



Evidence Review 7 Transport

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what works centre for
local economic growth



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Preface

This report presents findings from a systematic review of evaluations of the local economic impact of transport. It covers evidence on roads, rail (including light rail and subways), trams, buses, cycling and walking – areas of expenditure which account for the majority of transport schemes considered by local decision makers. Evidence on ports and airports will be covered in a further report.

This report is the seventh review produced by the [What Works Centre for Local Economic Growth](#). Our reviews consider a specific type of evidence – **impact evaluation** – that seeks to understand the causal effect of policy interventions and to establish their cost-effectiveness. To put it another way they ask ‘did the policy work’ and ‘did it represent good value for money’? With this review we are particularly interested in demonstrating that the local economic impacts of transport can be rigorously evaluated and in drawing out the wider lessons for policy – including questions of scheme appraisal and prioritisation.

Evidence on impact and effectiveness is a crucial input to good policy making. In the case of transport the main aim is not necessarily to improve the local economy. However, policymakers often claim economic benefits for these interventions, and so it is important to undertake economic impact evaluation to understand if these claims are justified. Other ways of considering the impact of transport (e.g. case studies) provide a valuable complement to impact evaluation, but we do not focus on these in this report.

We see these impact-focused reviews as an essential part of more effective policy making.

We often simply do not know the answers to many of the questions that might reasonably be asked when implementing a new policy – not least, does it work? Figuring out what we do know allows us to make better decisions and to start filling the gaps in our knowledge. **This also helps us to have more informed discussions and to improve policy making.**

These reviews therefore represent a first step in improving our understanding of what works for local economic growth. In the months ahead, we will be working with local decision makers and practitioners, using these findings to help them generate better policy.

Henry Overman;
Director, What Works Centre for Local Economic Growth

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Executive Summary

This report presents findings from a systematic review of evaluations of the local economic impact of transport projects. It covers evidence on roads, rail (including light rail and subways), trams, buses, cycling and walking. Evidence on ports and airports will be considered in a further report. This review is the seventh produced by the What Works Centre for Local Economic Growth.

The review considered more than **2,300** policy evaluations and evidence reviews from the UK and other OECD countries. It found 29 impact evaluations that met the Centre's minimum standards.

Approach

The Centre seeks to establish causal impact – an estimate of the difference that can be expected between the outcome for areas that benefit from transport investment and the average outcome they would have experienced without investment (see Figure 1). Our methodology for producing our reviews is outlined in Figure 2.

Figure 1: Evaluating impact

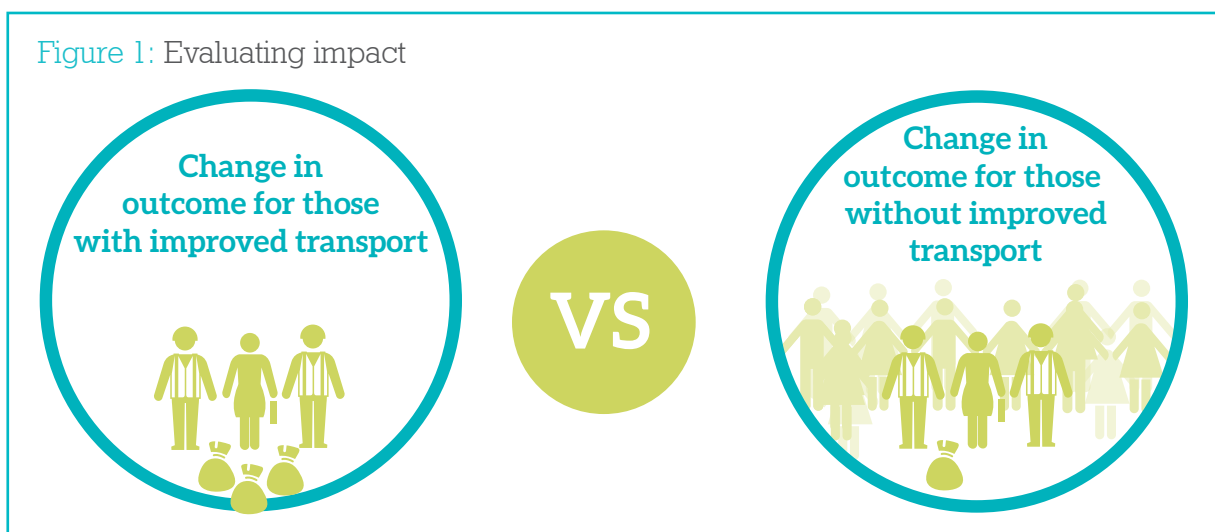
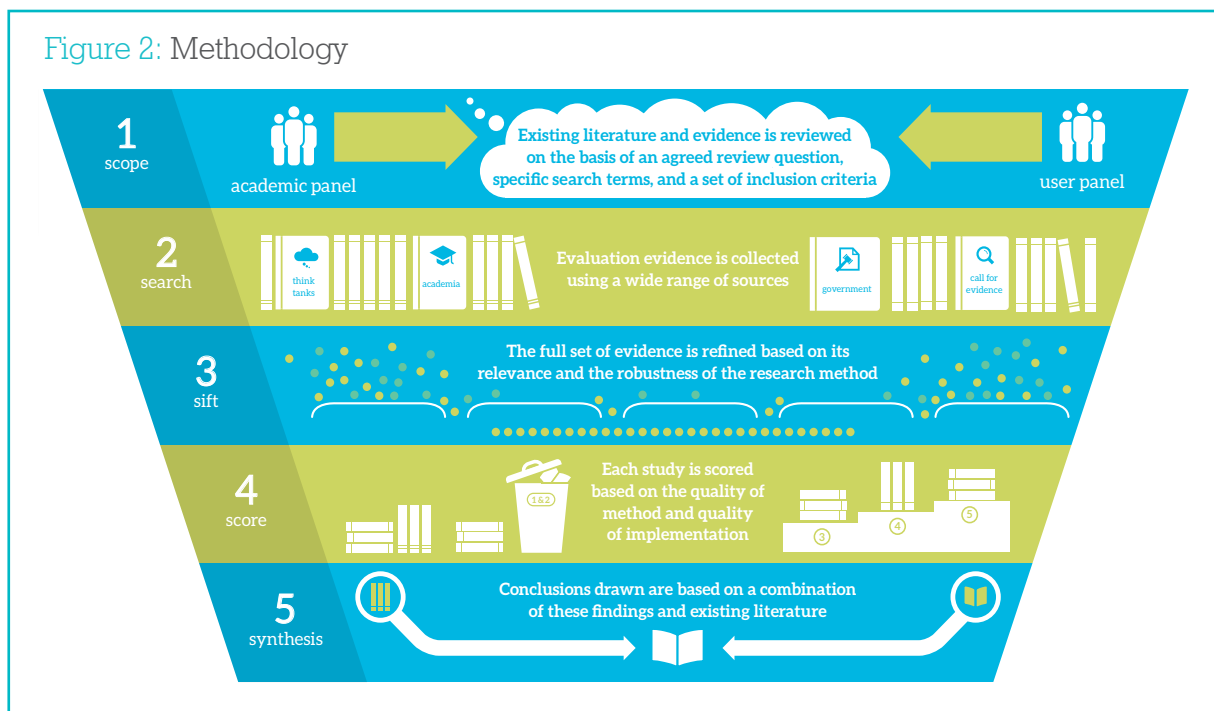


Figure 2: Methodology



Findings

This section summarises the detailed findings. We emphasise that many of these findings depend on a small number of studies. They are, however, consistent with other research on the economic impact of transport improvements.

What the evidence shows

- Road projects can positively impact local employment. But effects are not always positive and a majority of evaluations show no (or mixed) effects on employment.
- Road projects may increase firm entry (either through new firms starting up, or existing firms relocating). However, this does not necessarily increase the overall number of businesses (since new arrivals may displace existing firms).
- Road projects tend to have a positive effect on property prices, although effects depend on distance to the project (and the effects can also vary over time).
- The impact of roads projects on the size of the local population may vary depending on whether the project is urban, suburban or rural.
- There is some evidence that road projects have positive effects on wages or incomes.
- There is some evidence that road projects have a positive effect on productivity.
- Rail projects tend to have a positive effect on property prices, although effects depend on distance to the project (and the effects can also vary over time).

Where there is a lack of evidence

- We found no high quality evaluations that provide evidence on the impact of rail infrastructure on employment, and only a limited number of evaluations showing that road projects have a positive effect.

- We found no high quality evaluations that provide evidence on the impacts of trams, buses, cycling and walking schemes on any economic outcomes.
- Even when studies are able to identify a positive impact on employment, the extent to which this is as a result of displacement from other nearby locations is still unresolved. More generally, the spatial scale of any employment effects varies and we do not have enough evidence to be able to generalise about the spatial distribution of effects if they occur. The same is true for other outcomes. The scale at which the studies evaluate impact varies from adjacent neighbourhoods to much larger US counties.
- Surprisingly, very few evaluations consider the impact of transport investment on productivity (we found just three studies, two for roads and one for rail). Although the use of such productivity effects to calculate 'wider economic benefits' in transport appraisal is underpinned by a larger evidence base, it is still worrying that so few evaluations can demonstrate that these effects occur in practice.
- We have little evidence that would allow us to draw conclusions on whether large-scale projects (e.g. high speed rail or motorway construction) have larger economic growth impacts than spending similar amounts on a collection of small-scale projects (e.g. light rail or junction improvements).
- More generally, we do not know how differences in the nature of improvements (e.g. journey time saved or number of additional journeys) affect local economic outcomes.
- There is some evidence that context matters. For example, property price effects may depend on the type of property, while wage effects may differ between low skilled and high skilled workers. But, once again we do not have enough evidence to be able to generalise.

