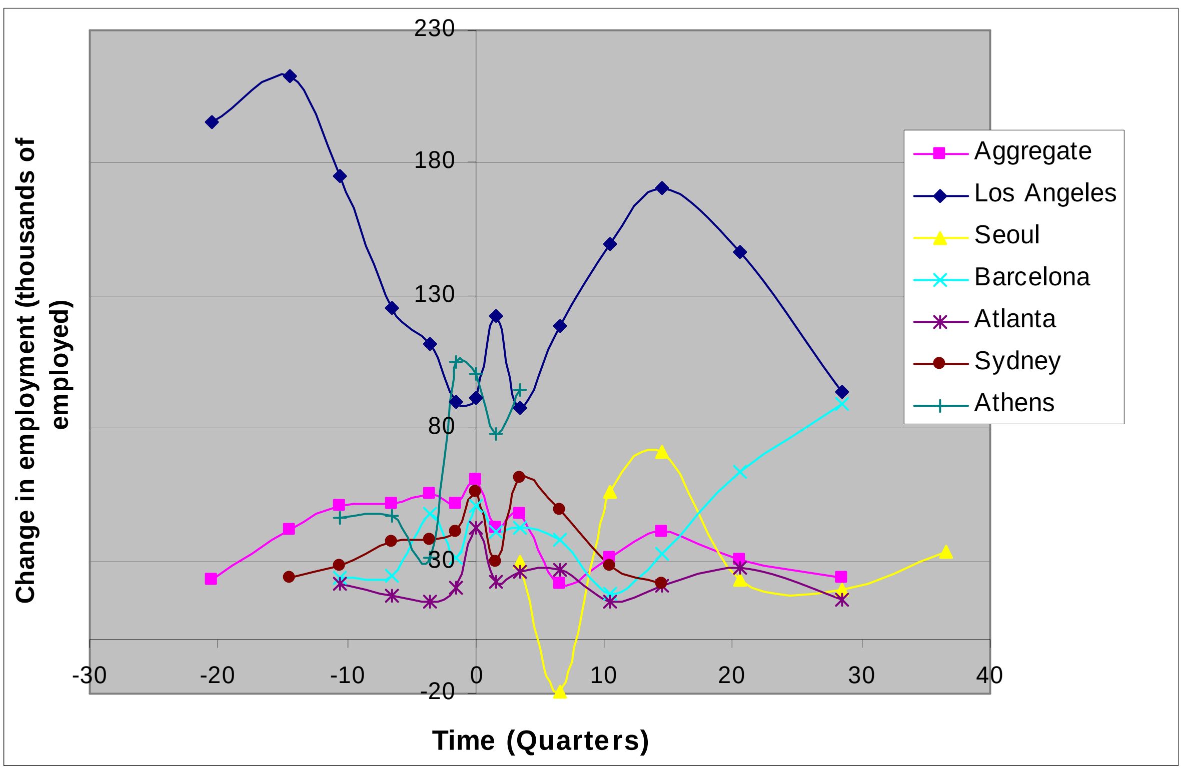
I have defined the variable for Olympic costs as an index. The cost of the Los Angeles Olympics is defined as 0, while the costs of all other Games are defined as the percentage in real terms by which the Games were more expensive than the Los Angeles Games. If one assumes that the Los Angeles Games represent a fair baseline for producing the Games at lowest reasonable cost, then a potential host may easily interpret the results of this model. For example, if an Olympic Committee doubles its costs through excessive construction, the model predicts that it may do so at an opportunity cost of nearly 75,000 jobs over the entire period of the Olympic effect. Such a large cost may be a strong incentive for Olympic planners to plan relatively sparse Games.

These results give credence to the notion of IOC monopoly power. Given the strong negative effect on employment shown in this model, one would expect that in general, cities would attempt to keep their costs down. However, they have not; the Sydney and Athens Organizing Committees spent more than twice what the Los Angeles committee spent, in real terms. A likely culprit is the bidding process, which has become much more competitive; eleven cities bid for the 2004 Olympics, a record high in the modern era (Preuss 2004). IOC monopoly power over granting the right to host the Olympics may cause cities to pursue excessively lavish Olympic plans, with strongly negative results.

