A circular graphic with a teal background and a semi-transparent image of industrial machinery. The text is overlaid on this graphic.

Evidence Review 10

Area Based
Initiatives:
Enterprise Zones

January 2016



what works centre for
local economic growth



Contents

Preface	3
Executive Summary	4
Introduction	9
Impact evaluation	12
Methodology	15
Definition	18
Findings	21
Summary of findings	37
References	40
Appendix A: Findings by outcome	41
Appendix B: Evidence Reviewed	44



Preface

This report presents findings from a systematic review of evaluations of Enterprise Zones and other economic area based initiatives. These are programmes that aim to improve economic growth in a specific, well-defined, local area or set of areas. A companion report covers the findings related to EU Cohesion Policy, especially Structural Funds spending.

This report is the tenth review produced by the [What Works Centre for Local Economic Growth](#). The What Works Centre is a collaboration between the [London School of Economics and Political Science](#), [Centre for Cities](#) and [Arup](#) and is funded by the [Economic & Social Research Council](#), [The Department for Communities and Local Government](#) and [The Department for Business Innovation & Skills](#).

These 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 area based initiatives can be rigorously evaluated and in drawing out the wider lessons for policy.

Evidence on impact and effectiveness is a crucial input to good policy making. Other ways of considering the impact of area based initiatives (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



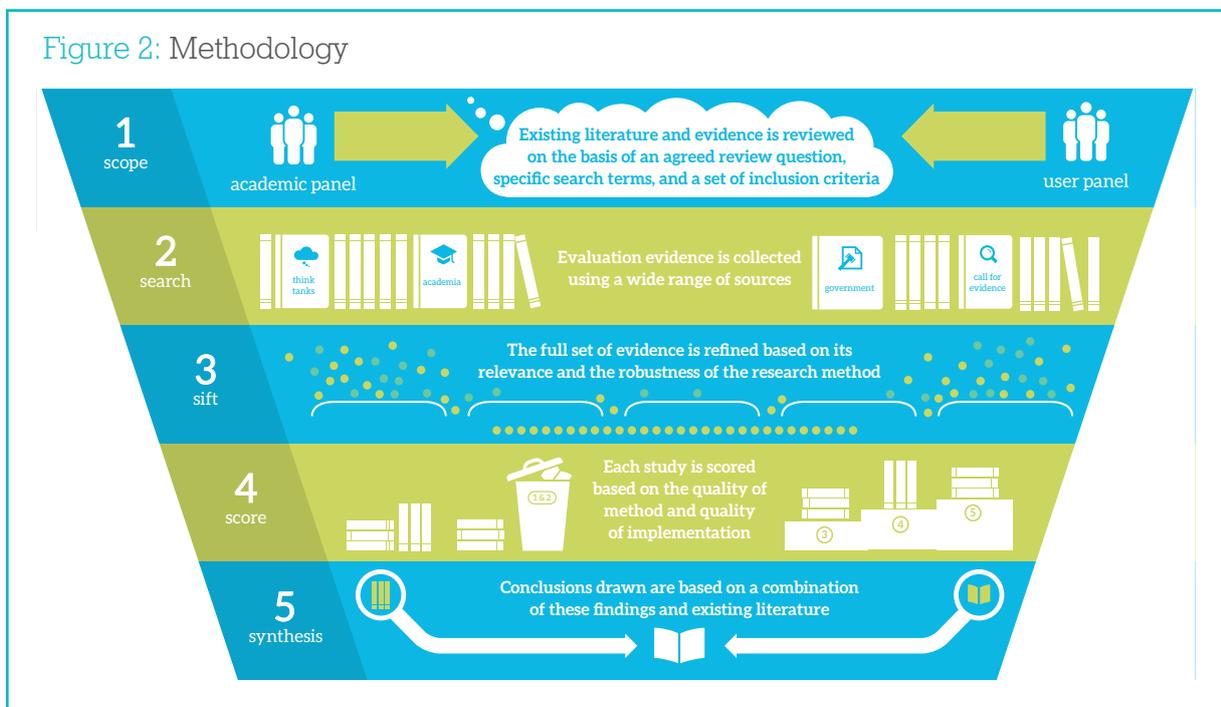
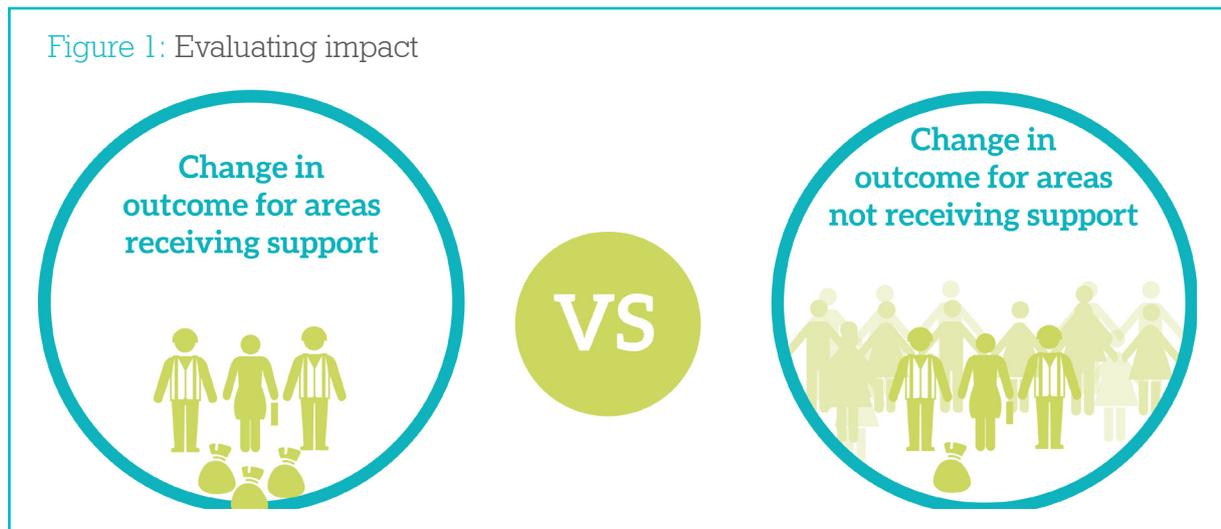
Executive Summary

This report presents findings from a systematic review of evaluations of EU growth and cohesion policies, Enterprise Zones and other economic area based initiatives. These are programmes that aim to improve economic growth in a specific, well-defined, local area or set of areas. This review is the tenth produced by the What Works Centre for Local Economic Growth. The report focuses on Enterprise Zones and other economic area based initiatives. EU programmes are discussed in a separate report published alongside this one.

The review considered more than 2,100 policy evaluations and evidence reviews from the UK and other OECD countries. It found 58 impact evaluations that met the Centre's minimum standards. We divided these into three groups: evaluations of EU policies (such as EU Structural Funds); Enterprise Zone evaluations; and a smaller set of evaluations covering other area based initiatives (such as Regional Selective Assistance).

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 support and the average outcome they would have experienced without support (see Figure 1). That is, shortlisted studies use evaluation methods that take deadweight into account and focus on additional impacts, if any. Our methodology for producing our reviews is outlined in Figure 2.



Summary of findings: EZs

What the evidence shows

- The preponderance of the evidence relates to US Enterprise Zones, US Empowerment Zones, and French Enterprise Zones.
- A little over half the reviews find positive impacts on zone employment. Evidence of employment effects is weakest for US Enterprise Zones and better for US Empowerment Zones
- Most of the reviews that consider unemployment find positive effects.
- Half of the studies that consider the impact on poverty report positive effects.
- Half of the evaluations that consider wages report positive effects.

- Most of the reviews that consider the number of businesses find positive effects.

Where there is a lack of evidence

- A number of studies suggest that positive effects for Zones may be driven by displacement from nearby areas. However, we have little evidence on whether overall effects at the wider area level are positive, or whether displacement is the main effect of EZ-type schemes.
- Very few studies look at differences in programme characteristics. As a result, we do not know how characteristics of programmes (e.g. selection, local employment conditions) change effectiveness, including the extent to which they may reduce displacement.
- One study suggests that programmes with fewer target areas are more effective in attracting new jobs and business activity; and that employment growth in existing target businesses is promoted only if programme incentives are tied to hiring requirements. It would be good to know if these findings generalize.

Lessons

- Decision makers need to take concerns over displacement seriously. If much of the growth within the zone comes at the expense of nearby local areas, then this will mean less (or even no) overall growth at the wider area level.
- Even if displacement effects are strong, EZs may play a role in helping concentrating local employment from a number of dispersed sites.

Summary of findings: other ABIs

What the evidence shows

- More than half of the studies that consider employment report positive effect.
- Only three studies look at unemployment, with all three reporting positive effects.