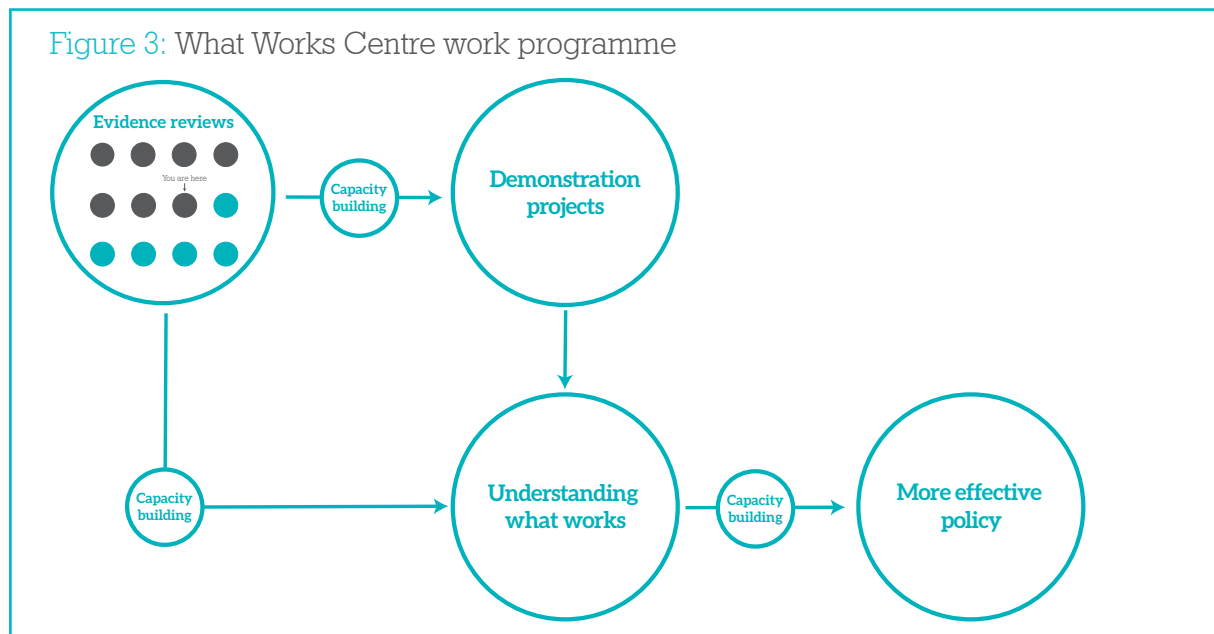
How to use these reviews

The evidence review highlights a number of factors for policy makers to be aware of when considering transport policy:

- Much more empirical work remains to be done on understanding the impact of infrastructure improvements on local economic growth. The economic benefits of transport infrastructure spending – particularly as a mechanism for generating local economic growth – are not as clear-cut as they might seem on face value.
- While it is understandable that political debate focuses on expenditure figures across different parts of the UK, they do not help answer the question of what would happen if expenditure was distributed differently. Arguments for spending more in areas that are less economically successful hinge on the hope that new transport is a cost-effective way to stimulate new economic activity. As this review shows, we do not yet have clear and definitive evidence to support that claim.
- These findings raise fundamental questions about scheme appraisal and prioritisation, and about the role of impact evaluation in improving decision-making around transport investment. Some preliminary recommendations based upon our work with DfT and LEPs are outlined in section 8 of the full report.

To determine policy priorities

The Centre's reviews consider a specific type of evidence – impact evaluation – that seeks to understand the causal effect of policy interventions and to establish their cost-effectiveness. In the longer term, the Centre will produce a range of evidence reviews that will help local decision makers decide the broad policy areas on which to spend limited resources. Figure 3 illustrates how the reviews relate to the other work streams of the Centre.



Helping to fill the evidence gaps

As should be clear from this review, there are many things that we do not know about the local economic impact of infrastructure. To help fill these evidence gaps, the final part of the review provides a number of recommendations aimed at improving the evaluation and appraisal of transport schemes.

The Centre's longer term objectives are to ensure that robust evidence is embedded in the development of policy, that these policies are effectively evaluated and that feedback is used to improve them. To achieve these objectives we want to:

- work with local decision makers to improve evaluation standards so that we can learn more about what policies work, where.
- set up a series of 'demonstration projects' to show how effective evaluation can work in practice.

Interested policymakers please get in touch.



Introduction

This review looks at the local economic impacts of transport investment. It covers evidence on roads, rail (including light rail and subways), trams, buses, cycling and walking – areas of expenditure which account for the majority of transport spending that will be considered by local decision makers.

Transport infrastructure and services are fundamental public goods that affect the way societies and economies function. Local decision makers will want to take many factors into account when deciding local transport policy, but our focus is on the narrower issue of understanding the economic impact.

There are two main economic aims of transport spending. First, to reduce transport costs to businesses and commuters (for example by reducing congestion – and thus saving time - or by reducing fares). Second, and related, to stimulate the UK and local economies, for example, by raising the productivity of existing firms and workers or by attracting new firms and private sector investment. To meet these policy aims requires an understanding of whether we are spending enough and on the right things.

To help answer this question, this review summarises some of the key theories and evidence regarding the impact of transport on the economy – with a particular focus on the lessons that we can draw from the limited number of available impact evaluations.

The basic message that emerges from this review is that the economic benefits of transport infrastructure spending – particularly as a mechanism for generating local economic growth - are not as clear-cut as they might seem on face value. In turn, this raises fundamental questions about scheme appraisal and prioritisation and about the role of impact evaluation in improving decision making around transport investment. The latter part of this review addresses some of these related questions.

The economic aims of transport spending

For a country like the UK with a well-developed transport network, we can identify two key policy aims (Gibbons, 2015). The first is to respond to growing demand so that increased congestion, longer travel times and higher costs to producers and consumers, do not constrain growth. On the basis of this kind of “ameliorative” argument, we should invest more in places where the economy and transport demand is growing.

One concern with this approach is that making travel easier in this way simply encourages more travel. If this happens, it may divert resources from other places and sectors, with little economic gain and big environmental costs. Another concern is that this kind of policy may exacerbate spatial inequalities by targeting resources at places which are already prosperous and growing.

A second aim of transport spending is to stimulate local economies. That is, to drive growth in the local economy, rather than just respond to it. Arguments for greater investment to meet this objective are based on the idea that lower transport costs allow for the more efficient allocation of existing resources. For example, a considerable body of evidence suggests that connecting people, firms and places more closely generates “agglomeration economies”, which increase productivity. Lowering transport costs also increases private sector returns and this may stimulate investment.

Building on these ideas, a number of recent reports have argued for greater investment to stimulate national growth, and also to tackle spatial disparities within the UK (e.g. City Growth Commission 2014). To meet the latter objective, such reports argue that we should target more resources to places where economic performance is lagging, in order to stimulate growth.

The high profile Eddington Review of the UK’s transport network focused more on the first of these issues. It highlighted the problems of congestion and the potential economic benefits of an improved system estimating that a 5% reduction in travel times nationally would be worth around 0.2% of GDP annually (Eddington 2006). The report argued that the UK was already well interconnected, and recommended that improvements should focus on increasing the performance of the existing network through management and pricing.

The key policy priorities the Eddington Review identified were growing and congested areas, urban areas, and major congested inter-city links. According to this analysis, transport infrastructure investment should aim to relax the constraints that a congested system imposes on travel and business costs. Investment should be targeted to places where there is growing demand for transport, implying that investment should flow to the fastest growing cities and regions.

The LSE Growth Commission (Aghion et al 2013) echoed many of these conclusions, and proposed a set of new independent institutions to unblock major transport infrastructure planning decisions – including a Strategy Board to determine long-term infrastructure plans (then ratified by Parliament), a Commission to deliver this plan (including generous compensation for losers to deflect Nimbyism) and an Infrastructure Bank to help with both finance and private expertise.

As the LSE Growth Commission report demonstrates, little has changed about our understanding of the interactions between transport and the economy since the Eddington report was written. However, since 2007 the Great Recession has led to a renewed focus on disparities between major cities (London in particular) and the rest of the country. In turn, this has raised questions about the extent to which transport investment could help narrow these disparities. For example, a recent report by IPPR (Cox and Davies 2013) on regional infrastructure issues highlighted stark differences in planned spending per person in different regions, and argued for greater spending in lagging areas in the North of England.¹ Recent reports such as these have once again raised the question of whether we can stimulate economic activity – locally, regionally or nationally – through infrastructure

¹ The extent of disparities depends crucially on whether the expenditure figures used for comparison include only public investment or are based on total investment where there is some element of public support. Disparities in the latter look much larger than disparities in public investment alone. It is also important to note that historical disparities – which underpin today’s differences in economic performance – are much smaller than disparities in planned spending. Finally, different ways of presenting these figures eliminate or even reverse these disparities. See Gibbons (2015) for further discussion.

investment, rather than simply targeting it to meet underlying demand. This question is central to our understanding of the role of transport investment in improving local economic growth and is the main focus of the remainder of this review.

The effect of transport investment on local economic growth

There are two ways of structuring our thinking about the likely economic impact of infrastructure investments. The first views public sector infrastructure investment as providing a capital stock that is complementary to private sector physical capital (i.e. machines and buildings) and to human capital (i.e. skills). The second thinks of infrastructure as providing a network that connects different places so that public sector investment reduces the transport costs between places.

The first way of thinking suggests that providing more infrastructure will always improve area level productivity (Jones, 2013). Of course, infrastructure can be very expensive so these productivity benefits might be outweighed by the costs of provision. This disparity between productivity benefits and costs may be particularly acute when infrastructure is used to try to turn around struggling local economies. Because infrastructure is durable, places that have seen slow growth will tend to have relatively large amounts of infrastructure per person. The concrete manifestation of this are relatively low congestion levels in poorly performing cities. Economic theory – supported by empirical evidence – suggests that adding further transport investment in those places may not do much to improve productivity.² In contrast, investing in congested places will tend to deliver higher returns because the congestion reflects the fact that these places have low infrastructure per person. Of course, these are general tendencies which don't rule out the possibility that specific projects may have larger impacts in poorly performing cities (and vice-versa).

The second way of thinking about infrastructure – as a network that connects different places – provides more mixed messages; particularly when it comes to better connecting rich and poor regions (Baldwin, et al; 2005). One way to think about these types of transport investment is to view enhanced integration as a way of increasing the effective size of the local economies. As a larger local economy means higher agglomeration economies this should help firms be more productive.

There are two important caveats concerning this line of reasoning. First, the available empirical evidence suggests that agglomeration economies may attenuate quite quickly with distance. It is not clear, therefore, whether connecting different cities will always generate significant agglomeration benefits.