These two models help to establish strong conclusions about the impact of hosting the Games. They also may help cities that are considering a bid to host the Olympics to gauge the expected impact on the local economy. However, they cannot establish as rich a portrait of the

Olympic employment effect as one gets by examining the employment impact associated with each individual Olympic Games. Using the same methods as my overall model, I have performed these regressions, establishing different bounds for the length of the employment impact for each city. I omitted the unnecessary fixed-effect dummies, as well as the variables for the effects of GDP and costs over the Olympic period, to produce results that simply represent the estimated size of the Olympic impact over time. Figure 2 charts the results of these regressions over time. For space and clarity reasons, I have not provided results tables here. However, the full regression results, providing Newey-West heteroscedasticity and autocorrelation-consistent standard errors, are available in the Data Appendix.

Figure 2: Employment impact results: Individual cities' regressions



From Figure 2, we can reach two important conclusions. The first conclusion is that the Olympic effect seems to have been considerably larger in Los Angeles than in any other city in the model.[19] This lends additional evidence to the theory of IOC monopoly. It also helps to explain why infrastructure expenditures may be so negatively correlated with the size of Olympic effects. The second conclusion is that there remain substantial differences in the overall shape of the Olympic effect from city to city. Bearing in mind that the values of these dummy variables for each city are positive and generally significant, there is still strong evidence that the Olympic Games lead to increased employment. If anything, this finding suggests that even this relatively robust model may not account for many local effects. Only by looking at a pooled time-series sample over a long period of time do we get an aggregated model that is significant and roughly matches an expected trend line. Perhaps this is why existing studies of individual Games have not found evidence of the same type of employment effect that I find in this model.

Critiques and Suggestions for Further Research

Data availability remains the primary limitation on a more thorough and robust analysis of the Olympics. Although every effort has been made to make data across countries comparable to each other, substantial trouble spots remain. Since differences in national statistical methods do not change over time, the fixed-effects model should prevent these differences in national statistical methods from biasing our estimates of the Olympic effect. However, more standardized international statistical definitions and availability would allow us to consider other variables—such as labor market composition—that are potentially theoretically important.

[19] This is despite the fact that we have defined a variable for the effect of the 1992 L.A. riots, which should tend to increase the size of the constant value for Los Angeles by capturing the downward trend, and therefore decrease the size of the estimated Olympic employment effect. Note also that many of the dummy values in the L.A. model are marginally significant. This may be the result of multicollinearity associated with defining the riot dummy variable over a large part of the period in which we expect no Olympic effect.

More uniform definitions for metropolitan statistical areas are sorely needed for international models such as this one to be improved. At present, almost all available labor data is compiled on the basis of these metropolitan statistical areas. More detailed breakdowns by county, city, or town are often unavailable. This problem is most pronounced in Greece and Spain, where employment statistics broken down by MSAs are simply unavailable. In no cases did I have a choice of definition for a given city in my sample. Since all statistical definitions used in the model encompass the primary area over which we would expect employment effects, this should not be a substantial source of bias in the model. However, if data were readily broken down into smaller units, studies such as this one could do more to eliminate potential sources of bias more rigorously.

Time-series interpolation was another major issue in my study. Although most countries report quarterly or monthly employment and population levels, the United States does not report civilian population estimates from labor force surveys. Forming civilian population estimates for Atlanta and Los Angeles required me to linearly interpolate the percentage of the population in each MSA of civilian age over the entire decade span between censuses, and to interpolate annual population estimates to predict quarterly values. For GDP per capita and price data, the decision to linearly interpolate all quarterly values from annual national GDP per capita figures was also done out of practical necessity. Although quarterly national GDP levels were available from the OECD, they were not available for all years, and they were only raw GDP levels in local currency units with a base only defined to the year, not the day (2006). Converting these values into some standardized currency would have been a substantial challenge, and would likely have been prone to substantial bias. Each decision to interpolate data was made based on a reasonable assumption of linear behavior, and should not be a source of bias in the model.