Where there is a lack of evidence

- We have very few estimates for other outcomes of interest.
- There is some evidence that targeting of schemes to firms that provide traded goods and services could make a difference in determining the extent of displacement, but this finding is based on only two evaluations of different schemes.

How to use these reviews

In the UK, the devolution of business rates offers an opportunity for local authorities to make their own decisions about using EZ-type programmes to address local economic conditions. However, the relative power of this incentive (which represent a small proportion of business operating costs) should not be overestimated.

It is also clear that decision makers need to take concerns over displacement seriously. If much of the growth within the zone comes at the expense of nearby local areas (e.g. within the same same LEP or city-region), then this will mean less (or even no) overall growth at the wider area level. Even then, EZs and other ABIs may play a role in helping concentrating local employment from a number of dispersed sites. There has been some suggestion that this may lead to additional productivity effects via agglomeration benefits. Given existing estimates of the effect of increased concentration on

productivity, and the extent of clustering involved, these effects are unlikely to be large in practice. Of more importance, are the implications for public service provision of more concentrated employment. For example, concentrating employment on a smaller number of sites, may help reduce costs of infrastructure provision such as transport, broadband and other services to business.

Given the small number of studies for other schemes, it is hard to draw any general conclusions in terms of policy effectiveness that might extend to EZ-type schemes. It is intriguing that a couple of UK studies suggest that the extent of displacement is much greater when schemes are non-selective and target non-traded services (LEGI) than when they are selective and target firms that serve non-local markets (RSA). This is certainly in line with a large body of theoretical and wider empirical literature, and taken together, this wider literature and the evaluation evidence urge some caution in the area specific targeting of firms that provide non-traded goods and services.

This review of ABIs reinforces two other overarching issues for policymakers in the UK:

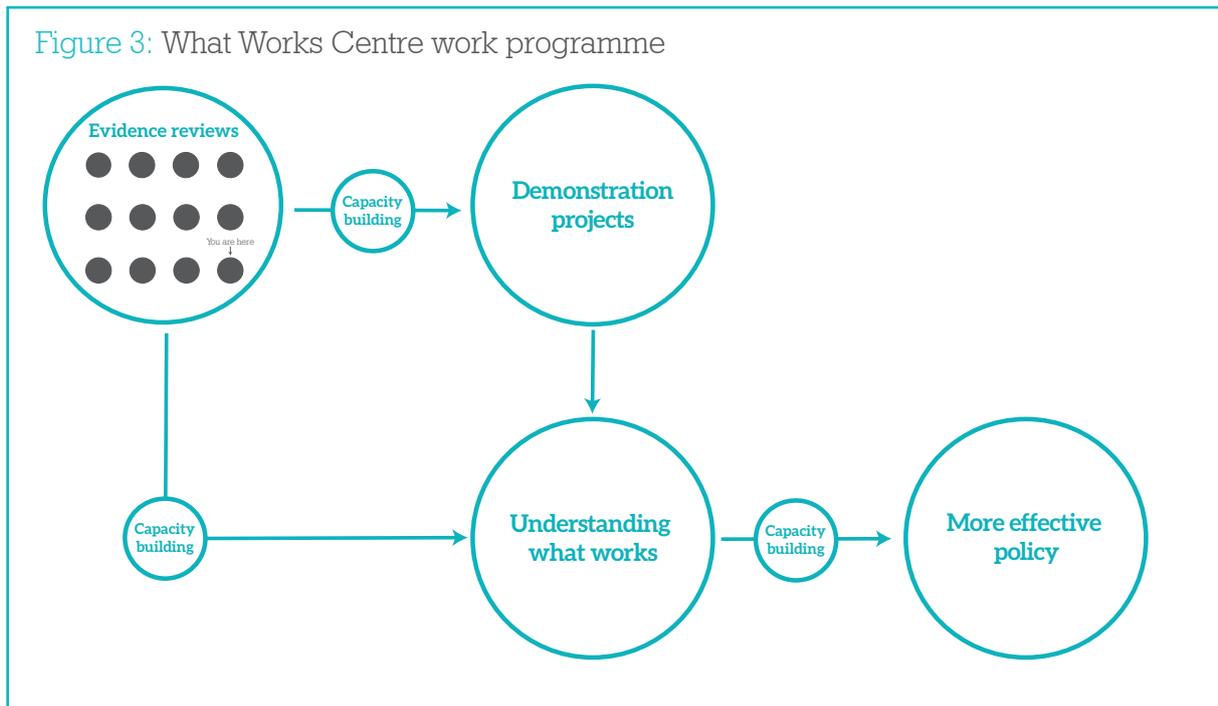
- Objectives of any area based policy must be very clearly defined, and the more specifically they can be targeted in terms of outcomes the better. The likely impacts of incentives across the targeted area, on adjacent areas, and over time, must be considered in the light of local conditions and objectives.
- We must make progress in the evaluation of the UK-style EZs if we are to say with confidence whether they are providing good value for money.

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.

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.





Introduction

This review looks at the impact of Enterprise Zones and other economic area based initiatives (such as Regional Selective Assistance). A companion review looks at the impact of EU funding, specifically EU Cohesion Programmes such as the Structural Funds.

Area based initiatives (ABIs) are policy initiatives aimed at tightly defined geographical areas, and provide a package of support aimed at improving economic, social or environmental outcomes within the zone. ABIs are very popular in many countries, as a tool for trying to tackle concentrated social or economic deprivation, especially in areas experiencing long term decline.

In this review we focus on ABIs that are designed to improve economic outcomes in programme areas. These interventions may target firms, households or the physical environment of the area itself. Some policies tend to focus on only one of these dimensions, others may target multiple dimensions. The policy mix varies, but typically includes some or all of tax breaks, wage subsidies, reduced regulation or improved physical / transport / communications infrastructure.¹

This report looks at the impact of a popular form of ABI often referred to as 'Enterprise Zones' (or similar), which involves firms receiving tax breaks and other incentives to locate or expand in specific areas. For example, in 2011 the UK government announced the creation of 21 Enterprise Zones in England (subsequently expanded to 24 zones).² These Enterprise Zones offer firms five year rebates on business rates (the UK's business property tax), simplified planning regulations and access to superfast broadband; in some cases firms are also able to claim tax relief on new plant and machinery investments. At the same time, local authorities will be able to keep 100% of any subsequent business rate growth for 25 years after the Zone begins operations.

Given the policy interest in these particular schemes (and confusion about their likely impacts), it is worth considering their possible effects in a little more detail.

Many economists worry that such schemes may be effective at redistributing activity within urban areas, but may not be very effective at redistributing across urban areas.³ When firms think about

1 Lawless et al (2011), Neumark and Simpson (2014).

2 <http://enterprisezones.communities.gov.uk/>, accessed 13 October 2015.

3 Glaeser and Gottlieb (2008); Cheshire et al (2014).

where to locate they are trading off the benefits and costs of producing in different places. When places are very similar in terms of costs and benefits – as might be the case for two neighbouring office developments – anything that lowers costs will have a big impact in terms of attracting new firms (or encouraging the expansion of existing firms). On that basis, an EZ in a given city may pull in activity from other parts of the city; the EZ will be ‘successful’ (with more firms and jobs than before) but city-wide levels of economic activity may stay the same (as firms and employment simply shift from outside the Zone to inside it). In practice, the extent to which activity is additional may depend partly on the strength of the wider urban economy, as we discuss below.

Once we start thinking about comparisons across different cities, any tax breaks (or other financial incentives) provided in poorer cities may be easily offset if productivity in those cities is too low or if labour or land costs are too high (e.g. due to national pay setting or local land regulations). This could then make a policy ineffective at larger spatial scales (for example, if we are using EZs as a tool for rebalancing the national economy). The spatial scale at which effects occur, and the extent to which any positive effects are driven by displacement, will be a central focus of our discussion of the evaluation evidence.

If labour markets are very local then even these shifts may benefit local residents within the zone. However, if labour markets are relatively large compared to zones, then this shifting employment may have no impact on employment for residents living within the zone (because jobs are taken by other workers who commute in). This makes it important to distinguish between effects on employment and on unemployment.⁴ Again, this is something that will consider in our discussion of the evaluation evidence.

To better understand the overall impact of these type of policies it is useful to distinguish between a) what happens in the Enterprise Zone, and b) wider effects on local employment and growth. In areas with strong economies, planning may act as a brake on business expansion and development, and local governments may have few incentives to allow more development. In these circumstances, EZs could help encourage growth. Some of this growth would come at the expense of other areas in the UK, but much of it could be additional. Overall, we might reasonably expect both local and national employment and growth to increase.⁵

But EZs, in Britain as elsewhere, have tended to be located in areas with weak economies. Here, the fundamental problem is that these are relatively unproductive places for business investment. The evidence on past waves of UK Enterprise Zones suggests that, with one or two exceptions such as the Docklands, they generated substantial physical improvements, but little additional employment benefit relative to their cost.⁶ Our review considers the extent to which this is confirmed by higher quality evaluations of similar international schemes.