Second, lowering transport costs may encourage firms to move into the richer market and serve their customers from there. This 'two way roads problem' is poorly understood, leading some policymakers to focus solely on the benefits to the poorer market – rather than thinking through the 'threats' from greater competition.

As will become clear from the evidence reviewed below, much more empirical work remains to be done on understanding the impact of infrastructure improvements on local economic growth. Theoretical analysis certainly urges caution in assuming that infrastructure investment can stimulate growth in poorly performing areas. In short, while infrastructure investment may be vitally important for growing cities, its role in stimulating growth is not as clear-cut as assumed by many decision makers.

² This is because investments in physical capital are likely to be subject to 'diminishing marginal returns'. This means that, when a place has lots of capital per person adding extra capital will not do much to increase productivity. See, for example, Solow (1956) and the large economic growth literature that builds on this work.

04

Impact evaluation

Governments around the world increasingly have strong systems to monitor policy inputs (such as spending on infrastructure provision) and outputs (such as the number and speed of journeys made on a new road). However, they are less good at identifying policy outcomes (such as the wider effect of transport on local employment). In particular, many government-sponsored evaluations that look at outcomes do not use credible strategies to assess the causal impact of infrastructure investment (henceforth, we refer to these as ‘projects’).

By causal impact, the evaluation literature means an estimate of the difference that can be expected between the outcome for areas undertaking a project (in this case, improving transport provision) and the average outcome they would have experienced without the project. Pinning down causality is a crucially important part of impact evaluation. **Estimates of the benefits of a project are of limited use to policy makers unless those benefits can be attributed, with a reasonable degree of certainty, to that project.**

The credibility with which evaluations establish causality is the criterion on which this review assesses the literature.

Using Counterfactuals

Establishing causality requires the construction of a valid counterfactual – i.e. what would have happened to an area (or part of an area) if the project hadn’t happened. That outcome is fundamentally unobservable, so researchers spend a great deal of time trying to rebuild it. The way in which this counterfactual is (re)constructed is the key element of impact evaluation design.

A standard approach is to create a counterfactual group of similar places not undertaking the kind of project being evaluated. Changes in outcomes can then be compared between the ‘treatment group’ (locations affected by improved transportation) and the ‘control group’ (locations not affected). As we discuss below, in the case of transport provision, such treatment and control groups are not always easy to identify.

A key issue in creating the counterfactual group is dealing with the ‘selection into treatment’ problem. Selection into treatment occurs when locations that undergo transport improvements differ from those who do not do so.

An example of this problem for transport projects would be when a government focuses transport investment on its best performing cities. If this happens, estimates of policy impact may be biased upwards because we incorrectly attribute better economic outcomes to the project, rather than to the fact that the city is already performing better than average.

Selection problems may also lead to downward bias. For example, if a local authority project explicitly targets slow growing areas for transport improvements then we may mistakenly attribute poor economic performance to the project rather than to underlying conditions in the area.

These factors are often unobservable to researchers. **So the challenge for good programme evaluation is to deal with these issues, and to demonstrate that the control group is plausible.** If the construction of plausible counterfactuals is central to good policy evaluation, then the crucial question becomes: **how do we design counterfactuals?** Box 1 provides some examples.

Box 1: Impact evaluation techniques

One way to identify causal impacts of a project is to randomly assign participants to treatment and control groups. For researchers, such **Randomised Control Trials** (RCTs) are often considered the ‘gold standard’ of evaluation. Properly implemented, randomisation ensures that treatment and control groups are comparable both in terms of observed and unobserved attributes, thus identifying the causal impact of the project. However, **implementation of these ‘real world’ experiments is challenging and can be problematic.** RCTs may not always be feasible for local economic growth policies – for example, policy makers may understandably be unwilling to randomise the location of projects.³

Where randomised control trials are not an option, **‘quasi-experimental’** approaches of randomisation can help. These strategies can deal with selection on unobservables, by (say) exploiting institutional rules and processes that result in some locations quasi-randomly undertaking projects.

Even using these strategies, though, the treatment and control groups may not be fully comparable in terms of observables. Statistical techniques such as **Ordinary Least Squares** (OLS) and **matching** can be used to address this problem.

Note that higher quality impact evaluation first uses identification strategies to construct a control group and deal with selection on unobservables. Then it tries to control for remaining differences in observable characteristics. It is the combination that is particularly powerful: OLS or matching alone raise concerns about the extent to which unobservable characteristics determine both treatment and outcomes and thus bias the evaluation.

3 Gibbons, Nathan and Overman (2014).

Evidence included in the review

We include any evaluation that compares outcomes for areas improving transport provision (the treated group) after the project with outcomes in the treated group before the project; relative to a comparison group used to provide a counterfactual of what would have happened to these outcomes in the absence of the project.

This means we look at evaluations that do a reasonable job of estimating the impact of the project using either randomised control trials, quasi-random variation or statistical techniques (such as OLS and matching) that help make treatment and control groups comparable. We view these evaluations as providing credible impact evaluation in the sense that they identify effects that can be attributed, with a reasonable degree of certainty, to the project in question. A full list of shortlisted studies is given in Appendix B.

Evidence excluded from the review

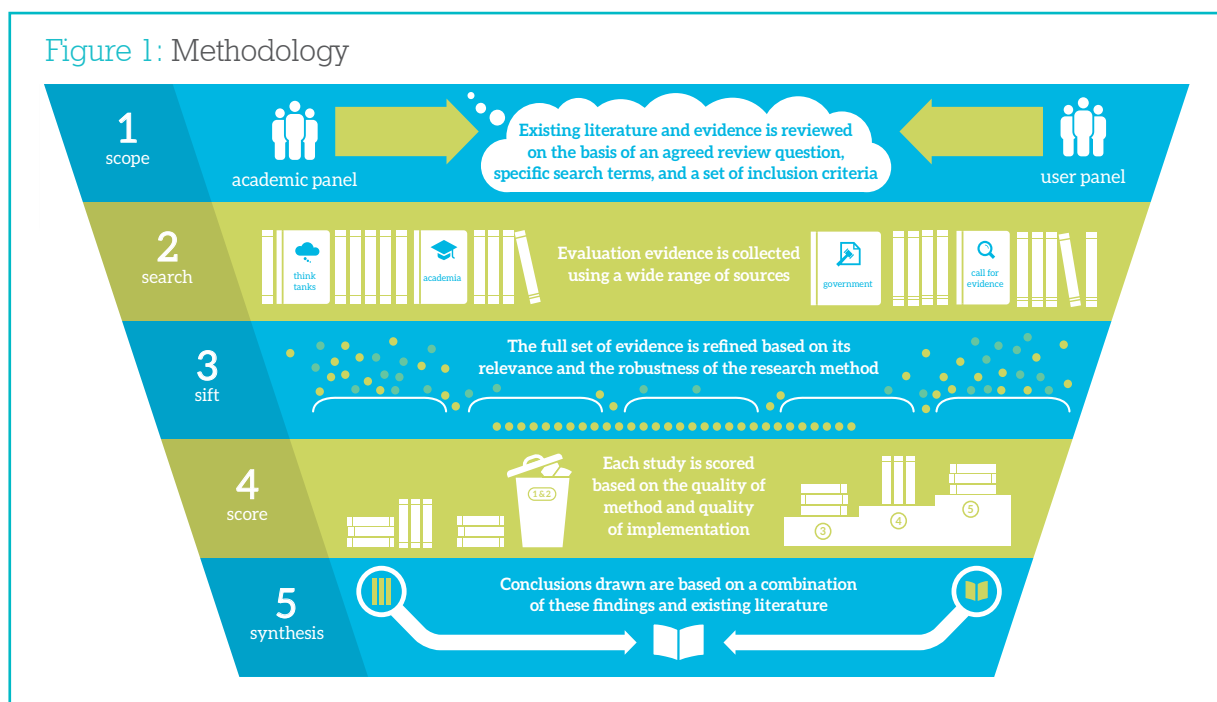
We exclude evaluations that provide a simple before and after comparison only for those places undertaking transport projects because we cannot be reasonably sure that changes for the treated group can be attributed to the effect of the project.

We also exclude case studies or evaluations that focus on process (how the project is implemented) rather than impact (what was the effect of the project). Such studies have a role to play in helping formulate better policy but they are not the focus of our evidence reviews.

05

Methodology

To identify robust evaluation evidence on the causal impact of transport we conducted a systematic review of the evidence from the UK and across the world. Our review followed a five-stage process: scope, search, sift, score and synthesise.



Stage 1: Scope of Review

Working with our User Panel and a member of our Academic Panel, we agreed the review question, key terms and inclusion criteria. We also used existing literature reviews and meta-analyses to inform our thinking.

Stage 2: Searching for Evaluations

We searched for evaluation evidence across a wide range of sources, from peer-reviewed academic research to government evaluations and think tank reports. Specifically, we looked at academic databases (such as EconLit, Web of Science and Google Scholar), specialist research institutes (such as CEPR and IZA), UK central and local government departments, and work done by think tanks (such as the OECD, ILO, IPPR and Policy Exchange.) We also issued a call for evidence via our mailing list and social media. This search found just over 2,300 books, articles and reports (the full list of search terms can be found online here: whatworksgrowth.org/policies/transport/search-terms).

Stage 3: Sifting Evaluations

We screened our long-list on relevance, geography, language and methods, keeping impact evaluations from the UK and other OECD countries, with no time restrictions on when the evaluation was done. We focused on English-language studies, but would consider key evidence if it was in other languages. We then screened the remaining evaluations on the robustness of their research methods, keeping only the more robust impact evaluations. We used an adjusted version of the Maryland Scientific Methods Scale (SMS) to do this.⁴ The SMS is a five-point scale ranging from 1, for evaluations based on simple cross sectional correlations, to 5 for randomised control trials (see Box 2). We shortlisted all those impact evaluations that could potentially score three or above on the SMS⁵. In this case we found no evaluations scoring five: for examples of impact evaluations that score three or four on the SMS scale see the case studies and our scoring guide available at: www.whatworksgrowth.org/resources/scoring-guide.