However, by potentially smoothing out patterns in the data, these interpolations may contribute to autocorrelation in the model.

Finally, data limitations have forced me to use an unbalanced pool for my pooled time-series analysis. Initially, I had planned to include data from each city for a uniform time period before and after the Olympics. With the exceptions of Seoul and Athens, this was feasible, and seemed to be the approach that was least susceptible to bias. However, when I limited cases to what I thought to be reasonable period of time—five years before the Olympics and seven years after—none of my results were significant. Given the long period over which the Olympic Games seem to positively impact employment in my final model, this finding is plausible. It may also explain why previous studies, which often focused on similar time periods before and after the Olympics, found little or no positive employment effect surrounding the Games. The fixed-effects model allows me to use an unbalanced aggregate model without biasing my results. However, in my regressions for individual cities, the limitations of not enough data points become more apparent. Although all cities in the model show a positive Olympic effect, my regressions for Seoul and Athens are not significant at standard levels of confidence. This indicates the probable presence of multicollinearity due to the small number of data points that are not in the period over which the dummy variables are defined.

In general, these data problems suggest that there is a substantial need for international cooperation. If studies of major international phenomena have some value to the international community, then statistical agencies must do more to make their statistics and definitions comparable. There has been a steady improvement in this regard; I found several instance of new survey data or improved statistical methods that I could not use because they had only recently become available. Moreover, I found that international organizations such as the OECD and the World Bank have made great strides in providing comparable data across countries on the national level. I believe that a renewed focus on providing comparable MSA-level data will reap substantial rewards through improved and increased studies of phenomena such as the Olympics. Thus, over time, I suspect that the potential quality of analyses such as this one will increase.

In addition to limitations that impact the variables in my model, other data limitations have kept variables out of my model. Any of a number of variables related to the Olympics themselves may be theoretically important. Although I have only considered expenditures, many other actions taken by Organizing Committees may substantially impact the length or size of the Olympic impact. Unfortunately, given readily available data, it is impossible to examine more than one of these variables because they do not vary over time; they are only reported as final numbers for each Olympics. By looking at Olympic on patterns of expenditures over time—such as quarterly accounting reports—a future model may be improved in several ways. In addition to giving us the ability to include other variables related to the Olympics in the model, this improvement would allow us to consider whether employment gains occur because of actions taken by the Organizing Committees or simply because of the presence of the Olympics.

Perhaps the strongest and most important critique of my model comes from my series of disaggregated models for each city. Although these models all show positive employment effects surrounding the Olympics, the size and duration of these effects varies substantially. The variables on expenditures and income levels capture some, but not all, of these differences. Moreover, my aggregated model cannot individually account for the different periods over which different cities showed increased employment. It can only average them to find some "typical" effect, which other cities may apply to their situation to predict changes in employment surrounding the Games. However, given these difficult-to-explain variations, it is a fair critique to suggest that few or no cities will exactly match this pattern.

The model I have created represents a clear improvement over existing models of the Olympic employment effect. The fact that it consistently finds larger and longer-lasting patterns of Olympic-related employment than existing models strongly suggests that existing models need to examine employment data over a longer time period. However, this model must be seen only as an exploratory model. It cannot yet account for many factors that may make a particular Olympic more or less successful than expected. It is my hope that these results will spur further research in this area. I believe that further research on the theory of IOC monopoly is particularly necessary. As more comprehensive and comparable data become available, we may begin to get a clearer picture of exactly what causes some cities to gain more jobs than others surrounding the Olympics.

Conclusions

Not only are the Olympics a massive phenomenon, they are a complex one. They impact local employment—and local economies more generally—by many different vectors. This study only begins to address them. The effects of Olympics costs and per capita income levels are two such issues, but they are far from the only ones. Many other questions warrant further study, such as the effect of IOC monopoly, along with the specific breakdown of how different boosts—urban development, tourism, and increased international visibility—combine to spur employment. We can only speculate on these issues using existing models. A complete model of the “Olympic effect” may only be achieved when these issues have been settled.