Of course, the outcomes of the current set of English EZs may be different from the historical or international experience (not least because compared with past versions of the policy, current policy aims to locate Zones in higher growth cities and city neighbourhoods).⁷ However, by construction, incentive packages for businesses will not address any more fundamental area-level problems, such as the educational level of the local labour force, which hold back productivity in those places. As we have just discussed, it is also possible that much of EZ-related growth would come at the expense of

4 See analysis by Busso et al (2013) on US Empowerment Zones (study 1220).

5 Kline and Moretti (2013).

6 Larkin and Wilcox (2011).

7 Unfortunately, we are not aware of evidence that would allow us to assess whether this is in fact the case for the zones selected so far.

other areas in Britain – most likely in the form of displacement from other areas very close by. In short, the effects of support may differ depending on the type of area, something that we will again need to consider in our review of the evidence.

One way to mitigate displacement effects is to have the policy apply at a larger geographical scale. Of course, the problem with this is that unconditional support across a large geographical scale would soon become very expensive. Many EU programmes are able to do this, and operate at large scales: for example, Structural Funds target whole regions and operate across multiple member states. These EU programmes also offer a broader package of support, as we discuss in more detail in our companion review.

An alternative approach is to make support ‘selective’, perhaps conditional on characteristics of the firm, or on certain firm behaviours. For example, in some cases support is targeted at firms that mainly serve national or international markets; in others support is conditional on firms hiring a certain proportion of workers from the target area, as in US Empowerment Zones or French Urban Free Zones (ZFUs). We will consider evidence on whether these restrictions can make a difference to policy effectiveness in the third part of this review.

In contrast to subsidies for firms, some ABIs target the characteristics and skills of households (e.g. to improve education or labour market participation). Generally speaking, we do not cover these kind of schemes in this review. That said, a number of the schemes we consider involve a component of support for households). As with many programmes, the boundary lines are sometimes fuzzy, but the focus in this report is on schemes that tend to support businesses either directly or indirectly. For similar reasons, we do not cover schemes that specifically target improvements to the built environment. Although, again, some of the area based schemes we consider will involve an element of expenditure that does this.

A number of the schemes that we consider – particularly the EU schemes – will involve a substantial component of infrastructure investment alongside economic incentives. Our previous reviews on broadband and transport investment discuss the likely effects of such investments.⁸

8 See: <http://www.whatworksgrowth.org/policies/>



Impact evaluation

Governments around the world increasingly have strong systems to monitor policy inputs (such as spending on a programme) and outputs (such as the number of new business in an Enterprise Zone). However, they are less good at identifying policy outcomes (such as the extent to which new firms in the zone increase overall area employment). In particular, many government-sponsored evaluations that look at outcomes do not use credible strategies to assess the causal impact of area based initiatives.

By causal impact, the evaluation literature means an estimate of the difference that can be expected between the outcome for areas receiving support and the average outcome they would have experienced without the support. Pinning down causality is a crucially important part of impact evaluation. **Estimates of the benefits of a programme are of limited use to policymakers unless those benefits can be attributed, with a reasonable degree of certainty, to that programme.**

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 programme 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 supported by the policy) and the 'control group' (locations not supported by the policy). As we discuss below, in the case of area based interventions, 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 receive support differ from those who do not do so.

For most area based initiatives, selection problems usually lead to downward bias. Areas targeted for support have weaker economies, so we may mistakenly attribute poor economic performance in the future to the programme rather than to underlying conditions in the area.

It is possible that an area based initiative might be targeted at relatively successful areas. If this happens, estimates of policy impact may be biased upwards because we incorrectly attribute better economic outcomes to the programme, rather than to the fact that the area is already performing better than average.

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**, especially for economic ABIs, where policymakers may understandably be unwilling to randomise the location of projects.⁹

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 becoming (say) EZs, or becoming EZs before other areas do.

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.

Evidence included in the review

We include any evaluation that compares outcomes for areas part of the programme (the treated group) after they receive support with outcomes in the treated group before they receive support; relative to a comparison group used to provide a counterfactual of what would have happened to these outcomes in the absence of the programme.

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,

⁹ Gibbons, Nathan and Overman (2014).

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

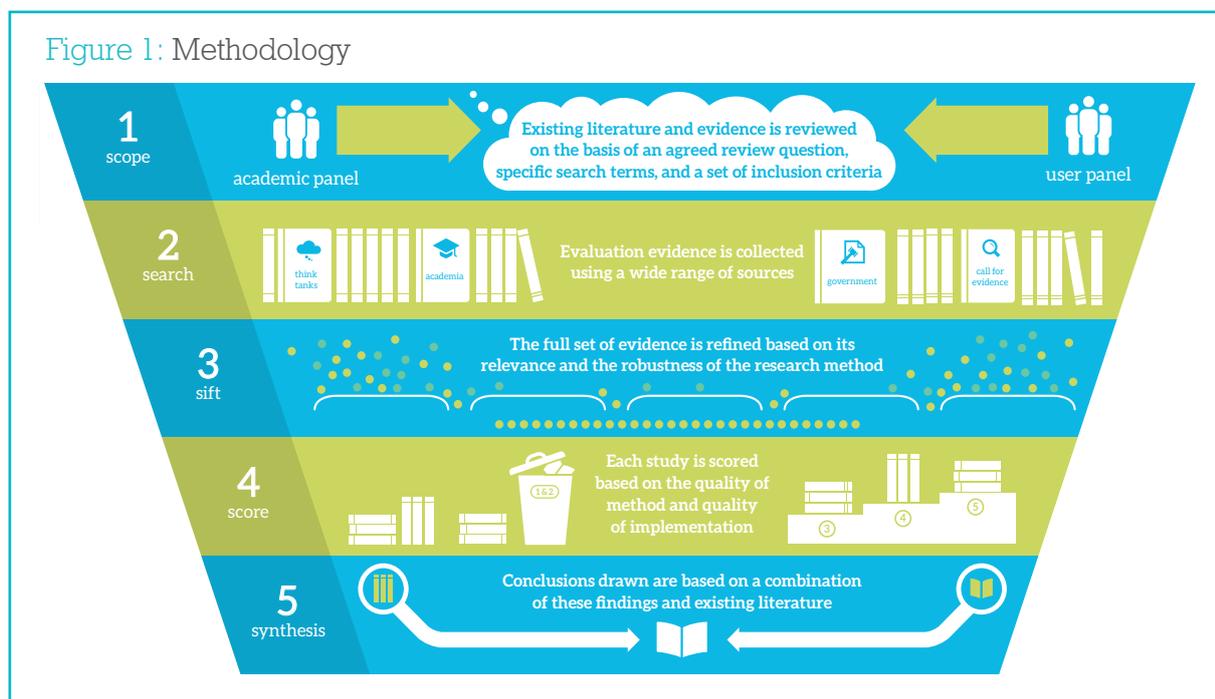
We exclude evaluations that provide a simple before and after comparison only for areas designated as Enterprise Zones, because we cannot be reasonably sure that changes for the treated group can be attributed to the effect of the project.

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 area based initiatives 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 2,100 books, articles and reports.

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.¹⁰ 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 SMS.¹¹ 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.

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 member 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.

¹⁰ Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway (1998).

¹¹ 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.



Definition of Area Based Initiatives

As discussed in the introduction, area based initiatives (ABIs) encompass a broad range of interventions at a variety of scales and of a variety of types. This evidence review focuses on three distinct types of support:

- EU Cohesion Policies (such as the European Social Fund and the European Regional Development Fund);
- Enterprise and Empowerment Zones;
- Other area based business support (such as Regional Selective Assistance).

We provide further details on coverage in the relevant sub-sections. This report focuses on Enterprise and Empowerment Zones, plus other types of ABIs. A companion report focuses on EU Cohesion Policies.

Impact evaluation for area based initiatives

Evaluating the economic effects of area based initiatives is challenging: they potentially 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 that explicitly target specific areas. In addition to our substantive interest in the impacts of policy, one of our motivations in considering area based initiatives 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.

Evaluation of area based initiatives is particularly challenging. Often these locations will already be experiencing weaker economic growth, which is precisely why they have been targeted by the policy.

The effects of these underlying factors ('selection effects') must be accounted for if we want to understand the extent to which the area based initiative actually increases growth.

Selection is likely to be a bigger problem for the economic area based initiatives we consider in this report, than for some of our previous reviews that considered other area-level interventions. For example, when reviewing the effects of sports and cultural projects or estate renewal, policymakers will often see economic factors as one consideration among many when making decisions on projects. However, for the area based initiatives considered here, economic factors are likely to be a core consideration. For this reason, treated areas are almost always likely to be different to untreated areas, and 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 area based interventions of the kind we consider here, it is hard to imagine situations in which true randomisation 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 the programme.