Stage 4: Scoring Evaluations

We conducted a full appraisal of each evaluation on the shortlist, collecting key results and using the SMS to give a final score for evaluations that reflected both the quality of methods chosen and quality of implementation (which can be lower than claimed by some authors). Scoring and shortlisting decisions were cross-checked with the academic panel members and the core team at LSE. The final list of included studies and their reference numbers (used in the rest of this report) can be found in Appendix B.

Stage 5: Synthesising Evaluations

We drew together our findings, combining material from our evaluations and the existing literature.

⁴ Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway (1998).

⁵ Sherman et al. (1998) also suggest that level 3 is the minimum level required for a reasonable accuracy of results.

Box 2: Our robustness scores (based on adjusted Maryland Scientific Methods Scale)

Level 1: Either (a) a cross-sectional comparison of treated groups with untreated groups, or (b) a before-and-after comparison of treated group, without an untreated comparison group. No use of control variables in statistical analysis to adjust for differences between treated and untreated groups or periods.

Level 2: Use of adequate control variables and either (a) a cross-sectional comparison of treated groups with untreated groups, or (b) a before-and-after comparison of treated group, without an untreated comparison group. In (a), control variables or matching techniques used to account for cross-sectional differences between treated and controls groups. In (b), control variables are used to account for before-and-after changes in macro level factors.

Level 3: Comparison of outcomes in treated group after an intervention, with outcomes in the treated group before the intervention, and a comparison group used to provide a counterfactual (e.g. difference in difference). Justification given to choice of comparator group that is argued to be similar to the treatment group. Evidence presented on comparability of treatment and control groups. Techniques such as regression and (propensity score) matching may be used to adjust for difference between treated and untreated groups, but there are likely to be important unobserved differences remaining.

Level 4: Quasi-randomness in treatment is exploited, so that it can be credibly held that treatment and control groups differ only in their exposure to the random allocation of treatment. This often entails the use of an instrument or discontinuity in treatment, the suitability of which should be adequately demonstrated and defended.

Level 5: Reserved for research designs that involve explicit randomisation into treatment and control groups, with Randomised Control Trials (RCTs) providing the definitive example. Extensive evidence provided on comparability of treatment and control groups, showing no significant differences in terms of levels or trends. Control variables may be used to adjust for treatment and control group differences, but this adjustment should not have a large impact on the main results. Attention paid to problems of selective attrition from randomly assigned groups, which is shown to be of negligible importance. There should be limited or, ideally, no occurrence of 'contamination' of the control group with the treatment.

Note: These levels are based on but not identical to the original Maryland SMS. The levels here are generally a little stricter than the original scale to help to clearly separate levels 3, 4 and 5 which form the basis for our evidence reviews.

06

Definition

Transport improvement projects are broad in scope, not only in terms of the transport mode (see below), but also in terms of the type of interventions. Three broad types of interventions were considered as part of this review:

- Physical intervention – i.e. the expansion and improvement of transport infrastructure. This could either include the building of new routes and facilities, or through making capital improvements to existing ones (e.g. increasing highway capacity through junction upgrades or extra lanes).
- Service enhancement – i.e. where the physical layout of the transport infrastructure remains unchanged but where its quality is increased (e.g. improvements to reliability, increasing service frequency).
- Revenue projects – i.e. changes to the way existing transport infrastructure is supplied and consumed. This can be split into two further groups:
 - Pricing interventions / subsidies – e.g. fare subsidies, car-pool lanes, congestion charges etc.
 - Sectoral service change – changing the ownership or operation of transport services, e.g. privatisation or nationalisation.

Whilst evaluations from all three groups were included during the search phase of the review, ultimately the majority of the articles meeting the Centre's standards focus on physical interventions to expand / improve infrastructure.

To help order the large amount of literature (around 2,300 policy evaluations and evidence reviews), studies were split by mode as follows:

- Road.
- Rail – covering a range of types, including high speed, regional, urban, and light (e.g. subway) rail infrastructure.
- Non-rail public transport – e.g. trams and buses.
- Walking and cycling.

- Ports.
- Airports.
- Multi-modal.

This report covers evidence on roads, rail (including light rail and subways), trams, buses, cycling and walking – areas of expenditure which account for the majority of transport spending that will be considered by local decision makers. Evidence on ports and airports will be considered in a further review.

Impact evaluation for infrastructure projects

As discussed above, evaluating the economic effects of transport projects is challenging: transport will affect multiple economic outcomes in ways that are hard for researchers to disentangle.

There are also specific challenges in undertaking high quality impact evaluation. It is fairly easy to understand how we might construct control groups and undertake evaluation for policies targeted at individuals, households or firms. It is harder to think about how we might do this for policies – such as rail and road – that target areas. In addition to our substantive interest in the impacts of policy, one of our motivations in considering transport is to help convince decision makers that better evaluation is possible. This section provides a brief explanation of how the reports we considered have tried to do this. Further details on specific examples can be found in our scoring guide available from www.whatworksgrowth.org/resources/scoring-guide.

Evaluation of the local economic growth effects of transport is particularly challenging. The use of cost-benefit analysis means that much infrastructure spending occurs in areas where there is expected to be strong and growing demand. Often these locations will already be experiencing economic growth and increases in jobs and wages – underlying factors that are driving the growth in demand. The effects of these underlying factors ('selection effects') must be accounted for if we want to understand the extent to which transport spending actually increases growth.

Selection is likely to be a much bigger problem for transport projects than for some of our previous reviews that considered similar area based policies. For example, when reviewing the effects of sports and cultural facilities or of estate renewal, economic factors may often be one consideration among many when making decisions on projects. However, for transport projects, economic factors are likely to be a core consideration. For this reason, treated areas are almost always likely to be different to untreated areas. Some of these differences will be hard to observe in available data, making it very difficult to construct an appropriate control group. Furthermore, it is unlikely that these underlying differences will be constant over time.

In many circumstances evaluations could, in principle, use randomised control trials to address these concerns over selection. For capital expenditure, where investments are durable, it is hard to imagine situations in which true randomisation of project placement would be either feasible or desirable. This means that we need to rely on alternative evaluation approaches to try to address the problem of selection and thus identify the causal impact of transport investment.

Many studies in this review attempt to address these 'selection problems' using variations on difference-in-difference or panel fixed effects methods. In these methods, the change in outcome in the 'treatment' areas (those that undertake projects) is compared with the change in outcome in a group of similar control areas (which do not). The control group is constructed to be similar to the treatment group either by matching on observed characteristics or by using control variables. By

taking a before-and-after difference, this method eliminates all fixed unobservable differences between the treatment and control groups. However, as already discussed, there are also likely to be time-varying unobservable differences that lead to investment in transport infrastructure. These methods cannot account for these underlying factors.

In order to allow for these unobservable factors, and thus more reliably assess the impact of transport projects it is important to exploit some source of randomness in the way transport infrastructure is delivered. Although the overall number of evaluations we have available is small, around one third of them have attempted to use methods that exploit some source of randomness. This is a larger share than for many of our other reviews (it is about the same as for broadband), reflecting the importance of such methods for evaluation in this policy area.

For example, study 1067 looks at the effect of highways on employment and wages of skilled labour using an instrument based on the fact that the US highway system was planned along a grid pattern.⁶ This means that highways are more likely to run through rural counties that are directly north, south, east or west of the nearest major city. This arbitrary feature of the system provides quasi-random variation in the delivery of roads to the rural counties that can be exploited to estimate a causal effect. On average, counties lying directly north of a major city are not expected to be different from counties lying say north-east of a major city apart from the fact they are more likely to receive a highway. Therefore, any difference in the employment and wages of skilled labour may more confidently be attributed to the effects of the highway infrastructure.

In a second example, paper 1017 examines the effect of roads on firm performance, exploiting the fact that the first few sections of an inter-city motorway provide improved access between locations within a local area. Since the intercity connection is provided to increase access between, rather than within local areas, this local improvement is considered quasi-random. Therefore any improvements in firm performance for the improved areas compared with similar unimproved areas in the same local area can be attributed to the effect of the road.

These methods are potentially the only way to achieve reliable estimates of the impact of transport investment on local economic growth outcomes. Future transport evaluations should pay close attention to techniques used in studies such as these, an issue to which we return below.

6 All study numbers refer to specific evaluations as listed in Appendix B: Evidence Reviewed.



Findings

This section sets out the review's findings. We begin with a discussion of the evidence base, and then explore the overall pattern of results. After this we consider specific outcomes in more detail.

The review initially considered 2,300 policy evaluations and evidence reviews from the UK and other OECD countries, identified during the initial keyword search. This is a significantly larger starting evidence base than our earlier reviews.

Following a further high level review, over 1,800 were sifted out as not relevant (e.g. because they were theoretical rather than data-based; reviewed non-OECD countries; or because of subject relevance). From the remaining evaluations, we discarded over 250 further evaluations as they were found not to be econometrically robust. Finally, 232 studies were shortlisted for detailed review. The results of that detailed review are outlined in the following sections, which split the evaluations by mode.

The scale at which the studies evaluate impact varies from adjacent neighbourhoods to much larger US counties.

Roads

Quantity and quality of the evidence base

Of the 232 shortlisted studies reviewed in detail, 80 considered the impact of roads projects.

Of these 80 studies, an additional 62 studies were discounted: Eight on grounds of relevance, and 54 on grounds of not meeting the Centre's minimum standard of evidence (i.e. scored 2 or below on the SMS scale). The remaining 17 studies have been included in this review.

This is a smaller evidence base than most of our reviews to date (on employment training, business advice, sports and culture projects, access to finance and estate renewal) but roughly on par with our review of broadband. As discussed above, this partly reflects the difficulties in evaluating transport projects but is also indicative of a failure to carefully evaluate existing policy interventions. Table 1 shows the distribution of the studies ranked by SMS score.

Table 1: Implementation Quality Scores

SMS Score	No. of studies	Evaluation reference numbers
3	12	1009, 1011, 1015, 1016, 1027, 1031, 1035, 1050, 1052, 1055, 1061, 1062
4	5	1005, 1017, 1037, 1063, 1067
Total	17	

We found no studies that used randomised control trials, but five studies that used credible random sources of variation. As discussed in the previous section, this is not that surprising given the nature of these projects. The remaining 12 studies used variations of difference-in-difference and panel methods (scoring 3 on the SMS). The techniques applied in these studies mean that we can be reasonably confident that they have done a good job of controlling for observable characteristics of areas, individual households and firms affected by the projects. However, it is likely that unobservable characteristics may still be affecting the results.