Nonetheless, this model provides a strong basis from which to estimate the effect of the Olympic Games on employment. Through the carefully considered use of a fixed-effects framework, it has made cross-Games analysis feasible. It provides more robust and more conclusive evidence of the positive impact of the Games on employment than other studies to date. It takes the first steps in examining econometrically the myriad factors that may impact the Olympics. It is a major step toward filling the serious gap in econometric analysis surrounding the Olympic Games.

Unlike ex-ante studies of the effects of the Olympics, econometric studies must rely on available data to measure the effects of the Games. International data are often extremely difficult to compare; different countries often use slightly different statistical methods. Many econometricians shy away from analysis involving data that are not easily comparable. For these reasons, too few studies exist of international phenomena such as this one. In their place, host cities and potential bidding cities rely on ex-ante studies that may be biased and unreliable. However, I have shown that the creative use of a fixed-effects model can account for statistical and macroeconomic differences in a robust way. Moreover, in doing so, I have shown that the Olympic effect may be larger and more widespread than had been previously shown.

The future of international events such as the Olympics depends on their ability to remain profitable, both for the International Olympic Committee and for Olympic hosts. To that end, further research on the Olympic effect is essential. Only when cities fully understand the impacts of the Olympics will they be able to make the best decisions about whether to host them and how to plan them. However, the application of this knowledge will benefit them, the IOC, and the goals of international sporting events around the world.

Data Appendix

Employment and Population Data Information:

Host Nation	Sources and Notes
Australia (Sydney)	**Source:** Australian Bureau of Statistics **Survey:** Labour Force Survey **Frequency:** Monthly **Scope:** Sydney Metropolitan Area **Accession Number:** 6291.0.55.001: Table 02 **URL:** http://www.abs.gov.au/Ausstats/abs@.nsf/mf/6291.0.55.001 **Access Date:** December 5, 2005 **Special Notes:** Civilian population defined as age 16 and over. Download of data files is not free.
Greece (Athens)	**Source:** General Secretariat of National Statistical Service of Greece **Survey:** Labour Force Survey **Frequency:** Quarterly **Scope:** Periphery of Attica **Accession Number:** Labour Force Survey; Table 2b **URL:** http://www.statistics.gr/eng_tables/S301_SJO_1_TS_Q1_98_Q3_05_2B_Y_EN.pdf **Access Date:** December 22, 2005 **Special Notes:** Civilian population defined as age 15 and over.
South Korea (Seoul)	**Source:** Korea National Statistical Office **Survey:** Economically Active Population Survey **Frequency:** Quarterly **Scope:** Seoul Metropolitan Area **Accession Number:** Choose Economically Active Population Survey by City & Province **URL:** http://kosis.nso.go.kr/cgi-bin/SWS_1021.cgi?KorEng=2&A_UNFOLD=1&TableID=MT_ETITLE&TitleID=EC&Fpub=4&UserID= **Access Date:** December 22, 2005 **Special Notes:** Civilian population defined as age 15 and over.
Spain (Barcelona)	**Source:** Instituto Nacional de Estadística (National Statistics Institute) **Survey:** Economically Active Population Survey **Frequency:** Quarterly **Scope:** Province of Barcelona **Accession Number:** Table 4.1 **URL:** http://www.ine.es/inebase/cgi/um?M=%2Ft22%2Fe308_mnu&O=inebase&N=&L=1 **Access Date:** December 10, 2005 **Special Notes:** Civilian population defined as age 16 and over.
United States (Los	***Employment and Civilian Population Data: National*** **Source:** U.S. Bureau of Labor Statistics