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 receive support) is compared with the change in outcome in a group of similar control areas (which do not). For example, Study 1264 examines the impact of French Enterprise Zones on the probability of exiting unemployment. The study estimates the change in the probability of unemployment exit for neighbourhoods in Greater Paris treated by the programme compared with control neighbourhoods matched on observed variables (e.g. average income). If EZ neighbourhoods were selected by policymakers based only on these observable characteristics then the matched neighbourhoods would be a suitable control group. The difference-in-difference would reveal the effect of the EZ because the treated group would be expected to follow a similar trend to the control group in the absence of the treatment. However, selection may also be based on unobservable neighbourhood characteristics. This implies that the treatment group is fundamentally different to the control group (even after matching on observables) and is likely to follow a different economic trend regardless of the policy. In this case the difference-in-difference method would be biased.

In order to allow for these unobservable factors, and thus more reliably assess the impact of area based initiatives it is important to exploit some source of randomness in the way that the support is targeted.

For example, paper 1314 uses variation in area-specific eligibility criteria to examine the effect of Regional Selective Assistance on economic outcomes in the UK. Since the policy targets underperforming firms and regions, a simple comparison of outcomes between treated and control groups is likely to be biased by selection effects. This study, therefore, makes use of area-level changes in eligibility criteria that are not thought to be related to individual firm performance and are therefore a source of randomness in the policy at the firm level. The papers uses these changes in an instrumental variables approach to estimate the causal effect of the policy.

A further example is study 1317, which examines the impact of the New Markets Tax Credit programme on private investment in low income US neighbourhoods. The programme works by

subsidising investment in neighbourhoods that fall below a certain threshold in median family income. The evaluation addresses selection effects by exploiting this threshold using a regression discontinuity design. Whilst treated neighbourhoods are different to non-treated neighbourhoods on average, those neighbourhoods either side of the threshold are likely to be very similar. Around the boundary the incidence of treatment is therefore relatively random. By comparing those neighbourhoods who only just received support (just inside) with those who only just missed out (just outside) the study estimates the causal effect of the programme.

If selection into area based programmes is based on unobservable as well as observable factors – as is likely – then these methods are potentially the only way to achieve reliable estimates of the impact on local economic growth outcomes. Future evaluations of area based programmes should pay close attention to techniques used in studies such as these, an issue to which we return in the below.



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 2,100 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 most our earlier reviews.

Following a further high level review, over 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). The remaining studies were grouped under the three sub-themes before undergoing a more detailed review. Below we outline the results of that review for Enterprise and Empowerment Zones, and for other ABIs. EU policies are covered in the companion report.

EZ policies

Quantity and quality of the evidence base

Of the 1,300 studies considered in more detail, 198 covered EZ policies. Of these, 168 were discounted on methodological grounds (i.e. scored 2 or below on the SMS scale). The remaining 30 studies have been included in this review.

This is a smaller evidence base than some of our reviews to date (on employment training, sports and culture projects, access to finance and estate renewal) but on a par with our review of business advice and more than for our review of broadband. As discussed above, this partly reflects the difficulties in evaluating area based initiatives 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: Ranking Studies by Quality of Implementation

SMS Score	Number of studies	Evaluation reference numbers
4	8	1225, 1235, 1255, 1258, 1262, 1267, 1290, 1313
3	22	1217, 1219, 1220, 1230, 1232, 1233, 1234, 1236, 1238, 1245, 1248, 1251, 1256, 1257, 1260, 1263, 1264, 1265, 1285, 1288, 1289, 1294
Total	30	

We found no studies that used randomised control trials: the lack of RCTs is not surprising given the nature of these projects. However, we found eight studies that used credible random sources of variation. The remaining 22 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 EU support. However, it is likely that unobservable characteristics may still be affecting the results.

Type and Focus of Support

In contrast to the EU policies covered in the previous section, Enterprise Zones cover smaller geographical areas and usually involve a much smaller range of interventions.

Paper 1217 provides a concise summary of how these programs may differ in a number of key dimensions:

- Type and magnitude of financial support offered to businesses;
- Eligibility of different businesses; and
- Criteria for selecting EZ areas.

The majority of studies included in this review look at one of two interventions, Enterprise Zones (EZ) and a US variant known as Empowerment Zones (EmpZ).

Although the concept of Enterprise Zones (EZs) originates in the UK, no UK based evaluations (of past or current policy) meet our minimum criteria.¹² The vast majority of evaluations covered in this review come from the USA, or from France, where the equivalent policy areas are termed ‘Zones Franches Urbaines’ or ‘Urban Free Zones’.

In the USA, EZs are administrated at the state level, having been implemented by over 40 states at various times since the 1980s. As a result, EZ policies encompass a variety of programmes which differ by state. Although details differ for state EZ programmes, they all share the common components of EZ policy, which include tax credits, credits for new jobs and credits for property taxes. 10 of our shortlisted papers look at US Enterprise Zones.

In France, the equivalent of US EZs are the Zones Franches Urbaines (ZFU), created by the French government in 1996 and designed to reduce urban inequality in French neighbourhoods. ZFUs are the smallest spatial unit of a three-tier system of intervention, comprising Zones Urbaine Sensible (ZUS) or “Sensitive Urban Zones”; Zones de Redynamisation Urbaine (ZRU) or “Urban Renewal Zones”, and the ZFUs. Seven papers cover these policies.

¹² We had 15 UK EZ papers in our original long list. 10 were rejected for being qualitative/ non-econometric / not impact evaluations; 2 were surveys; 1 was a review paper and 1 was a blog post. One UK EZ paper made the final shortlist, scoring only 2 on the SMS.

ZUS areas comprise the 757 most deprived areas in France (including overseas Départments), and cover a population of just under 5 million people. ZRUs comprise the 436 most deprived ZUSs based on unemployment rates, skills profile, proportion of young people and potential tax revenues. ZFUs in turn comprise the 100 most deprived ZRUs with a population over 10,000 people, and are selected using the same criteria as the ZRUs. Tax cuts are in place in all three tiers of intervention, however ZFUs provide the most generous incentives. In ZUS, local authorities are able to provide optional local tax exemptions to firms, whereas in ZRUs there are limited tax rebates over a short period of time for new firms. In contrast, in ZFUs, tax relief is granted for an initial period of five years, targeted at new and existing firms with fewer than 50 employees. This is followed by a gradual reduction in fiscal assistance over the following five years, meaning that small firms receive some form of tax break for at least a decade after setting up or locating in the zone.

In rural areas, the French government implemented the designation of Zones de Revitalisation Rurale (ZRR), essentially a rural EZ program, providing financial incentives to struggling rural communities based upon population density and socio-economic structural disadvantages.

In an attempt to boost local employment in these deprived areas, ZFUs also carry an additional requirement that businesses must employ one third of their employees from within the zone in order to reap the benefits of the policy. In this regard, they share characteristics with US Empowerment Zones, which we consider next.

Unlike most EZs, the US EmpZ programme is a national-level policy that ties business tax credits to the employment of local residents, and includes a series of large block grants and other kinds of support aimed at broad economic goals (such as reducing poverty and improving local infrastructure). After a round of bidding by local governments in 1994, six urban EmpZs (plus two Supplemental EmpZs) and three rural EmpZs were designated by the federal government, whilst 49 unsuccessful areas were also granted smaller Enterprise Community status, entitling them to a reduced series of benefits. Two further rounds of designation took place in 1998 and 2001, resulting in a further 23 EmpZs and 80 Enterprise Communities, plus a series of 28 urban and 12 rural areas being given Renewal Community status, entitling them to a similar magnitude of benefits as EmpZs.

The EmpZ programme is administered at federal level and involves a standardized package of fiscal benefits applied to neighbourhoods defined at a specific scale (1990 census tracts). As a result, unlike state level EZs which are characterised by a large degree of variability, the EmpZ programme has a largely singular set of benefits and incentives. These are summarised in paper 1220, and comprise the following:

- Employment Tax Credits;
- Social Services Block Grants;
- Loan Guarantees and Economic Development Initiative (EDI) Grants;
- Enterprise Zone Facility Bonds;
- Increased write-off thresholds for tangible business assets; and
- Regulatory waivers and priority in other federal programmes.

In terms of size, the average Round 1 EmpZ was 10.6 square miles, had a population of approximately 120,000 people and a poverty rate of 45%. Thirteen studies look at Empowerment Zones, two cover Enterprise Communities and two cover Renewal Communities.

Table 2 breaks down the shortlisted studies by intervention type.

