Evaluation examples - Sports and Culture

The following four studies present examples of the evaluations used in the What Works Centre for Local Economic Growth Sports and Culture evidence review. These examples are not intended to be read as best practice case studies for evaluating policies, but are instead intended to demonstrate how our methodology works in practice and where different types of evaluation fit on the Scientific Maryland Scale.

You can find out more about our methodology and the Scientific Maryland Scale on:
www.whatworksgrowth.org/resources/how-to-use-the-evidence-reviews

Statistical approach; SMS level 3

This 2014 study by Gabriel Ahlfeldt and Georgios Kavetsos (study 311) examines the effect on property prices of two new football stadia in London: The New Wembley Stadium and the Emirates Stadium. In both cases the stadia were replacements for older facilities helping to separate effects of ‘form’ from effects of ‘function’. The authors test whether the new and improved architectural quality provides benefits that improve the desirability of the neighbourhood and therefore property prices.

In general, the sites for football stadia are not chosen randomly. The choice of neighbourhood is often a result of history and institutional decision-making. This makes it harder to evaluate their effects, because there are a variety of potential reasons for different property prices closer to a stadium compared with further away. For example, the stadium may be centrally located and prices in that central area may be high for other reasons (such as good rail access, high level of consumption amenities, proximity to workplaces) rather than anything to do with the stadium itself.

In order to deal with this problem the authors use a difference-in-difference approach. Distance rings up to 5km are drawn around the two stadia (e.g. 0-1km, 1-2km, 3-4km and 4-5km) and properties in the outermost ring serve as the control group. Properties in the inner rings are separate treatment groups. The control properties are chosen to be as close as possible to the treated properties – but without being treated themselves – so that they are as similar as possible. The authors then look at the change in property prices inside each inner ring, and compare to the change in the outer ring. Furthermore, they use a large set of control variables to account for differences in property and location characteristics between the treatment and control groups. This method controls well for observable differences between the treatment and control group. However, there likely remain some unobservable differences, despite the close proximity of the two groups. Therefore we score it a ‘3’ on the Maryland Scientific Method Scale.

To implement this approach, the authors used a dataset of property prices from Nationwide Building Society. This dataset contains the address, price and property characteristics for 5,263 properties within 5km of Wembley and a further 9,933 within 5km of Emirates. Importantly, they observe transactions in both the pre- and post-construction periods for both stadia. They were also able to replicate their results using an alternative dataset of Land Registry property transactions.

This evaluation finds significant increases in property prices surrounding both new stadia compared with the control groups. In both cases, this effect decreases with distance to the new stadia but for New Wembley the decline is more gradual. The authors suggest the wider effect for New Wembley is a result of an architectural feature – the arch that stretches about 130m high. They calculate the total increases in property prices (£1.91bn) to be larger than the construction costs (£1.4bn). For Emirates, where the stadium was relocated by around 500m, they found an increase in property prices where distance to the stadium was reduced. Property prices increased around Emirates but decreased around the old Arsenal stadium leading to a net negative effect on the neighbourhood.

What do these results mean for policymakers? These results point to the existence of large positive stadium effects on nearby properties. This suggests that stadia, particularly when of high architectural quality, may be able to contribute to physical neighbourhood regeneration. In this case, it might be socially beneficial to invest public funds to ensure a high quality of stadium design. However, the distributional consequences of such investment would be
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complex. The benefits will typically accrue to homeowners, who experience a gain in property value, rather than renters, who will likely experience higher rents and may be displaced. Further, it may be that the neighbourhoods surrounding the new stadia are made more attractive at the expense of the desirability of other neighbourhoods elsewhere, which then suffer. Finally, whilst the empirical method is fairly robust, there may be unobservable factors driving these results. If so, the true effects could be much smaller.

Statistical Approach; SMS level 3

This 2013 study by Lasse Steiner, Bruno Frey and Simone Hotz (paper 324) looks at the effect of a major cultural event, the European Capital of Culture, on urban & regional GDP and residents’ life satisfaction. Between 1990 and 2009, 29 European cities were chosen to be European Capital of Culture (ECOC) for a given year. The host cities were given a budget for both cultural projects and infrastructure improvements – with investments being mostly (77.5%) generated from the public sector. Projects vary in type and scale but the best represented sectors are theatre, visual arts, music, street parades, open-air events, heritage/history and architecture. There are an average of 500 events in the award year.

The fact that ECOC cities are selected by the EU’s Council of Culture Ministers makes the event harder to evaluate. The selected cities are likely to have different characteristics to non-selected cities. For example, the ministers of culture may choose cities that are struggling or (alternatively) ‘on the up’, and any differences in outcome may be due to these conditions rather than the ECOC status and related investment. Furthermore, the individuals who live in the regions that are selected may have fundamentally different levels of life satisfaction or at least different tendencies in reports of life satisfaction.