Type and Focus of Support

In most of our previous evidence reviews we have focused on specific policy interventions aimed at delivering particular objectives (e.g. government funded employment training in our first review). In contrast, the vast majority of studies in this review focus on evaluating the impact of specific investment projects or overall spending rather than evaluating a specific policy with explicit objectives and rationales. This is unsurprising given the nature of most transport investment.

The majority of programmes were publicly funded (either at a national, local or EU level). Of the evaluations:

- Two studies evaluate named policies which provided funds for road building and improvement projects:
 - European Regional Development Fund – Trans-European Networks (TENs), EU.⁷
 - The use of European Structural Funds to upgrade roads in Spain.⁸
- Ten evaluations examined construction and improvement works related to road networks generally:
 - The expansion of Portugal's motorway network into economically lagging regions.⁹
 - The expansion of the United States Interstate Highway System.¹⁰
 - Investment in the Spanish roads network generally.¹¹
 - Road construction and improvement projects in the United Kingdom generally.¹²
 - Development of roads in the United States generally.¹³

7 Study 1055.

8 Study 1016.

9 Study 1061.

10 Study 1067.

11 Study 1062.

12 Study 1017.

13 Study 1037.

- The growth of road networks in the state of Minnesota, USA.¹⁴
- Road widening projects in the state of North Carolina, USA.¹⁵
- One study refers to the United States 1947 highway plan as a proxy for dataset construction.¹⁶
- Another study focusses on federal road-building in the United States following the Highway Act 1944 and the Interstate Highway Act 1956.¹⁷
- One study focusses on toll roads, constructed in Orange County in California, USA and operated by Transportation Corridor Agencies.¹⁸
- Five studies focus on the construction of specific roads or road networks:
 - The opening of Interstate 105 in California, USA.¹⁹
 - The construction and opening of the Interstate 210 extension in California, USA.²⁰
 - The construction and opening of State Routes 87, 85 and 237 in Los Angeles, USA.²¹
 - The extension of President George Bush Turnpike and Dallas North Tollway toll roads in Dallas, USA.²²
 - The construction of the M6 and M60 highways in Hungary.²³

Of the seventeen studies on the roads final shortlist, only one focusses on the effects of road construction and improvements in the UK. The majority of studies (eleven), examine programmes in the USA. The remaining studies evaluate programmes in Spain (two), Portugal and Hungary with one study examining programmes throughout the EU.

Findings by outcome

A breakdown of the studies by outcome and overall finding is provided in the tables in Appendix A.

Employment

Road projects can positively impact local employment. But effects are not always positive and a majority of evaluations show no (or mixed) effects on employment.

Table 2: Road investment evaluations by outcome on employment

Outcome	No. of studies	Evaluation reference numbers
Positive	2	1011, 1017
Zero	3	1027, 1031, 1067
Mixed	1	1015

14 Study 1027.

15 Study 1031.

16 Study 1063.

17 Study 1005.

18 Study 1009.

19 Study 1011.

20 Study 1035.

21 Study 1015.

22 Study 1050.

23 Study 1052.

If we are interested in the role that transport may play in driving economic growth, then a central question is the extent to which projects cause changes in employment. Six evaluations consider the impact of road projects on local employment. Of these, two evaluations find positive impacts, three find no impact and one evaluation shows mixed results.

Of the two evaluations reporting positive effects, one finds impacts that are relatively large: employment in the treatment group increased by 200% against 10% in the control group over the study period, between 1980 and 1997.²⁴ The authors suggest that the strength of these effects may reflect negative spillovers – i.e. positive effects on areas along the highway corridor (the ‘treatment’ group), accompanied by losses for areas at a greater distance (the ‘control’ group). Moving jobs around is not the same as creating jobs, therefore, this issue of displacement should be a major concern for local decision makers interested in distinguishing between total and additional economic growth.

For the ‘no impact’ evaluations, one study found the relationship between road network expansion and employment growth disappeared after controlling for locational factors such as human capital levels and tax rates.²⁵ Similarly one study found that even after capacity enhancements, the highway network quickly became saturated with traffic.²⁶ Employment between 1985 and 1997 remained unchanged in ‘treated’ counties with an increase in density of highway lane-miles during that period.

The mixed result looked at several case studies which showed increasing, static and decreasing total employment in the areas around highway expansion or improvement programmes in three neighbouring California counties.²⁷

Firm Entry and Number of Businesses

Road projects may increase firm entry, although not necessarily the overall number of businesses (as new entrants may displace existing firms).

Table 3: Road investment evaluations by outcome on firm entry

Outcome	No. of studies	Evaluation reference numbers
Positive	2	1017, 1061
Zero	1	1016

When employment effects are positive, this may be driven by both expansion of existing firms and entry of new firms. Even in the absence of employment growth, effects on firm entry, exit and the overall number of business may be of interest to local decision makers.

Three evaluations consider these effects, with two finding positive effects and one finding zero effect. The two studies reporting positive effects both look at firm entry.²⁸ In one of these studies, plant birth was affected positively across most sectors within 10km of new motorways, although the effect was largest for sectors requiring proximity to markets and clients such as primary industries.²⁹ The share of industrial sector plant births in municipalities within 10km of motorways increased at a much higher rate than the share of service sector firm births in the same municipalities between 1986 and 1997. The

²⁴ Study 1011.

²⁵ Study 1027.

²⁶ Study 1031.

²⁷ Study 1015.

²⁸ Studies 1017 and 1061.

²⁹ Study 1061.

authors attribute this difference in effect across sectors to a possible greater reliance on transport by manufacturing firms. The positive effects tended to decrease beyond 10km, with zero effects beyond 50km. Again, this raises the possibility that there is some displacement to areas next to the project, from areas close to but not next to the project. The second positive study³⁰ considered business accessibility to road improvements by UK electoral wards and found that wards in close proximity to road improvements recorded an increase in new plants over the study period. In this evaluation it was found that a 10% improvement in accessibility leads to a 3% increase in the number of businesses and employment up to 30km from the site of the improvement. In contrast to these two studies, the third study, found that national road capacity in Spain had no effect on the total number of firms.³¹

Property Prices

Road projects tend to have a positive effect on property prices, although the effect in prices may depend on distance to the project (and the effects can vary over time).

Table 4: Road investment evaluations by outcome on property prices

Outcome	No. of studies	Evaluation reference numbers
Positive	3	1009, 1035, 1052
Mixed	1	1050

Four evaluations consider property prices. Three find positive impacts with one showing mixed results. Two of the studies look at average house prices in ‘treated’ areas and find positive impacts on house prices relative to ‘untreated’ areas.

The other two studies suggest that price effects depend on distance to the road project (consistent with the hedonic pricing literature that looks at the link from property characteristics to prices). Houses close to the project do not experience the same positive price rises as those close, but not immediately adjacent to, the project. They may even depreciate. The size of the ‘buffer’ zone in which these non-positive effects occur varies: in one study negative effects are present up to 0.2 miles, but positive from 0.25 miles away³²; while a second study shows overall positive effects on property prices in all treatment areas albeit with slightly smaller increases up to 0.4 miles from the intervention.³³

One of the two evaluations (study 1035) that considered the spatial pattern of distance effects also looked at whether these changed over time. It found little evidence of announcement effects (2 years prior to construction) but prices were already increasing close to the project in the first three years of the five year construction period.³⁴ After project completion, the greatest price appreciation was 0.4-0.8 miles away during the first 3 years following completion, 0.8-1.2 miles away 4 years following completion with effects disappearing in the fifth year.

30 Study 1017.

31 Study 1016.

32 Study 1050 shows negative effects of around 10% up to 0.2 miles from the toll-road corridor, while prices increase 13% at 0.25-1 mile distant and 19% at 1-2 miles distant.

33 Study 1035 shows that houses 0.4 miles from the highway are between \$22,000 and \$33,000 more expensive than those adjacent, with these positive effects diminishing as properties are located further from the highway.

34 These effects disappeared for years 4 and 5 of construction, which the authors speculate this can be attributed to increased noise externality during that period.

The general finding that price effects vary by distance from the project and time from construction may apply to other contexts (and as already noted the distance effect is consistent with the wider hedonic literature).³⁵ This could have important implications for predicting the impact of schemes and for incorporating land price uplift in appraisal and evaluation. We return to this issue in the conclusions.

Population

The impact of roads projects on local population may vary depending on whether the project is urban, suburban or rural.

Three evaluations considered the impact of roads on local population. All three evaluations looked at the effect of overall road investments rather than a specific project.

Study 1005 found that a new highway passing through a city centre leads to an 18% fall in population, while each 'ray' (a highway segment connecting the Central Business District with the suburbs) causes a 9% drop. This implies that the construction of a road may lead to suburbanisation.

Consistent with this, study 1015 found positive effects on housing development (and hence population) for non-urban areas within 0-0.75 miles of new roads.³⁶

Study 1027 comes to a similar conclusion that the linkage between highway infrastructure and growth patterns varies depending on the type of improvement and characteristics of the location. In urban settings, highways may lead to population decline³⁷ while in a suburban or rural context, population increases close to the highway³⁸.

As with the property price effects, it is hard to know whether these results generalise. However, as with the employment results, they emphasise the fact that local effects of road projects need not necessarily be positive.

Income/Wages

There is some evidence that road projects have positive effects on wages/income.

Only two evaluations considered the impact of road construction on income and/or wages with one study finding positive effects, the other reporting mixed findings.³⁹

The positive effects in study 1017 vary with the extent of changes in accessibility. Within 20km of new road construction projects (including new junctions, dualling, widening, upgrades and road construction) a firm experiencing the mean increase in accessibility saw a 0.2% average increase in wages (calculated as total wage bill per worker).

Study 1067 reports more mixed findings for rural counties in the vicinity of the United States Interstate Highway System. Counties with a high endowment of skilled workers saw increases in wages, while those with a low proportion of skilled workers saw decreases.