Type of Intervention	Number of Studies	Reference
US Enterprise Zones	10	1217, 1219, 1225, 1232, 1235, 1255, 1256, 1265, 1288, 1294
ZFUrban (France)	6	1233, 1257, 1260, 1262, 1263, 1264
ZRRural (France)	1	1313
US Empowerment Zones	13	1220, 1225, 1230, 1236, 1238, 1245, 1248, 1251, 1258, 1267, 1285, 1289, 1290
US Enterprise Communities	2	1225, 1234,
US Renewal Communities	2	1248, 1251
Total	34*	

*Note: Three evaluations cover multiple programmes (study 1225 covers 3 programmes (EMPZ, EC, EZ) while study and 1248 and 1251 cover 2 programmes (1248: EMPZ, RC; 1251: EMPZ, RC))

Conditions of ABIs

Many of the evaluations considered look at the impacts of EZs (or other ABIs) upon local employment. In this context, it is useful to investigate whether any of the programmes offer initiatives to incentivise, or even require, the employment of local people.

Table 14 below highlights those interventions which are accompanied by conditions to employ local people. For the majority of the programmes, such conditions do exist – although these change depending on the intervention. For example, In US EmpZs, a wage credit is provided up to 20% of the first \$15,000 earned by each employee living and working in the zone, whereas for Renewal Communities there is a blanket wage credit provided of \$1,500 per employee living and working in the community. In French ZFUs, firms are offered exemption from payroll tax if 33% of their employees come from within the ZFU area. Enterprise Zones and Enterprise Communities, both in the US, are not reported to include any conditions for the employment of local workers. However in EZs, there is tax credit available for hiring disadvantaged employees, of 50% of qualified wages in the first year, falling by 10% each year until reaching zero after five years.

Table 3: Conditions of benefits

Type of ABI	Evaluations	Local Employment Condition	Other Conditions
Enterprise Zone (EZ) - USA	1217, 1219, 1225, 1232, 1235, 1255, 1256, 1265, 1288, 1294	No evidence of a local employment condition, but tax credit available for hiring disadvantaged employees, of 50% of qualified wages in the first year, falling by 10% each year until reaching zero after five years	Priorities vary depending on geography and may be some area specific conditions. Reported conditions include: Colorado – additional tax credits for businesses in manufacturing, mining and R&D; and \$1,000 tax credit for firms recruiting employees adding value to agricultural commodities through manufacturing and processing.
Empowerment Zone (EmpZ) - USA	1220, 1225, 1230, 1236, 1238, 1245, 1248, 1251, 1258, 1267, 1285, 1289, 1290	Wage credit provided of up to 20% of the first \$15,000 earned by each employee living and working in the EmpZ area [not referenced in all evaluations].	Priorities vary depending on geography and may be some area specific conditions. None reported in these evaluations.
Renewal Community (RC) - USA	1248, 1251	Wage credit provided of \$1,500 per employee living and working in the RC area.	No evidence of other conditions.
Enterprise Community (EC) - USA	1225, 1234	No evidence of a local employment condition.	No evidence of other conditions (not targeted at firms, more plan-making)
Zones Franches Urbaines (ZFU) - France	1233, 1257, 1260, 1262, 1263, 1264	Exemption from payroll tax if employ 33% of employees from within the ZFU area [initially 20% but increased to 33%]	ZFU benefits almost all targeted at businesses with >3 and <50 employees and turnover of <10 million euros.
Zones de Revitalisation Rurale (ZRR) - France	1313	Initially temporary (1996-2005) and then permanent (from 2005) exemption from payroll tax resulting in a 20% reduction in labour cost.	Initially targeted at small firms with <50 employees, then in 2005 extended to cover all employees in “Public Interest Organisations”. Target areas were designated based on population density and socio-economic factors.

Of those papers which do report a condition on the employment of local people, only one looks at the effect of this specifically (others look more generally at the programme effects as a whole). This paper, 1260, looks at the impact of changing the requirement for French ZFUs to employ local people from 20% to 33% in 2002. It finds that the effect on employment moves from insignificant prior to 2002, to significantly positive in 2002.

Very few initiatives include other conditions, such as targeting by sector or firm size. ZFUs and ZRRs do target specific types of firm; for ZRUs these are those with between 3 and 50 employees, and with a turnover of less than 10million euros per annum; for ZRRs these were initially small firms with fewer than 50 employees, and more latterly all firms designated as “Public Interest Organisations”. In theory, these were small firms that maintain a form of social life in rural areas; however in reality larger firms also benefitted, particularly in the health and education sectors.¹³ In addition, US EZs include various sectoral foci, often dependent on geography. For example, Paper 1255, on Colorado’s EZ programme, highlights a number of additional tax credits made available through the EZ specifically for manufacturing, mining, R&D and agricultural sectors.

None of the other papers identify other conditions imposed on intervention areas in order to receive benefits. Nevertheless, a number of the papers do consider the sectoral impacts of policies.

UK Enterprise Zones in context

As should be clear from this discussion, the conditions imposed on the ABIs for which evaluation evidence is available make these schemes somewhat different from the current incarnation of Enterprise Zones in the UK. More so than any of our other reviews to date, it is important to consider how the evidence presented here on two sets of schemes from two different countries can provide lessons for UK EZ policy.¹⁴

As we will see, differences in the extent of incentives offered by the scheme can affect the likelihood that evaluations detect positive employment effects within the zone. In terms of the type of support offered, the current UK scheme is closer to the French and US Enterprise Zones, rather than the more intensive support offered in US Empowerment Zones and Renewal Communities.

Other characteristics of schemes can also make a difference. In particular, scheme conditions on local employment make it more likely that increases in employment in the zone are accompanied by more jobs and decreased unemployment for residents living near to the zone. These local employment conditions do not apply to UK schemes, which makes it less likely that findings on unemployment will generalise.

This also affects the extent to which findings on displacement will generalise. Stronger scheme impacts within the zone are likely to increase the chance of displacement from areas close to the zone (after all, displacement will not occur if the zone has no direct employment effect). This means that evidence on displacement is more useful as an indicator of whether such effects are likely to occur for similar schemes (including those in the UK) rather than providing much precision on the scale of displacement. Local employment conditions also affect the dimensions on which displacement is likely to be observed. For example, in the absence of local employment conditions, displacement on employment is unlikely to translate in to displacement on unemployment (because households do not need to be resident within the zone to take a job there). In contrast, when local employment

13 Paper 1313 and Daniel et al 2009 – ‘Evaluation des Mesures en Faveur des Zones de Revitalisation Rurale’. [list in references]

14 In all of our other reviews we have had evidence on a broader set of schemes covering a broader set of countries.

conditions are imposed, displacement on employment may also translate in to displacement on unemployment as households within the zone benefit at the cost of residents outside the zone.

Finally, the selection mechanism for schemes may also play a role in the extent to which the findings that we report below apply. The US EZ and EmpZ programmes both explicitly targeted disadvantaged neighbourhoods, with the aim of using the tax breaks and other incentives to stimulate rejuvenation of these areas. In contrast, for the recent round of Enterprise Zones, the UK government has claimed to focus on zones that ‘make the most of economic assets’ which can involve targeting relatively advantaged areas (although we have no evidence of whether this applies in practice). This may mean that UK zone effects are stronger than those reported here with the planning and infrastructure components playing a more important role than the tax (i.e. business rate) incentives. In contrast, the US programmes’ attempts to counter market forces may mean that their impact is going to be smaller. On the other hand, the extent of displacement may be more of a worry for UK EZs given that they allow more businesses to locate in the relatively more attractive part of a city.

We begin by ignoring the issue of displacement and considering the extent to which policies have positive effects on outcomes for the zone.¹⁵ We then return to consider the extent to which these positive outcomes come at the extent of displacement from the surrounding area.

Employment impacts

Only three out of eight US EZ studies report positive effects on employment. A higher proportion of studies report positive employment effects for EmpZs and Renewal Communities, which offer more intensive support. The most consistently positive employment findings are for French ZFU.

Employment outcomes are considered in 23 evaluations, providing 27 estimates of employment effects (because some evaluations cover more than one programme). 15 out of these 27 find evidence of positive effects on employment, seven report mixed effects, and five report no effect.

Given the difference across programmes documented above, it is useful to breakdown the results by programme. Only three out of eight evaluations detect positive employment effects for US Enterprise Zones, with a further three each reporting mixed findings or no effects. The more intensive support provided through US Empowerment Zones and Renewal Communities appear to be more likely to generate positive employment effects: positive impacts are reported in five out of 10 Empowerment Zone evaluations (as well as four mixed, one zero) and both evaluations of Renewal Zones. The one evaluation that looks at the employment effects of US Enterprise Communities also reports positive effects. The most consistently positive effects are found for French Enterprise Zones with four out of five reporting positive effects (only the rural zones appear to have no employment effect).