Host Nation	Sources and Notes
Angeles, Atlanta)	**Survey:** Current Population Survey **Frequency:** Monthly **Scope:** National **URL:** http://data.bls.gov/PDQ/outside.jsp?survey=ln **Access Date:** December 5, 2005 ***Employment Data: Metropolitan Areas*** **Source:** U.S. Bureau of Labor Statistics **Survey:** Local Area Unemployment Statistics Program **Frequency:** Monthly **Scope:** Atlanta-Sandy Springs-Marietta, GA Metropolitan Statistical Area; Los Angeles-Long Beach-Glendale, CA Metropolitan Division **URL:** http://data.bls.gov/PDQ/outside.jsp?survey=la **Access Date:** December 5, 2005 **Special Notes:** Civilian population defined as age 16 and over. ***Population Data: Metropolitan Area Annual Population Estimates*** **Source:** U.S. Census Bureau, Population Division **Frequency:** Annual (July 1 Base) **Scope:** Atlanta-Sandy Springs-Marietta, GA Metropolitan Statistical Area; Los Angeles-Long Beach-Glendale, CA Metropolitan Division **Accession Number:** CBSA-EST2004-01 (estimates since 2000) MA-99-03a (county estimates 1990-1999) **URL:** http://www.census.gov/popest/estimates.php (general information) http://www.census.gov/population/www/estimates/Estimates%20pages_final.html (estimates since 2000) http://www.census.gov/popest/archives/1990s/MA-99-03a.txt (estimates 1990-1999) http://www.census.gov/popest/archives/1980s/e8089co.txt (estimates 1980-1989) http://www.census.gov/popest/archives/pre-1980/e7079co.txt (estimates pre-1980) **Access Date:** January 23, 2006 **Special Notes:** Metropolitan Division population calculated using Los Angeles County figures before 1990.

Host Nation	Sources and Notes
United States (Los Angeles, Atlanta)	***Population Data: Census Data by Age (for estimating civilian metropolitan population)*** **Source:** U.S. Census **Frequency:** Decennial (1990, 2000; April 1 Base) **Scope:** Atlanta-Sandy Springs-Marietta, GA Metropolitan Statistical Area; Los Angeles-Long Beach-Glendale, CA Metropolitan Division **Access Information:** Data were collected using DataFerrett. Application available: http://dataferrett.census.gov/ **Access Date:** January 24, 2006 **Special Notes:** Percentage of population age 16 and over extrapolated from linear 1990-2000 trend for the period from 2000-2005. ***Population Data: Population Estimate by Age (for estimating civilian population)*** **Source:** U.S. Census Bureau, Population Division **Frequency:** Annual (July 1 Base) **URL:** http://www.census.gov/popest/archives/1980s/PE-02-1980.pdf **Access Date:** January 24, 2006 **Special Notes:** 1980 Population estimates by age used in place of (unavailable online) 1980 Census data by age. Age breakdown in document is 15-19; estimated population at age 15 is subtracted by using average percentage of age 15 out of age group 15-19 in 1990 and 2000 Census data. Percentage of population age 16 and over extrapolated from 1980-1990 trend for the period from 1978-1980.

Population Estimation Procedure for U.S. Data:

1. Percentage of Population age 16 and over in Census data and 1980 estimate interpolated linearly to produce yearly estimated percentage of population age 16 and over (July 1 base).
2. Yearly estimated percentage of population age 16 and over multiplied by annual metropolitan population estimates to compute annual civilian population estimates (July 1 base).
3. Annual civilian population estimates interpolated linearly to produce quarterly civilian population estimates (quarterly midpoint bases).
4. Annual civilian population estimates extrapolated linearly to produce quarterly civilian population estimates from July 1, 2004 to 3rd Quarter 2005 (quarterly midpoint bases).

Employment Data Notes:

- All data are seasonally unadjusted.
- I averaged monthly data to compute quarterly values for Los Angeles, Atlanta, and Sydney.

Other Data Information:

Host Nation	Sources and Notes
All Countries	***GDP and Price Data*** **Source:** World Bank World Development Indicators 2005 **Frequency:** Annual **Scope:** National **Series:** GDP per capita (constant 2000 US$); PPP conversion factor to official exchange rate ratio **URL:** http://devdata.worldbank.org/dataonline/ **Access Date:** January 24, 2006 **Special Notes:** I linearly interpolated annual data to estimate quarterly values. I assumed annual base to be July 1; I transformed to quarterly midpoint bases. I transformed GDP data into a logarithmic form for my model. Download of WDI data files is not free.
All Countries	***Olympic Cost Data*** **Source:** Preuss, Holger. 2004. *The economics of staging the Olympics: A comparison of the games 1972-2008*: Cheltenham, U.K. and Northampton, Mass.: Elgar. **Frequency:** Single-value per Olympics **Units:** million 2000 US$ **Access Information:** Figure 9.1, p. 195: Overall operational expenditures of the OCOGs from Munich 1972 to Beijing 2008 Figure 12.2, p. 277: Surplus and deficit of the Olympic Games according to official reports and own calculations **Special Notes:** I computed overall costs by adding the difference between officially reported and calculated profit margins in figure 12.2 to operational expenditures in Figure 9.1. Profit margin figures for Athens Games are reported as “Estimated December 2003.” I transformed cost data into an indexed scale with the Los Angeles Games equal to zero using the following formula: $C_{Index} = \frac{C_{Games} - C_{L.A.Games}}{C_{L.A.Games}}$
All Countries	***Olympic Games Dates*** **Source:** International Olympic Committee **URL:** http://www.olympic.org/uk/games/index_uk.asp **Access Date:** January 24, 2006 **Special Notes:** I defined the quarter in which the Olympics were held as the quarter in which the Opening Ceremonies were held (the Seoul and Sydney Olympics began in one quarter and ended in the next).

Statistical Methods Information:

Operation	Methods Information
Pooled Regressions	**Method**: Prais-Winsten panel-corrected standard errors **Software:** Stata 8.02 **Command:** xtpcse **Documentation URL:** http://www.stata.com/help.cgi?xtpcse **Selected Options:** correlation(psar1) pairwise
Individual Host City Regressions	**Method:** Newey-West standard errors **Software:** Stata 5 **Command:** newey **Documentation URL:** http://www.stata.com/help.cgi?newey **Selected Options:** lag(1)

Regression Results of Individual Cities' Regressions

- All results report Newey-West heteroscedasticity and autocorrelation-consistent standard errors.
- As with the overall model, different time-based dummy specifications are defined based on findings of significance in preliminary OLS regressions. Not all variables are significant as currently defined. One reason for this may be large standard errors associated with multicollinearity, especially in cities for which less data is available, such as Seoul and Athens. The findings in Los Angeles also suggest the presence of multicollinearity

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
Los Angeles *(R-Squared = .929)*						
Constant	89.12165	1413.418	0.063	0.95	-2718.88	2897.124
D_{Q2}	-51.7875	13.19136	-3.926	0.000	-77.9944	-25.5806
D_{Q3}	-38.1777	14.83482	-2.574	0.012	-67.6497	-8.70573
D_{Q4}	-72.516	11.76087	-6.166	0.000	-95.881	-49.1509
$\log(y)$	-401.308	457.8302	-0.877	0.383	-1310.87	508.252
PPP	706.215	1718.153	0.411	0.682	-2707.2	4119.626
$D_{\mathrm{Pr}e1724}$	195.6157	80.26673	2.437	0.017	36.15185	355.0796
$D_{\mathrm{Pr}e1316}$	212.5734	81.4337	2.61	0.011	50.79118	374.3557
$D_{\mathrm{Pr}e912}$	174.8405	86.43787	2.023	0.046	3.11662	346.5644
$D_{\mathrm{Pr}e58}$	125.48	85.27748	1.471	0.145	-43.9386	294.8986
$D_{\mathrm{Pr}e34}$	111.9408	74.11362	1.51	0.134	-35.2988	259.1804
$D_{\mathrm{Pr}e12}$	89.95205	71.61428	1.256	0.212	-52.3222	232.2263
D_{Post0}	91.36643	70.67977	1.293	0.199	-49.0513	231.7841
D_{Post12}	122.0633	65.79509	1.855	0.067	-8.65008	252.7768
D_{Post34}	87.81095	62.3633	1.408	0.163	-36.0846	211.7065