35 However it is unlikely that the exact pattern of effects reported in these two evaluations will generalise: indeed, study 1050 show that they vary depending on local context and the particular stretch of road evaluated.

36 These effects can be quite large. In one area (in Merced County), there was 65,501 more square feet of housing constructed per kilometer squared within 0-0.75 miles from the highway. Note, however, that this was partly offset by negative effects 2.7-3 miles from the highway.

37 Study 1005.

38 Study 1015.

39 Studies 1017 and 1067.

It is hard to generalise from these results – although study 1017 is the only study that focuses on road projects in the UK. They do provide some, albeit limited, evidence that productivity effects (which underpin UK WebTAG calculations of wider benefits) occur in practice. These productivity effects are also considered directly in two further studies which we consider next.

Productivity

There is some evidence that road projects have a positive effect on productivity.

Two evaluations consider the impact of roads on productivity. One study finds that, in general, the construction of or improvement to major roads leads to a 0.4% uplift in GVA per worker (though a 0.2% increase in workers' wages suggests that part of this productivity gain is paid out in increased salaries – these findings are complementary).⁴⁰

The second study also finds positive effects on provincial productivity with stronger effects for areas intensive in sectors that are more dependent on roads (e.g. manufacturing and logistics).⁴¹ Road use by provincial industries is proxied by these industries' vehicle intensity with the study finding a particular increase in use of roads by industrial sectors.

Along with the results on wages, this provides more direct evidence that the productivity effects that underpin WebTAG appraisal guidance occur in practice. Although, as should be clear from the discussion so far, the number of evaluations that can demonstrate a causal link from road projects to productivity is extremely limited.

Other Outcomes

Two evaluations considered impact on business and trade volume⁴². Both found positive impacts. The first of these observed a 1.4% uplift in the value of trade and a 1.9% increase in the volume of trade for every 1% reduction in travel distance between trading partners.⁴³ The same study also found a 10% increase in the stock of urban highways increased export weight by 5% (but did not induce an uplift in value). The second study found that trucking activity increased by 7-10 percentage points per capita in rural counties crossed by highways.⁴⁴

Two evaluations considered the impact on innovation (one in terms of outputs, the other in terms of inputs). The first of these considered the impact of Interstate highways in the USA on patenting activity⁴⁵. The findings suggest that a 10% increase in a region's highway stock caused a 1.7% increase in regional innovation growth over a five year period. The second study considered the effect of road infrastructure on GDP in NUTS1 and NUTS2 EU regions.⁴⁶ The evaluation found that the economic performance of regions with a good endowment of motorway infrastructure is enhanced when they have – and are surrounded by regions with – high levels of R&D investment.

⁴⁰ Study 1017.

⁴¹ Study 1062.

⁴² Studies 1037 and 1067.

⁴³ Study 1037.

⁴⁴ Study 1067.

⁴⁵ Study 1063.

⁴⁶ Study 1055.

Rail

Quantity and quality of the evidence base

Of the 232 shortlisted studies reviewed in detail, 95 considered the impact of rail projects.

Of these 95 studies, an additional 83 studies were discounted: Eight on grounds of relevance (e.g. they looked at an outcome not related to local economic growth, such as ridership levels),⁴⁷ and 65 on grounds of not meeting the Centre's minimum standard of evidence (i.e. scored 2 or below on the SMS scale). The remaining 12 studies have been included in this review.

This is a smaller evidence base than our reviews to date (on employment training, business advice, sports and culture projects, access to finance and estate renewal and broadband) as well as being smaller than that available for roads. As with roads, this partly reflects the difficulties in evaluating transport projects but is also indicative of a failure to carefully evaluate existing policy interventions. Table 5 shows the distribution of the studies ranked by SMS score.

Table 5: Ranking Studies by Quality of Implementation

SMS Score	Number of studies	Evaluation reference numbers
3	10	1070, 1071, 1074, 1083, 1107, 1108, 1111, 1112, 1114, 1116
4	2	1075, 1109
Total	12	

We found no studies that used randomised control trials, but two studies that used credible random sources of variation. As discussed for roads, this is not that surprising given the nature of these projects. The remaining ten studies used variations of difference-in-difference and panel techniques (scoring 3 on the SMS). The techniques applied in these studies mean that we can be reasonably confident that they have done a good job of controlling for observable characteristics of areas and individual households and firms affected by the projects. However, it is likely that unobservable characteristics may still be affecting the results.

Types and Focus of Support

As with roads, the studies included in the final shortlist did not generally evaluate specific policies (e.g. nationalisation/privatisation, co-ordinated rolling stock improvement schemes etc.). Instead, they either:

- Focussed on individual new rail projects in specific locations.
- Evaluated the impact of access to rail more generally.

The evaluated projects varied by both scale and type:

- Two evaluations looked at high speed rail. The first looked at the impact of new rail services between Cologne and Frankfurt, on two small towns that got new stations on the line.⁴⁸ The

⁴⁷ This included one high quality study (SMS level 4) which evaluated the impact of rail expansion in nineteenth century America on 'farm improvement' showing that counties that gained access to railways between 1850 and 1860 experienced significantly greater increases in the percentage of farms that were improved.

⁴⁸ Study 1075.

second evaluated the opening of a high speed (Shinkansen) rail line in Japan.⁴⁹

- Three evaluations looked at regional rail (i.e. connecting different cities or wider regions).⁵⁰ All three of these studies looked at rail access generally, rather than specific projects.
- Seven evaluations looked at light rail:
 - Study 1108 and 1114 both focused on a new light rail line in Charlotte, North Carolina.
 - Study 1071 evaluated the Hudson-Bergen Light Rail system in New Jersey.
 - Study 1070 evaluated the River Line rail line in New Jersey.
 - Study 1083 focused on the Metrorail in Miami.
 - Study 1111 evaluated light rail transit in Minneapolis.
 - Study 1112 studied the impact of extensions to the Docklands Light Railway and Jubilee Line in London.

Funding and delivery for the programmes are not stated in many of the evaluations, but where this is mentioned they are overwhelmingly publicly funded. Similarly, the objective of or rationale for the intervention is often not reported, though in some cases it is implied that at least part of the rationale was to boost economic growth.

Findings by outcome

A breakdown of the studies by outcome and overall finding is provided in the tables in Appendix A.

Property Values

Rail projects tend to have a positive effect on property prices, although the size of the effect varies considerably.

Table 6: Rail investment evaluations by outcome on property values

SMS Score	Number of studies	Evaluation reference numbers
Residential		
Positive	5	1070, 1071, 1107, 1108, 1112
Zero	2	1074, 1083
Commercial		
Zero	1	1108

Seven evaluations consider property prices.⁵¹ All seven studies consider the effect of proximity to new rail stations on residential property prices, with study 1108 also looking at commercial property prices. Five out of the seven studies that considered residential property found positive effects of proximity to stations, while two studies found no effect of proximity. The one study that considered commercial property prices found no effects of proximity to stations.

49 Study 1117.

50 Studies 1107, 1109, 1074.

51 Studies 1071, 1107, 1074, 1070, 1083, 1108 and 1112. Most evaluations measured changes in property values using hedonic models applied to repeated sales datasets.

For evaluations showing positive effects, the degree of price appreciation ranged from extremely small to quite substantial. For example, Study 1108 (which looked at the impact of light rail in Charlotte, North Carolina) found effects that ranged from near zero up to around 13%, depending on: the type of property (for example, condominiums see a greater increase than single-family properties); and proximity from the station (for example, single-family homes within half a mile of the station see no impact, whilst condominiums within half a mile are subject to a greater increase than those further away). Study 1071 found effects as high as 18.4%. This high variation in price effects across studies implies a similarly high variation in the implied value of improvement in rail access in terms of willingness to pay for residential housing.

In contrast, Study 1074 found that access to intercity rail connections in post-unification Berlin had no impact on property prices. Interestingly, the evaluation also found that the new mainline network had on average an adverse impact on mainline accessibility at the city level (as a result of the allocation of transport capacity favouring some lines over others, including the complete disconnection of a station in an area which had served as the CBD of West Berlin for decades), which may explain the lack of economic benefit. Study 1083, which looked at the impact of the Miami Metrorail on the value of houses near station locations, also found no statistically significant effects on residential property prices.

As with roads, a number of evaluations suggest that the price effects depend on distance from the project (consistent with the hedonic pricing literature that looks at the link from property characteristics to prices). Study 1071 finds positive effects up to a quarter mile from the station, with effects decreasing with distance. Study 1107 found similar evidence that effects decayed with distance, but effects were still positive up to 2.2 miles from the station. Results were similar for study 1070, although here effects were positive up to 4 miles. Finally, study 1108 suggests that the effect of distance may differ by property type with the largest effects for condominiums at half a mile, but the largest effects for one-family homes at a mile. In contrast to the findings for roads, none of the evaluations report smaller effects for properties very close to stations.⁵²

A number of evaluations also considered the timing of price changes. For example, Study 1107 (in the Netherlands) and 1083 (in Miami) found weak evidence of an announcement effect – i.e. the appreciation of property prices post-announcement, but pre-completion of the project. The length of time over which price effects are observed also varies by study. Some studies only look at effects one or two years after completion of a station (for example, Studies 1071, 1047); others considered changes over a much longer time period (for example, Studies 1070, 1112).

Other outcomes

Aside from the effect on property prices, the evaluation evidence on other local economic impacts is extremely limited. We found no evaluations that considered the impact on employment – which is startling given the degree of interest in the existence and size of these effects.

One evaluation (Study 1116) looked at the impact of a new Japanese high speed (Shinkansen) passenger rail line on business performance and business productivity, finding positive effects in both areas. Business performance is measured by sales revenue and business productivity is measured by sales revenue per employee. The authors suggest that these benefits occurred as a result of increased access and lower search costs to other markets, resulting in firms being able to find better suppliers. These benefits occurred despite the fact that the intervention only lowered the cost/time of

⁵² Although a number of the SMS 2 level studies did report smaller, or negative, price effects for properties very close to improvements.

passenger travel and did not affect freight transportation costs. Overall, the results indicated that for input-intensive firms sales per employee increased relative to the industry by 42%.