Unemployment impacts

The studies provide nine estimates of unemployment effects. All but two of these suggests positive effects on unemployment.

¹⁵ As we discuss further in the section on displacement, some studies consider effects for an area larger than the zone itself. In these cases, findings provide the effect on the area as a whole, which may mask some displacement within the area between zone and non-zone neighbourhoods.

Unemployment is considered in seven evaluations, providing nine estimates of employment effects (because some evaluations cover more than one programme). Seven out of nine report positive effects on unemployment, with two reporting zero effects. This might suggest that effects on unemployment are more likely to be positive than for employment. When schemes impose local employment conditions, it is possible for unemployment in the zone to fall even if overall employment does not rise, if employers switch to favouring local residents (which is itself one of the reasons why these conditions are imposed). However, it is important to note that none of the studies reporting zero or mixed employment effects consider unemployment, whereas two of the studies that do consider unemployment also show positive employment effects. So the differences in proportions may simply reflect some selection in which studies evaluate unemployment effects.

Given the smaller number of studies, it's difficult to identify any differences across programmes: positive effects are reported in the only evaluation looking at US Enterprise Zones; three out of four Empowerment Zone studies and one out of two Enterprise Zone evaluations; and both studies that look at the unemployment effect of French Enterprise Zones.

Wage impacts

The studies provide fourteen estimates of the effect on wages. Half of these suggest positive effects on wages

Results for wages are broadly similar as to those for employment. Eleven studies consider the effects on wages, providing fourteen estimates of wage effects (because some evaluations cover more than one programme). Seven of these estimates are positive, with remainder finding zero effect.

Poverty impacts

The studies provide ten estimates of poverty effects. Half of these suggest positive effects on poverty.

Results for poverty are also broadly similar as to those for employment and wages. Poverty is considered in eight studies, providing ten estimates of poverty effects (because some evaluations cover more than one programme). Half of those ten report positive effects on poverty. Again, given the small number of studies, it is hard to identify differences across programmes. Seven of these estimates are for the more intensive support provided by Empowerment Zones, with three reporting positive effects.

Number of Establishments, number of new firms

Two out of four studies find positive effects of EZs on the overall number of businesses; this seems to be largely driven by relocation. One further study that looks just at new firms finds positive effects, again with relocation a more important factor than new firm births

We break down these results into impacts on the overall number of firms; the number of new firms, and the specific mechanisms behind these: firm birth, firm relocation (into or out of the Zone) and firm survival / exit.

Four studies look at the overall number of new firms in EZs: this might be driven by some combination of new firms setting up, relocation patterns, and firm exits. Two of these studies (1262 and 1263, both on French ZFUs) find positive effects on overall firm numbers. Within this, study 1262 reports positive EZ effects on firm creation and relocation, with the latter larger than the former. Similarly, study 1263 finds that changes in firm births drive overall firm counts for the first three years of the policy; after this relocations become more powerful as a driver of overall firm counts.

Consistent with these, study 1289 (on US Empowerment Zones) looks both inside and outside the EZ and finds mixed effects overall: firm numbers inside the Zone rise, but decrease in neighbouring areas. This likely reflects relocations from outside to inside the EZ, as in the previous two studies.

By contrast, Study 1235, on California's Enterprise Zone programme, finds that the EZ leads to a drop in overall firm numbers. The authors also find little EZ effect on employment; they interpret this as suggesting that firms in the zone are getting larger, or that larger firms are more attracted to EZ location than SMEs.

One further study (1257, on French ZFUs) looks just at new firms. It finds a positive overall effect of ZFU designation over two waves of the policy, which is driven more by relocations than by new firm births. Crucially, the first wave of the programme seems more effective than the second; in the latter case, the firm creation effect is only positive in the first year of the programme, then drops to zero.

Property Prices and Rents

Effects on residential property prices or rents are generally positive

As with many other area targeted policies, improvements in local amenities (broadly defined as area attributes that benefit households or firms) are likely to be at least partially capitalised in to increased land prices (for either residential or commercial use). As a result, looking at such increases in land prices provides one way of estimating the value to firms of the support being offered within the zone. It is even possible that land prices capture benefits to firms even in circumstances where we see no changes in other economic outcomes (e.g. employment or number of firms) if existing firms are willing to pay higher rents to remain in the improved location. Unfortunately, we found no evaluations that look at the effect on the commercial property market. This remains a major gap in the evidence base on the impact of these policies.

The effect on residential property prices is considered in four studies, with three reporting positive effects.¹⁶ Both studies that look at residential rents report positive effects.¹⁷ To the extent that the benefits that are getting priced in reflect the improved prospects for local businesses, then it is likely that these positive effects will be replicated for commercial properties. It is, however, possible that some of the scheme benefits (e.g. reduced vacancy rates and associated reductions in, say, vandalism) represent amenities for households, but not firms. We can't distinguish between these two possibilities in the absence of evaluation evidence on the impact of commercial rents.

¹⁶ Studies 1220, 1230 and 1290 report positive effects, while study 1234 reports negative effects.

¹⁷ Studies 1220 and 1285. Whether or not this is actually a positive outcome is open to debate.

Other Outcomes

The evidence on a range of other outcomes is mixed (with only one study per outcome).

A number of studies consider other outcomes, although for most of these other outcomes we have only one or two studies, which makes it hard to reach any general conclusions. We give a flavour of the findings here.

Study 1263 looks at various indicators of firm performance in French ZFUs between 2004 and 2007. It finds zero effects on income, cashflow, sales or wages of companies in Zones.

Study 1236 covers the US EmpZ programme in Baltimore, Chicago, Detroit and New York (four of the six Round 1 programmes). Besides the economic outcomes discussed above, it also looks at impacts on area-level vacancy rates, home ownership and residential mobility in 'treated' areas. It finds zero effects of the programme on all three outcomes.

Three studies look at changes in area education levels in US EmpZs. Study 1220, which covers all Empowerment Zones designated between 1994 and 2000 (all Round 1 and Round 2 Zones) finds that Zone designation led to a 2.3% higher share of Zone residents with college degrees. Study 1230, which looks only at the Round 1 Zone in Chicago, also finds higher average educational attainment in the Zone post-designation. Study 1220 checks whether displacement of existing residents accounts for these changes, suggesting that demographic trends only account for little of observed improvements, and up-skilling of existing residents is the main cause.

By contrast, study 1236, which looks at four Round 1 EmpZ cities - Baltimore, Chicago, Detroit and New York - finds zero effect of Zones on education levels. This study also looks at residents' occupational status, and finds zero programme effect here.

Two studies look at whether EZ-type programmes alter the share of residents on benefits (specifically, public assistance programmes in the US). Study 1234 explores Enterprise County designation in rural areas, finding that the share of residents on benefits goes up after scheme designation. It may be that in-migration explains this change, rather than the programme having directly harmful effects. By contrast, study 1238, which is an urban case study of the Chicago Empowerment Zone, finds zero effect of EmpZ designation on the share of local people claiming benefits.

Displacement

Displacement is not considered in all studies. But the majority of those that do, find evidence of displacement

When assessing the impact of EZ, EmpZ and other similar area based initiatives, a key consideration is whether the policy has generated new economic activity, or whether increased activity in the zone is the result of displacement from areas outside the designated zone. If displacement occurs, this will reduce the impact of such interventions at larger geographical scales. This may be of particular concern if displacement occurs in nearby areas as this will reduce the overall impact on the local area.

A number of studies directly address this issue. The most commonly used methodology is to look for effects for both the zone and at a larger spatial scale – e.g. for the municipality that includes the zone.

Alternatively, a smaller number of studies look directly for displacement by considering changes in non-eligible areas surrounding the zone.

Assessment of displacement is further complicated by the fact that some studies are only able to measure the outcomes on broader areas. When zones are very small data for the zone alone is often not available, meaning that the effects of the programme can only be estimated at the neighbourhood, ZIP code, or even municipal level. While this approach has the benefit of estimating the effect on the broader geography net of displacement, it does not allow assessment of displacement within the broader area from non-eligible to eligible areas. It also leaves open the question as to whether displacement to the broader area containing the zone comes at the expense of other neighbouring areas. Clearly, the use of a spatial unit of analysis close to the actual zone area combined with data for broader areas paints the clearest picture of the direct and displacement effect of policy.

Six of the seven studies that consider the French EZs provide evidence on displacement. Four out of five studies that report positive effects also find some evidence of displacement.¹⁸

Papers 1257 and 1262 find the strongest evidence of displacement with results suggesting that the majority of business growth created by the French EZ programmes was due to pre-existing firms re-locating to the Zone to the detriment of the wider area.¹⁹ Results from study 1263 similarly show strong displacement effects on business creation reporting significant negative spillover effects in neighbouring areas during the first three years of the programme in terms of number of establishments and creation rates. Negative spillovers were, on average, found to be of the same magnitude as the positive impacts within EZs themselves, meaning that the positive impact on treated areas is likely to be totally offset by negative effects on the immediate vicinity. Somewhat puzzlingly, no negative spillover effect was observed for employment outcomes.