In order to deal with this problem the authors use a difference-in-difference approach to estimate the effect on quality of life and GDP. Individuals in regions that have ECOC status in a particular year form the treatment group. The control group is formed of individuals in all other European regions that are not ECOC. The authors look at how much higher quality of life is in ECOC regions in the year that they were ECOC, compared with the control group in the same year. Importantly, a variety of control variables are used to account for regional and individual differences that may affect outcome variables in a particular year. The control variables for satisfaction were personal characteristics such as age, income, and so on, as well as regional economic growth. The controls for regional growth were macroeconomic factors such as population density, sectoral shares and human capital, represented by education. This method does a fairly good job controlling for observable differences between individuals across ECOC and non-ECOC regions/cities but is not able to deal with unobservable differences. Therefore we award it a ‘3’ on the Maryland Scientific Methods Scale.

In order to implement this approach the authors make use of ‘The Mannheim Eurobarometer Trend File 1970-2002’, which is a longitudinal dataset of individuals in 18 Europeans nations (i.e. it follows the same people over time). The dataset includes self-reported life satisfaction, the dependent variable, which is rated on a 4-point scale from ‘not at all satisfied’ to ‘very satisfied’. Individual-specific factors are also available such as employment situation, income, gender, etc. This dataset is combined with regional GDP per capita based on data from BAK Basel.

The results for GDP show no effect of ECOC status on GDP in either the run-up to the event, during the event or after the event. The results for life satisfaction indicate a negative effect for individuals in regions hosting the ECOC during the year of the event. The effect is 0.09 points lower on the 4-point scale, which the authors suggest is considerable. There is no significant effect on life satisfaction in the year prior to the event. After the event is over, life satisfaction returns to pre-ECOC levels. Unemployed people suffer the greatest drops in satisfaction during the event – being unemployed roughly doubles the negative impact on life satisfaction.

What do these results means for policymakers? If taken at face value, there are no GDP effects related to the ECOC and the wellbeing impact is actually negative. Therefore, the number of reasons why a region would want to host such an event is greatly diminished. However, since the control group in this evaluation is made up of all other European regions (rather than only similar regions), it is likely that there remain significant unobserved differences between ECOC and non-ECOC regions that could be responsible for lower quality of life during the event year, despite the large number of control variables. Furthermore, self-reported quality of life is a complex outcome variable to truly understand – hence policy recommendation based evidence of this type should always remain cautious.
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Statistical approach; SMS level 3

In this 2013 study, Arne Feddersen and Wolfgang Maennig evaluate the employment impacts of the 1996 Summer Olympics in Atlanta, Georgia. The event was expected to generate significant ‘legacy’ impacts, particularly employment gains. Such a jobs boost might come directly (via spending on the events and Games infrastructure, or through visitor spending), or indirectly (through job training provided to local workers and unemployed people). Conversely, we might expect Olympic-related expenditure to divert spending away from other job-creating activities; some potential visitors might stay away from the Games, and some locals might leave town.

Identifying the employment effects of a mega-event like this is not easy. First, the winning city is not randomly or transparently selected. Second, the scale and timing of ‘Games effects’ aren’t straightforward to identify. Together, these factors make it hard to model the ‘treatment’, and to identify decent comparison groups. Unobservable individual, neighbourhood or city-level characteristics might also drive employment shifts, rather than any Games effect. And researchers need to control for long term local growth trends.

To deal with these issues, the authors compare changes in employment outcomes in treatment and ‘quasi-control’ areas (a ‘difference in difference’ approach). ‘Treatment’ areas are counties containing Olympic venues, or those immediately around those counties. ‘Control’ areas elsewhere in Georgia and outside the state are identified using matching.

Employment had been rising across Georgia since 1985, over a decade before the Games took place. This underlying trend also varied between treatment and control areas, both pre- and post- 1996. To handle this, Feddersen and Maennig use a ‘trend shift regression’ that captures changes in employment growth in Olympic versus non-Olympic counties, as well as controlling for underlying long run trends. They also extend their model to test different treatment zones (37 counties or 10 key Olympic sites); allow effects to vary across space (over 80% of the Games took place in Atlanta itself, so impacts should be largest there); and allow for employment spillovers across county boundaries.

This methodology handles many of the challenges identified above, but is not perfect. Randomisation is not an option, and the IOC’s selection decision is not observable. Even though area and time fixed effects control for unobservable factors at the aggregate level, individual resident characteristics will vary within areas, and may help explain employment outcomes. We therefore score this study ‘3’ on the Scientific Maryland Scale.