A second high speed rail study⁵³ looked at the impact on GDP for two small towns that lie on the new high speed line connecting Cologne and Frankfurt. The study reports quite large effects amounting to a 2.7% increase in total GDP. Unfortunately, the context that makes this study attractive from an analytical point of view (this study scores level 4 on the SMS scale) also makes the findings hard to generalise. The small towns of Montabaur and Limburg (populations of 12,571 and 33,843⁵⁴) happened to get stations following complex negotiations, despite their small size and peripherality. This helps address concerns about 'selection in to treatment',⁵⁵ but makes it impossible to know whether these GDP effects would extrapolate to the larger cities (e.g. Birmingham, London and Manchester) that would typically be home to new high speed rail stations.

The results from the only evaluation that considers population (study 1109) are similarly hard to extrapolate. This study looked at the impact of railway construction in Sweden in the nineteenth century, evaluating its impact on the growth of cities since that time. The study found that cities with early access to the network continued to grow faster over the first half of the twentieth century, and that the treatment group cities are on average 51% larger today compared to cities that did not gain access to the railroad network in the first wave of expansion. All of this suggests that effects may play out over the very long term, but it is hard to know whether these generalise to additions to an already existing well-developed rail network.

Perhaps of more interest to local decision makers is study 1111 which examined the impact of a metro line in Minneapolis on land use changes, finding only small and very localised impact. Specifically, single-family and industrial properties within half a mile of operational stations experienced a small increase in the likelihood of land use change away from these uses. However, on a larger scale the introduction of the new line did not increase the likelihood of changes in land use above normal levels, nor did it have any effect on the likelihood of changes in land use away from vacant land, commercial properties and multi-purpose families.

A final study⁵⁶ looked at the impact of rail on levels of crime. Study 1114, looking at the impact of new light rail transit in Charlotte, North Carolina, found that the announcement of the rail transit led to a decrease in property crimes, which was maintained even after the stations were opened. This was attributed to public and private decisions to invest along transport corridors, which gentrified surrounding neighbourhoods and decreased criminal activity. Once the stations opened, the decrease in crime was maintained and did not return to pre-announcement levels. As with a number of previous reviews, this finding serves to highlight the fact that infrastructure investment can deliver amenity benefits that are important, but separate to, the effect on local economic growth.

Other modes: Trams, Buses, Cycling and Walking

Of the 232 shortlisted studies reviewed in detail, 10 considered the impact of buses, 1 the impact of trams, 1 cycling; 1 walking. Unfortunately, from this total of 13 studies covering these four areas, we found no high quality evaluations that provide evidence on the impacts of trams, buses, cycling and walking schemes on any economic outcomes.

53 Study 1075.

54 Population statistics from Statistisches Bundesamt (2014).

55 See the Impact Evaluation for Transport Infrastructure section for further discussion of this selection problem.

56 Study 1114.



Summary of findings

This section summarises the detailed findings. We emphasise that many of these findings depend on a small number of studies. They are, however, consistent with other research on the broader impact of transport improvements.

What the evidence shows

- Road projects can positively impact local employment. But effects are not always positive and a majority of evaluations show no (or mixed) effects on employment.
- Road projects may increase firm entry (either through new firms starting up, or existing firms relocating). However, this does not necessarily increase the overall number of businesses (since new arrivals may displace existing firms).
- Road projects tend to have a positive effect on property prices, although effects depend on distance to the project (and the effects can vary over time).
- The impact of roads projects on the size of the local population may vary depending on whether the project is urban, suburban or rural.
- There is some evidence that road projects have positive effects on wages or incomes.
- There is some evidence that road projects have a positive effect on productivity.
- Rail projects tend to have a positive effect on property prices, although effects depend on distance to the project (and these can also vary over time).

Where there is a lack of evidence

- We found no high quality evaluations that provide evidence on the impact of rail infrastructure on employment, and only a limited number of evaluations showing that road projects have a positive effect.
- We found no high quality evaluations that provide evidence on the impacts of trams, buses, cycling and walking schemes on any economic outcomes.
- Even when studies are able to identify a positive impact on employment, the extent to which this is a result of displacement from other nearby locations is still unresolved. More generally,

the spatial scale of any employment effects varies and we do not have enough evidence to be able to generalise about the spatial distribution of effects if they occur. The same is true for other outcomes. The scale at which the studies evaluate impact varies from adjacent neighbourhoods to much larger US counties.

- Surprisingly, very few evaluations consider the impact of transport investment on productivity (we found just three studies, two for roads and one for rail). Although the use of such productivity effects to calculate 'wider economic benefits' in transport appraisal is underpinned by a larger evidence base, it is still worrying that so few evaluations can demonstrate that these effects occur in practice.
- We have little evidence that would allow us to draw conclusions on whether large-scale projects (e.g. high speed rail or motorway construction) have larger economic growth impacts than spending similar amounts on a collection of small-scale projects (e.g. light rail or junction improvements).
- More generally, we do not know how differences in the nature of improvements (e.g. journey time saved or number of additional journeys) affect any local economic outcomes.
- There is some evidence that context matters. For example property price effects may depend on the type of property, while wage effects may differ between low skilled and high skilled workers. But, once again we do not have enough evidence to be able to generalise.

How to use these reviews

The evidence review highlights a number of factors for policy makers to be aware of when considering transport policy:

- Much more empirical work remains to be done on understanding the impact of infrastructure improvements on local economic growth. The economic benefits of transport infrastructure spending – particularly as a mechanism for generating local economic growth – are not as clear-cut as they might seem on face value.
- While it is understandable that political debate focuses on expenditure figures across different parts of the UK, they do not help answer the question of what would happen if expenditure was distributed differently. Arguments for spending more in areas that are less economically successful hinge on the hope that new transport is a cost-effective way to stimulate new economic activity. As this review shows, we do not yet have clear and definitive evidence to support that claim.
- This raises fundamental questions about scheme appraisal and prioritisation, and about the role of impact evaluation in improving decision-making around transport investment.

Helping to fill the evidence gaps: improving evaluation and appraisal

In many instances local economic impact is an important part of the case for transport investment. Such investment also forms a central component of many governmental policy initiatives aimed at increasing local economic development (e.g. the UK government's Local Growth Deal process). It is therefore vital that progress is made in filling the evidence gaps and in improving our understanding of the effect of transport improvement on local growth.

In this final section, we make some preliminary recommendations building on recent work with the Department for Transport, as well as the discussions of a LEP working group convened by the What Works Centre for Local Economic Growth. Our recommendations focus on the need for more, and better, ex-post impact evaluation and the need to embed such evaluation in to the appraisal process.

Considerable resources are already devoted to the ex-ante appraisal of transport schemes as part of the decision making process. Cost-benefit analysis plays a central role in such appraisals. The increased interest in the effect of transport investment on the local economy has also been accompanied by growing criticism of the appraisal approaches used to help facilitate scheme prioritisation. In particular, there is growing criticism that the current approach to cost-benefit analysis does not capture all of the benefits that may be associated with transport investment.

If the main aim of new transport infrastructure is faster journeys, then benefits to the economy materialise because time saved can be used on productive or otherwise valuable activities (either in business or leisure). This is why the most fundamental input into transport infrastructure cost-benefits analysis has traditionally been the so called 'value of travel time savings'. This is travel time saved, converted into monetary units. These monetised time savings are a crucial measure of the economic benefit from transport investment (and can be supplemented by monetised estimates of the benefits of reductions in other costs like accidents and unreliability).

But over the past 15 years there has been greater interest in the potential for transport to generate 'wider economic benefits' that go beyond these travel times savings (in addition to a range of other wider social and environmental benefits). In the context of UK appraisal a particular focus, in terms of wider economic benefits, has been on those that come from effectively bringing people and businesses closer together to form agglomerations of economic activity. The logic follows from the observation that cities are more productive than rural places and big cities are more productive than smaller cities. So linking places together may help generate productivity improvements.

Despite these improvements to appraisal practice, there continue to be concerns that appraisal misses important benefits of transport investment – particularly in terms of the impact on local economic growth. Some of these issues were recently considered in an independent report for the Department for Transport (Laird, Overman and Venables, 2015). The report concludes that, in some cases, traditional cost-benefit analysis may indeed miss important benefits that should be included in the analysis (although the Department's WebTAG guidance includes almost all of them).

The report also argues that there may be instances in which local decision makers are interested in the local economic effects of transport – e.g. on employment and investment – even when these should not be included in a cost-benefit analysis which seeks to evaluate the overall (i.e. national) gains from a new project. A concrete example would occur when employment growth near to new transport investment is purely driven by displacement from elsewhere in the economy. A traditional cost-benefit analysis – which tries to assess the overall gains to society – would ignore such displacement. But these effects may be of legitimate interest to local decision makers.

DfT is planning to refresh WebTAG (the set of procedures which outline how appraisals should be conducted) to respond to these observations. While such an exercise will be welcomed by many, the findings in this review also highlight the importance of complementing any further work on the ex-ante appraisal framework (i.e. analysis to predict what might happen) with additional work to strengthen the ex-post evaluation of transport investment (i.e. analysis of what actually happened). Not least because, as this report makes clear, there is limited evidence that the employment (and other effects) that would underpin any changes to the guidance actually occur in practice.