Paper 1260 uses two measures to examine for displacement first comparing results for the zone and for the wider municipality, second comparing unemployment outcomes for residents inside and outside the zone. Results when changing the unit of analysis suggest positive effects across both geographical areas in terms of unemployment but with far smaller effects at the municipal level consistent with some displacement. In contrast, the results comparing unemployment outcomes suggest no significant difference between zone and non-zone residents and thus no obvious displacement effect.

In contrast, study 1264 finds positive effects on unemployment, but no evidence of displacement. To consider displacement it compares French municipalities with an EZ (ZFU) to neighbouring ones without, and finds positive effects within the treated municipality but no significant spillover effect into neighbouring municipalities in terms of exit rate from unemployment.²⁰

Study 1313 on rural French EZs looks to see whether effects are larger in treated cantons that are surrounded by non-treated ones, or those that are close to non-eligible firms. It continues to find that these zones have no effect on outcomes, consistent with the overall finding of no effects. The absence of any direct effect means that this is not very informative about displacement, although it does suggest that for these rural EZs characteristics which might increase displacement still are not sufficient to generate a positive effect within the zone.

18 Studies 1257, 1260, 1262, 1263 find positive effects and evidence of displacement; study 1264 finds positive effects and no displacement, study 1313 finds no positive effects and no displacement

19 Study 1257 uses small areas which do not exactly match up with EZ boundaries and defines an area as 'treated' if more than 50% of its area is within the EZ.

20 Because municipalities are generally larger than zones this study is unable to assess displacement within treated municipalities.

Six of the studies that consider US schemes provide direct evidence on displacement. Two of the four studies that report positive effects also find some evidence of displacement.²¹

Study 1289 finds negative spillovers to areas bordering EmpZs in terms of the number of business establishments, which would suggest that displacement has taken place. Employment specifications (without controls) report negative spillovers but results are insignificant once controls are included.

Study 1285 also suggests that EmpZ policy has resulted in displacement – but this time in terms of households rather than firms. Results suggest that pre-existing high-income households re-locate to the area, whilst there are no benefits to existing poorer residents. This means that whilst the effect within EmpZs is positive for average household income, it is likely to come about through the displacement of poorer existing residents. In contrast, study 1220 argues that its results indicate that federal EmpZs do not result in gentrification.

Study 1225 estimates the treatment effect on the nearest non-enterprise zone area; non-empowerment zone area and non-enterprise community area.²² It looks at unemployment, poverty rate, wages and employment and finds no evidence of displacement effects (estimates are positive but insignificant).

One study that finds no evidence of effects (1255) also considers displacement finding no evidence that it occurs (something which seems unsurprising given the lack of positive zone effect).

Three further US studies provide indirect evidence on displacement because they are only able to consider treatment effects for areas that are larger than the underlying zones. Study 1217 and 1256 look at the effect of EZs on municipalities containing the zone (which will be larger than the zone) finding no effect on employment. Similarly, study 1265 finds no effect when looking at ZIP codes that have some overlap with EZs. This could either be because there is no zone effect, or because displacement from non-eligible parts of the municipality completely offset positive effects within the zone.

Heterogeneity of effects

Very few of the papers report any detail on the heterogeneity of effects within or between EZs and EmpZs. Only two papers really look at this in any detail: papers 1233 and 1257. Paper 1233 looks at the impact of French Enterprise Zones (ZFU) on number of firms in the area. It finds that the impacts of policy are highly heterogeneous, and that the effect is stronger in those areas which were initially less depressed. In addition, positive impacts on firm numbers are greater for sectors where relocation costs are less, and particularly strong for both health and business services. Paper 1257 investigates how transport and market access influence the impact of ZFUs on the number of establishments. It finds that an increase of two train stations within 500m of the ZFU leads to an increase of 5.8% in business counts. By contrast, a one standard deviation in the amount of impassable roads within a ZFU leads to a 6.3% decrease in the number of establishments.

It should be noted that for the majority of EZs there are strict criteria for designation. As a result many of the ABI designated areas share similar characteristics, making heterogeneity less likely. Many of the papers consider the difference between those areas designated as EZs or similar, and those that were not designated, rather than comparing EZ areas with each other. Papers 1217 and 1238 do consider the different factors that might effect EZ designation, finding that areas that have experienced

21 Studies 1285 and 1289 find positive effects and evidence of displacement; studies 1220 and 1225 find positive effects and no displacement, studies 1265 and 1285 find weak zone effects and no displacement.

22 This study uses data from (aggregations of) US census tracts which don't exactly align with EZ areas but give an extremely close match.

economic hardship are more likely to become EZs than those which are economically successful.²³ However, neither paper provides much detail on the differences between EZs.

Paper 1232 looks in broader terms at the effects of EZ in urban and rural US communities. While there is no effect of EZ designation on employment per establishment in urban areas, the paper finds some (small) effects in rural areas. However, this positive effect is only felt in firms employing less than three people. On the whole the paper finds that there is little difference between rural and urban impacts. Paper 1313 only reports on the effects of the French rural EZ Programme (ZRR), finding no effect of the policy on either employment or wages.

Three other papers (1225, 1245, and 1248) compare the impacts of EZs across different cities and states. However, none of these papers analyse the meaning of these different effects, or undertake causal analysis in relation to economic or other characteristics of these different places.

Two studies look at other aspects of heterogeneity. Study 1267 looks at the changing share of industry types in response to EmpZ policy. A change in the share of industry types must inherently come about due to the displacement of certain businesses in favour of others. The paper finds that gains in the share of retail and service sectors (0.16-0.30 percentage points) were offset by losses of a similar magnitude (0.16-0.19 percentage points) in the transportation and finance, insurance, and real estate industries.

Study 1219 looks at heterogeneity at the individual level. It finds that for those people within an EZ area (in the US), those with a very low initial income felt stronger positive effects for wages, than those who had a somewhat higher initial income.

Paper 1257, for example, finds that wages in the medical sector increase by 15% as a result of the ZFU, but in the manufacturing sector, there is no effect. Similarly, paper 1267 reports that Employment Zone designation affected firm location decisions differently in different sectors, with positive effects in manufacturing, negative effects in transport and no effect in retail.

Other issues

Many of the studies contain additional information on scheme specific aspects of impact. It is not clear the extent to which these would generalize. Perhaps of most interest from this set of findings are those from study 1288 which examines the effect of programme characteristics on employment outcomes, reporting two headline findings: first, programmes with fewer target areas are more effective in attracting new jobs and business activity; second, employment growth in existing target businesses is promoted only if programme incentives are tied to hiring requirements. Given the decision to expand the UK EZ programme, and the lack of any restrictions on hiring, it would be interesting to know if these effects generalize (something which will require careful evaluation of the current UK programme).

²³ An alternative perspective comes from Beekmans et al. (2015) which looks at the factors behind ABI designation in the Netherlands. Focusing on two regeneration programmes, it finds little difference in economic outcomes (such as employment growth, firm counts and property values) between targeted and non-targeted sites. The authors suggest that “other criteria, such as political and strategic decision-making influence policymakers’ decisions to target industrial sites for regeneration”.

Other ABIs

Quantity and quality of the evidence base

Of the 1,300 studies considered in more detail, 163 covered other ABIs. Of these, 151 were discounted on methodological grounds (i.e. scored 2 or below on the SMS scale). The remaining 10 studies have been included in this review.

The papers presented in this section fall into two broad categories; approximately half examine either UK based Regional Selective Assistance or comparable policy from elsewhere; whilst the remaining papers consider programmes offering an assortment of incentives.

Table 4 shows the distribution of the studies ranked by SMS score.

Table 4: Ranking Studies by Quality of Implementation

SMS Score	Number of studies	Evaluation reference numbers
4	4	1286, 1314, 1315, 1317
3	6	1254, 1292, 1294, 1298, 1301, 1306
Total	10	

Types and Focus of Support

Table 5 breaks down the shortlisted studies by intervention type.