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
D_{Post58}	118.9187	54.93484	2.165	0.033	9.781091	228.0564
$D_{Post912}$	149.7048	51.14784	2.927	0.004	48.09065	251.3189
$D_{Post1316}$	170.3238	53.46063	3.186	0.002	64.11492	276.5327
$D_{Post1724}$	146.6547	46.92764	3.125	0.002	53.42472	239.8846
$D_{Post2532}$	93.8649	43.51454	2.157	0.034	7.41568	180.3141
D_{Riot}	-220.551	30.02661	-7.345	0.000	-280.204	-160.898
Seoul *(R-Squared = .840)*						
Constant	-552.283	147.6733	-3.74	0.000	-848.35	-256.216
D_{Q2}	-188.239	18.68293	-10.075	0.000	-225.696	-150.782
D_{Q3}	-193.793	15.74847	-12.306	0.000	-225.367	-162.219
D_{Q4}	-126.123	15.17473	-8.311	0.000	-156.546	-95.699
$\log(y)$	438.383	174.6669	2.51	0.015	88.19708	788.569
PPP	310.8137	120.0706	2.589	0.012	70.08671	551.5407
D_{Post34}	29.47811	68.91923	0.428	0.671	-108.697	167.6529
D_{Post58}	-19.4569	57.07407	-0.341	0.734	-133.884	94.96973
$D_{Post912}$	55.88859	58.61432	0.953	0.345	-61.626	173.4032
$D_{Post1316}$	71.14701	54.09978	1.315	0.194	-37.3165	179.6105
$D_{Post1724}$	22.624	42.14432	0.537	0.594	-61.8703	107.1183
$D_{Post2532}$	19.00599	41.84598	0.454	0.652	-64.8901	102.9021
$D_{Post3340}$	33.11567	30.92544	1.071	0.289	-28.8861	95.11745
Barcelona *(R-Squared = .934)*						
Constant	-810.089	64.76296	-12.509	0.000	-938.771	-681.406
D_{Q2}	1.313534	5.746234	0.229	0.820	-10.1041	12.73118
D_{Q3}	-8.92579	7.149077	-1.249	0.215	-23.1309	5.279273
D_{Q4}	-7.12061	6.916999	-1.029	0.306	-20.8645	6.623318
$\log(y)$	772.8441	81.09046	9.531	0.000	611.7191	933.9691
PPP	134.3219	46.07009	2.916	0.004	42.78167	225.8622
$D_{\Pr e912}$	23.46741	8.650986	2.713	0.008	6.278088	40.65674
$D_{\Pr e58}$	24.26969	11.46284	2.117	0.037	1.493269	47.0461
$D_{\Pr e34}$	47.58316	11.98318	3.971	0.000	23.77283	71.39349
$D_{\Pr e12}$	31.19239	12.68153	2.46	0.016	5.994462	56.39032
D_{Post0}	50.53912	12.60701	4.009	0.000	25.48925	75.58899
D_{Post12}	40.63355	11.11668	3.655	0.000	18.54495	62.72215
D_{Post34}	42.46001	7.186146	5.909	0.000	28.18129	56.73873
D_{Post58}	37.67552	6.684135	5.637	0.000	24.39429	50.95676