What form should such ex-post evaluation take? As discussed above, for capital expenditure, where investments are durable, it is hard to imagine situations in which true randomisation of project placement would be either feasible or desirable. This means that we need to rely on alternative evaluation approaches

to try to identify the causal impact of transport investment. On the basis of our review work, plus our wider work on the issue of transport evaluation we think that work to develop a new approach is urgently needed. Any such approach needs to be both feasible and proportional. It also needs to produce evidence that is helpful in improving future decision making. Unfortunately, many existing studies appear to have cost much but with arguably little benefit in improving decision making. There are a number of possible avenues that can be explored and issues that will need to be considered. For example:

1. At present the Highways Agency undertakes Post Opening Project Evaluation (POPE) of a large number of schemes. This takes two forms – one, lighter touch, for smaller projects (Local Network Management Schemes); and a more extensive evaluation for larger projects (Major Schemes). POPE aims to determine how schemes have performed in their opening year and, for major projects, five years after opening. Findings are compared to ex-ante appraisal to assess accuracy and an annual meta-study pulls together findings from all POPE undertaken that year. POPE uses before and after analysis of scheme data – an approach which would score level 2 on the SMS scale. More recently, the DfT has issued a Monitoring and Evaluation Framework for Local Authority Major Schemes (LAMS) that provides guidance that outlines a POPE style approach for those schemes. This guidance outlines three approaches: standard and enhanced monitoring which parallel the smaller projects approach in POPE; and fuller evaluation which is closer to the POPE guidance for Major Schemes. As with POPE, there is a strong emphasis on before and after comparisons.⁵⁷
2. It would be helpful to consider how the use of appropriate control groups could refine these processes. Interestingly, the most recent POPE guidance for major schemes has recognised the importance of controlling for the background reduction in the number of collisions when assessing benefits from accident reduction. LAMS also places more emphasis on the use of control groups – at least for the fuller evaluations (see, for example, the discussion of control groups in the assessment of changes in travel behavior). It would be useful to undertake further work to identify appropriate control groups and to encourage their use for benefits which might be most affected by other ‘background’ changes. Control groups could be constructed in a number of ways: For example a) for similar parts of the network that have not been subject to improvement⁵⁸ b) from schemes that are likely to be funded in the future but have not yet been funded; c) from schemes that have similar benefit-cost ratios but were declined funding; d) for areas close to funded schemes that are not directly affected by the scheme. More simply, appropriate area wide averages (which would ideally exclude new schemes) could be used to provide a very basic control group. Similar approaches could be used to identify suitable control groups when using individual level data (e.g. on travel behavior). There will, of course, be pros and cons to all of these approaches and further work would be needed to consider the alternatives (and whether the benefits in terms of improved POPE and LAMS analysis, outweigh the additional costs).
3. Although POPE considers performance of each scheme against the Government’s four WebTAG objectives (economy, environment, society and public accounts) the analysis of economic impacts tends to focus on direct benefits – particularly in the form of reduced accidents and improved journey times. This is unsurprising given the current focus of POPE on performance. Interestingly, while the lighter touch approaches in LAMS give less consideration

⁵⁷ A third set of guidelines consider the monitoring and evaluation framework for the Local Sustainable Transport Fund. Many of the points we make here could also apply to development of the LSTF framework but the text focuses on POPE and LAMS which have clearer parallels and a stronger impact evaluation focus than LSTF.

⁵⁸ This is the approach is similar to that used for collisions – at least for key links – where adjustment is based on national trends for that type of road (although some proportion of the network will also have benefited from improvements).

to some WebTAG objectives⁵⁹ they place more emphasis on economic benefits – particularly in terms of effects on employment and rental values. POPE for major schemes does allow for the assessment of wider economic benefits with the consideration given to these benefits varying according to the level of POPE that is undertaken. At its most extensive, this will involve a survey of local business at one year to ‘identify emerging concerns or positive outcomes associated with the scheme’ and at five years a ‘focused survey of businesses to identify wider economic impacts’. Given the increased interest in wider economic effects, we think that this process could be improved to better align the POPE and LAMS processes to ensure that both carefully assess these economic effects.⁶⁰ As is made clear in LAMS, not all schemes would warrant such an analysis, but this should be considered when employment, or other local economy effects, are an important component of the strategic or economic case for major schemes. Results from this analysis are unlikely to be useful in isolation. Both LAMS and POPE highlight the importance of comparing outcomes to key appraisal assumptions.⁶¹ But we would also highlight the importance of broader comparisons to both the strategic and economic cases that form part of the appraisal process.

4. Once again, it will be important to consider how the use of suitable control groups could play a part in the analysis of these economic effects. As LAMS recognises, there are strong arguments in favour of developing such an approach – at least for larger schemes. Some of the options for constructing such control groups were discussed under point (2). It would also be useful to consider whether a light touch approach could be developed for smaller schemes.
5. There needs to be a much closer link between the ex-ante appraisal and ex-post evaluation of schemes. Our review of the literature discovered a large number of ex-post evaluations that appear to live in a vacuum,⁶² with no attempt made to link the findings from these reports back to scheme appraisals. Higher quality impact evaluations – i.e. those that seek to identify the causal impact of investments using changes in outcomes compared to a control group (i.e. are scored SMS 3 and above and included in our review) – are still helpful even in the absence of such comparisons. These are the studies that we have used in this review. This is, unfortunately, not so true for less robust evaluations (e.g. those involving simple before and after comparisons). Embedding evaluation in to the scheme prioritisation process is an important step in ensuring that money spent on ex-post evaluations is cost-effective in improving prioritisation for future spending. One of the advantages of incorporating the evaluation of wider economic impacts within an improved POPE methodology is that these comparisons are already part of the POPE ‘meta-analysis’ process. It will be important to develop a similar ‘meta-analysis’ for LAMS that parallels the POPE process. These comparisons across evaluations should allow findings on scheme effects and the comparison to appraisal assumptions to be used to improve scheme prioritisation (for example, through the use of optimism bias to adjust predicted employment effects). Given the interest in the economic impacts of investment in other transport modes we should consider how and when a similar approach could be extended to such schemes not covered by POPE or LAMS.
6. In line with the recommendations of the DfT TIEP report, there should be far greater attention paid to the critical analysis of both the economic and strategic cases for support. This

59 For example, in LAMS environment is considered in enhanced monitoring, but not in standard monitoring.

60 In POPE, in particular, this would involve bringing the approach to wider economic effects in line with those used to more carefully measure changes in journey times, etc.

61 For example, POPE systematically compares the monetised value of changes in accidents and journey times to the ex-ante appraisal predictions.

62 Indeed, a number of evaluations are not (easily) accessible even when they have been publically funded. While concerns over commercial confidentiality may be problematic for some aspects of the ex-post evaluation (as for appraisal) routine publication of publically funded evaluations should, arguably, be the norm.

critical analysis should occur both ex-ante (on the basis of available evidence – including that covered in this review) and ex-post (on the basis of appropriate evaluation – including analysis developed according to the recommendations above).

7. Ex-post evaluation needs to include due consideration of the extent to which any employment effects are likely to result from displacement (the shifting of jobs from one place to another). Addressing concerns over displacement will need to be a key question in understanding the net impact of investments. There are similar concerns over spillover effects whereby employment growth resulting from transport improvements drives growth in areas not directly affected by the scheme. However, for both these questions carefully identifying any direct employment impact is a first step in understanding these other factors. Concerns over displacement and spillovers should not prevent progress in asking the simpler question as to whether any changes in employment occur directly as a result of the scheme. Work is needed to tackle all three questions – what is the employment effect; are any local employment changes additional; do these spillover to wider areas? A similar point holds with regard to variations in effects across schemes. Work to identify the average effect should be a first step in understanding how variation in effects depends on context. Again, concerns over heterogeneity of effects should not prevent progress on the simpler question of identifying average effects.
8. The current LAMS guidance, including a comparison to appraisal assumptions, would appear to provide an appropriate framework for undertaking and improving evaluation and scheme prioritisation for individual LAs/LEPs. However, there is a role for DfT in helping develop the guidelines for how this analysis could be conducted and improved along the lines of points (2) to (7). This will ensure, as with POPE, that results for specific LAs/LEPs are transferable across areas. The devolution agenda raises questions about the extent to which such an approach could (or should) be mandatory. Regardless of the outcome of that debate, many LAs/LEPs would still welcome guidance on how best to proceed – especially given local constraints on analytical capacity.
9. Consideration needs to be given as to how to ensure LAs/LEPs have the incentive (and the resources) to collect data in control/comparison areas. It is possible that central government departments could provide appropriate area data (and the use of such secondary data would substantially reduce the cost implications of undertaking evaluations). Where data is collected at the local level it will be important to ensure that such data are available to researchers for use in aggregated/multi-intervention analysis.
10. Given the complexity of many of the issues raised above, and the need for comparison across areas, it is likely that DfT will need to play a coordinating role in addressing many of these evaluation challenges. There is also a role for DfT in undertaking multi-intervention ex-post analysis using the kind of approaches used by the higher quality studies considered as part of this report.

Further work would be needed to develop the issues discussed here and to consider appropriate solutions. It is crucial that further work recognises the importance of embedding evaluation in to the scheme prioritisation process (DfT is currently undertaking work on this issue). This means better aligning appraisal and evaluation, particularly if the objective is to improve scheme prioritisation. Without closer integration, there is a danger that we undertake refinements to the appraisal process – e.g. to include employment and investment effects – without knowing the likely magnitude of effects, whether they are additional, etc. Similarly, while ex-post evaluation can serve some role in terms of monitoring and accountability, its main aim should be in improving future decision making. This means thinking about ways in which evaluation can feedback in to the scheme prioritisation process – both in terms of developing ex-ante appraisal, but also in providing a means of scrutinising strategic cases for future investment (again, an area in which DfT is currently undertaking work).

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Appendix A: Findings by outcome

Table A1: Roads

Outcome	+ve	Zero	-ve	Mixed	No. +ve
Employment	1011, 1017,	1027, 1031, 1067	-	1015	2/6
Property Values/Rents	1052, 1009, 1035	-	-	1050	3/4
Business Volume/Sales	1037, 1067	-	-	-	2/2
Firm entry and number of businesses	1017, 1061	1016	-	-	2/3
Productivity	1017, 1062	-	-	-	2/2
Innovation	1063, 1055	-	-	-	2/2
Income/Wages	1017	-	-	1067	1/2
Population	-	-	1005	1015, 1027	0/3

Table A2: Rail

Outcome	Total evaluated	+ve	Zero	-ve	Mixed	No. +ve
Economic						
Property Values	6	1070, 1071, 1107, 1108, 1112	1074	-	-	5/6
GDP	1	1075	-	-	-	1/1
Business Productivity	1	1116	-	-	-	1/1
Non-economic						
Population	1	1109	-	-	-	1/1
Crime-reduction	1	1114	-	-	-	1/1

Note: In addition to outcomes reported here, Study 1111 showed evidence of changes in land use as discussed in the text.

Appendix B: Evidence Reviewed

Roads

Ref No.	Reference
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Ref No.	Reference
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Find the full list of search terms we used to search for evaluations on our website here:

whatworksgrowth.org/policies/transport/search-terms.

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