The analysis is run with quarterly employment data from 1985 to 2000. An earlier study by Hotchkiss et al (study 328) found a 17% employment gain in Olympic counties. Using the same data, and controlling for underlying trends, Feddersen and Maennig find no significant effects of the Games on county employment growth. Rather, the Olympic counties were already experiencing strong employment growth, to which the Games added very little. Allowing the Olympic effect to start from 1994, they find a positive weakly significant effect, adding up to a 1% boost to jobs growth in Olympic counties. They also find a weak ‘rebound’ effect in Olympic counties between 1995 and 1996. However, the likely range of these results includes zero. So overall, there is very little evidence for an Olympic jobs boost.

What can we learn from these results? Today’s Olympic Games almost always highlight a ‘legacy’, which typically includes positive employment effects. However this study implies ex-post evaluations of British mega-events like London 2012 and Glasgow 2014 may find similarly small employment impacts. It also suggests that smaller sporting events and festivals – which typically don’t involve new infrastructure – will not create net job gains. That does not mean we shouldn’t run or fund such events, of course – simply that the main benefits are unlikely to be economic.

Statistical approach; SMS level 3

In this 2013 paper (study 327), Douglas Noonan looks at the effect of cultural districts on employment, income and property prices in US neighbourhoods. Cultural districts are formally designated zones within a city: sometimes simply branded as an ‘arts zone’, sometimes with accompanying tax breaks or incentives for artists to move in. Some districts already have a cluster of cultural amenities and institutions (e.g. museums, arts workshops and studios) as well as complementary amenities (e.g. restaurants, cafes). Here, zoning is designed to maintain organic
growth. In a few cases, local policymakers use designation to attract cultural players – such as artists, musicians, galleries or studios – into a neighbourhood with no arts presence. Cities often use cultural districts as part of a place-making strategy, and specifically to ‘revitalise’ the neighbourhood in question (in the US, the number of cultural districts rose from 40 to 127 between 1995 and 2008).

How do we assess the economic effects of cultural districts? Districts are most likely chosen for growth potential (or need for regeneration). So a city might pick a location where employment is already rising, which makes isolating any additional cultural district effect difficult. Some of these growth or decline factors may be unobservable. Alternatively, wider (city or national) conditions may influence outcomes – if the urban economy is growing, District outcomes are likely to improve whether or not zoning is in place. It is also not straightforward to identify the spatial scale of impact, or model its intensity across a treatment area – we can imagine property market effects might be biggest in the streets directly around a new museum or arts space.

Noonan gathers information on 99 cultural districts across the US, and combines this with city and neighbourhood-level data from the 1980, 1990 and 2000 Census. Neighbourhoods are defined by ‘block groups’, small areas with an average population of about 4,000.

Noonan then runs a ‘difference in difference’ analysis on a range of neighbourhood-level economic outcomes. Specifically, he compares changes in outcome between 1990 and 2000 for neighbourhoods with cultural districts, neighbourhoods immediately adjacent and neighbourhoods in the rest of the county (the latter used as a control group). To deal with underlying differences between areas, he also includes a county-level time trend (that captures the fact that counties may exhibit long run differences in growth rates), and a set of 1980 neighbourhood outcomes as controls.

This approach deals with some of the challenges above, but leaves others unsolved. There is no formal testing of whether control areas truly provide a good comparison group, and the evaluations do not control for some neighbourhood-level observable factors (such as supply of workspace) or unobservable factors (such as the quality or energy of local arts scenes). Spillovers from cultural districts might not be limited to adjacent areas – the other treatment group – and there may be some variance in effects within the designated zone. For these reasons we score this study ‘3’ on the Scientific Maryland Scale.

Noonan finds multiple positive effects of cultural districts: growth in property values is 9.3% higher in district neighbourhoods than the rest of the county; income growth is 5.4% higher and employment growth is 4.4% higher. Poverty declines by 2.3% more in zoned neighbourhoods, but commuting times do not change; there also is some evidence of more skilled residents moving in, and increased population turnover. Taken together, this implies that residents are likely working outside the neighbourhood (so additional jobs may not go to locals), and there is some displacement of existing residents (likely connected to rising property prices). There is generally little difference in impacts within cultural districts and in adjacent areas.

This analysis raises important questions about who benefits (economically) from cultural districts and similar initiatives. If part of an economic growth strategy to help residents into work, the wage and employment effects are positive but pretty small (and may not accrue to existing residents). By contrast, property owners (whether residents or businesses) experience much larger gains. Local conditions will vary in UK cities compared to US cities, so some caution is needed in applying these results to Britain. The surest way to test the findings is to replicate the study. Some improvements could be made: for example, running a competition for cultural district designation, then using ‘losers’ as a control group, or comparing outcome shifts in active districts against areas where districts were planned but not enacted.

You can find further evaluation samples, methodological resources and guides on how to evaluate on: www.whatworksgrowth.org/resources

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