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
$D_{Post912}$	17.31989	9.263807	1.87	0.065	-1.0871	35.72688
$D_{Post1316}$	32.54236	11.65571	2.792	0.006	9.382709	55.70201
$D_{Post1724}$	63.70327	10.72114	5.942	0.000	42.40059	85.00595
$D_{Post2532}$	88.8257	12.10182	7.34	0.000	64.77964	112.8718
Atlanta *(R-Squared = .813)*						
Constant	-230.209	541.1221	-0.425	0.673	-1320.09	859.6664
D_{Q2}	-28.6753	3.463657	-8.279	0.000	-35.6515	-21.6991
D_{Q3}	-20.4332	3.352516	-6.095	0.000	-27.1855	-13.6809
D_{Q4}	-17.74	3.543391	-5.007	0.000	-24.8768	-10.6033
$\log(y)$	-240.051	56.13475	-4.276	0.000	-353.112	-126.99
PPP	856.7349	602.6324	1.422	0.162	-357.029	2070.499
$D_{\Pr e912}$	21.07674	6.617472	3.185	0.003	7.748472	34.40502
$D_{\Pr e58}$	17.13898	7.010899	2.445	0.018	3.018302	31.25965
$D_{\Pr e34}$	14.66457	3.210729	4.567	0.000	8.197831	21.13131
$D_{\Pr e12}$	19.7735	3.994007	4.951	0.000	11.72916	27.81784
D_{Post0}	42.78088	3.144227	13.606	0.000	36.44808	49.11367
D_{Post12}	22.46226	3.655115	6.145	0.000	15.10048	29.82404
D_{Post34}	26.29618	3.093017	8.502	0.000	20.06652	32.52584
D_{Post58}	26.47464	4.653936	5.689	0.000	17.10113	35.84815
$D_{Post912}$	14.81152	7.732272	1.916	0.062	-0.76207	30.38512
$D_{Post1316}$	20.3657	13.70428	1.486	0.144	-7.23613	47.96752
$D_{Post1724}$	27.50554	9.804308	2.805	0.007	7.758646	47.25243
$D_{Post2532}$	15.06081	7.115372	2.117	0.040	0.729711	29.3919
Sydney *(R-Squared = .753)*						
Constant	-60.1539	39.50618	-1.523	0.131	-138.64	18.33204
D_{Q2}	0.440014	3.086642	0.143	0.887	-5.69214	6.572167
D_{Q3}	-0.41222	3.53207	-0.117	0.907	-7.42929	6.604853
D_{Q4}	-1.09235	3.08641	-0.354	0.724	-7.22404	5.039348
$\log(y)$	69.85611	27.82089	2.511	0.014	14.58506	125.1272
PPP	11.58252	16.31352	0.71	0.480	-20.8271	43.99218
$D_{\Pr e1316}$	23.46012	3.646206	6.434	0.000	16.2163	30.70395
$D_{\Pr e912}$	27.84361	4.642457	5.998	0.000	18.62055	37.06666
$D_{\Pr e58}$	36.95493	6.020394	6.138	0.000	24.99436	48.91549
$D_{\Pr e34}$	38.27426	5.537557	6.912	0.000	27.27294	49.27558
$D_{\Pr e12}$	41.33428	8.122204	5.089	0.000	25.19811	57.47046

Variable	Coefficient	Std. Err.	z	P>|z|	[95% C.I.]	
D_{Post0}	55.97805	6.177708	9.061	0.000	43.70495	68.25114
D_{Post12}	30.05528	6.979173	4.306	0.000	16.18993	43.92062
D_{Post34}	61.63505	9.53648	6.463	0.000	42.68917	80.58093
D_{Post58}	49.39218	7.179972	6.879	0.000	35.12792	63.65645
$D_{Post912}$	28.51992	8.465387	3.369	0.001	11.70195	45.33789
$D_{Post1316}$	21.08802	5.97697	3.528	0.001	9.213722	32.96231
Athens *(R-Squared = .971)*						
Constant	-774.357	697.108	-1.111	0.281	-2238.93	690.2125
D_{Q2}	-27.4234	6.917747	-3.964	0.001	-41.9571	-12.8898
D_{Q3}	-38.5911	8.461227	-4.561	0.000	-56.3675	-20.8147
D_{Q4}	-13.0508	6.982111	-1.869	0.078	-27.7196	1.61812
$\log(y)$	833.5172	612.5579	1.361	0.190	-453.419	2120.454
PPP	-92.7542	148.2897	-0.625	0.539	-404.299	218.7909
$D_{\Pr e912}$	46.26983	19.44972	2.379	0.029	5.407474	87.13218
$D_{\Pr e58}$	46.99474	36.03254	1.304	0.209	-28.7068	122.6963
$D_{\Pr e34}$	31.17047	55.15458	0.565	0.579	-84.705	147.0459
$D_{\Pr e12}$	105.0377	63.08562	1.665	0.113	-27.5003	237.5757
D_{Post0}	100.2544	73.156	1.37	0.187	-53.4407	253.9494
D_{Post12}	77.59826	80.79858	0.96	0.350	-92.1533	247.3498
D_{Post34}	94.26452	90.61328	1.04	0.312	-96.1069	284.636

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