Table 5: Type of Intervention

Type of Intervention	Number of Studies	Reference
Area based Tax Credit Schemes	3	1286, ²⁴ 1294, ²⁵ 1317 ²⁶
Regional Selective Assistance (RSA) – UK	4	1298, 1301, 1306, 1314
Regional Investment Grants (RIG) – Sweden	1	1292
Area based Investment & Grant Scheme	1	1254, ²⁷
Local Enterprise Growth Initiative	1	1315 ²⁸
Total	10	

Three studies consider Area Based Tax Credits (all for the US). Six studies consider Regional Investment Grants. Four of these consider the impact of the UK's Regional Selective Assistance (RSA) which was first introduced in the UK in 1972, and provided discretionary grants to firms in disadvantaged Assisted Areas. These areas were designated based upon levels of GDP per capita and unemployment according to EU rules with the degree of assistance depending on the extent of local deprivation. Rules for area eligibility and the extent of support have changed over time, but support has always involved assistance to firms to undertake capital expenditure on property, plant or

24 Evaluates the US New Markets Tax Credit (NMTC) Program

25 Evaluates the Georgia (US State) Job Tax Credit Scheme

26 Evaluates the US New Markets Tax Credit (NMTC) Program

27 Evaluates the Italian Patti Territoriali (Territorial Pacts)

28 Evaluates the UK Local Enterprise Growth Initiative (LEGI)

machinery, to create or safeguard employment. The vast majority of RSA funds (more than 90%) are allocated to businesses in the manufacturing sector.

Swedish Regional Investment Grants are broadly similar, although there are some differences in the level of support offered.

Both schemes are selective, with support going to firms that can demonstrate they serve non-local markets or face foreign competition. In order to combat the possible effects of local displacement, firms are not allocated funds if it is believed that local competition would be undermined. In short, these schemes differ from Enterprise Zones in both the type of support offered (investment grants) and the selective nature of support.

The sixth study (paper 1254) considers the Italian Territorial Pacts established in 1997 and offering a mix of public infrastructure investment and investment grants to private sector firms to assisted areas (again chosen in line with EU criteria). The public infrastructure component makes it different to the UK and Swedish schemes (as does the more generous threshold for extent of assistance) although the focus on grants for investment in relatively productive plants does mean that there is a degree of selection

Finally, paper 1315 evaluates the early impacts of the UK's Local Enterprise Growth Initiative (LEGI), which ran from 2005 to 2010 (the evaluation tests impacts through 2012). . LEGI involved funding for local authorities, designed to increase productivity and enterprise in deprived areas. Funding was provided through a competitive bidding process, and successful local authorities determined how best to allocate the funds to support the growth and reduce the failure rate of locally owned businesses.

As a result, the allocation of LEGI funds differed quite significantly by area. Across the programme as a whole about 30% of expenditure went on supporting existing local businesses, with projects supporting new business start-ups receiving a similar share. Support to residents, accounted for about 20% of expenditure, while about 10% was spent on area improvements or promotion. Management and administration accounted for the remaining 10% of expenditure. In total, yearly expenditure on the programme was approximately £80 million - £100 million.

Given the heterogeneity in these schemes, we focus only on the effects on employment and unemployment and on the evidence on displacement. We briefly consider other outcomes in an annex. As with Enterprise Zones, we begin by ignoring the issue of displacement and considering the extent to which policies have positive effects on outcomes for the zone.²⁹ We then return to consider the extent to which these positive outcomes come at the extent of displacement from the surrounding area.

Employment

Employment effects are positive in a little over half of the studies that consider effect on employment

Seven of the 10 studies consider the effect on employment. Of these, four find positive effects, while three find no effect. In terms of the overall ratio of positive to no effect, this is broadly along the lines of the proportions found for the EZ-type programmes considered in the previous section. Given the

²⁹ As before, some studies consider effects for an area larger than the zone itself. In these cases, findings provide the effect on the area as a whole, which may mask some displacement within the area between zone and non-zone neighbourhoods.

wide variety of ways of reporting both effects and costs, it is not possible to go any further in terms of comparing the effectiveness of these different types of programme.

Unemployment

Unemployment effects are considered in three studies, with all three reporting positive effects

As with EZ-type programmes, results for unemployment are more likely to be positive than results for employment. Note, however, that all three studies that look at unemployment also report positive employment effects. So, as with EZ-type programmes this difference in the proportion reporting positive effects may simply reflect the underlying selection of programmes evaluated.

Displacement

As with the EZ-type programmes, positive effects in the area supported may come about because of displacement from elsewhere. Again, this may be a particular concern if displacement is from other nearby areas as this may mean little overall employment growth at local level.

Only two studies consider displacement directly, by looking at the effect of the policy in nearby areas. Unfortunately, one of these, Study 1286 (which looks at New Market Tax Credits) reports no positive effect for the area itself, so the finding of no displacement is not particularly informative. In contrast, study 1315 (UK LEGI) reports strong displacement effects at the treatment area boundary, with all positive effects within the zone the result of displacement from near the zone resulting in zero effect at the aggregate level. The authors suggest that this particularly strong displacement effect reflects the fact that the scheme mainly supported non-tradeable services where we would expect displacement to be large.

Study 1314 looks at the effect of RSA (a programme that involves selection partly on the basis of the extent to which a firm serves non-local markets) finds positive effects for both targeted areas and at the wider area level. As with the EZ-type schemes, this indirect approach to displacement does not allow a full consideration of displacement toward the zone, but does tell us that both targeted area and wider area results are overall positive.

Finally, study 1294 also reports positive effects of New Market Tax Credits on wider area employment although it does not provide separate estimates for the targeted and wider areas.

Lessons for EZs

The evaluation of other ABIs face many of the same challenges as for EZ-type schemes. Some of the approaches adopted to provide high quality evaluations of other ABIs (e.g. using allocation rules as in study 1314) might help improve the evaluation of EZ-type schemes.

Given the small number of studies, it is hard to draw any general conclusions in terms of policy effectiveness that might extend to EZ-type schemes. It is intriguing that a couple of UK studies suggest that the extent of displacement is much greater when schemes are non-selective and target non-traded services (LEGI) than when they are selective and target firms that serve non-local markets (RSA). This is certainly in line with a large body of theoretical and wider empirical literature and taken together this wider literature and the evaluation evidence urge some caution in the area specific targeting of firms that provide non-traded goods and services.



Summary of findings: EZs

What the evidence shows

- 15 out of 27 report positive effects on employment, 7 report mixed effects, and 5 report no effect. Evidence of employment effects is weakest for US Enterprise Zones (3/8 studies) and better for US Empowerment Zones (5/10 studies).
- Seven out of nine report positive effects on unemployment with two reporting mixed effects.
- Half of the ten studies that consider the impact on poverty report positive effects.
- Half of the 12 evaluations report positive effects for wages.
- Four out of six report positive effects on the number of businesses.

Where there is a lack of evidence

- A number of studies suggest that positive effects for zones may be driven by displacement from nearby areas. However, we have little evidence on whether overall effects at the wider area level are positive, or whether displacement is the main effect of EZ-type schemes.
- Very few studies look at differences in programme characteristics. As a result, we do not know how characteristics of programmes (e.g. selection, local employment conditions) change effectiveness, including the extent to which they may reduce displacement.
- One study suggests that programmes with fewer target areas are more effective in attracting new jobs and business activity; and that employment growth in existing target businesses is promoted only if programme incentives are tied to hiring requirements. It would be good to know if these findings generalize.

Lessons

- Decision makers need to take concerns over displacement seriously. If much of the growth within the zone comes at the expense of nearby local areas, then this will mean less (or even no) overall growth at the wider area level.

- Even if displacement effects are strong, EZs may play a role in helping concentrating local employment from a number of dispersed sites.

Summary of findings: other ABIs

What the evidence shows

- Four out of seven studies report positive effects on employment
- Only three studies look at unemployment, with all three reporting positive effects.

Where there is a lack of evidence

- We have very few estimates for other outcomes of interest
- There is some evidence that targeting of schemes to firms that provide traded goods and services could make a difference in determining the extent of displacement, but this finding is based on only two evaluations of different schemes.

How to use these reviews

In the UK, the devolution of business rates offers an opportunity for local authorities to make their own decisions about using EZ-type programmes to address local economic conditions. However, the relative power of this incentive (which represent a small proportion of business operating costs) should not be overestimated.

It is also clear that decision makers need to take concerns over displacement seriously. If much of the growth within the zone comes at the expense of nearby local areas (e.g. within the same LEP or city-region), then this will mean less (or even no) overall growth at the wider area level. Even then, EZs and other ABIs may play a role in helping concentrating local employment from a number of dispersed sites. There has been some suggestion that this may lead to additional productivity effects via agglomeration benefits. Given existing estimates of the effect of increased concentration on productivity, and the extent of clustering involved, these effects are unlikely to be large in practice. Of more importance, are the implications for public service provision of more concentrated employment. For example, concentrating employment on a smaller number of sites, may help reduce costs of infrastructure provision such as transport, broadband and other services to business.

Given the small number of studies for the 'other' ABI category, it is hard to draw any general conclusions in terms of policy effectiveness that might extend to EZ-type schemes. It is intriguing that a couple of UK studies suggest that the extent of displacement is much greater when schemes are non-selective and target non-traded services (LEGI) than when they are selective and target firms that serve non-local markets (RSA). This is certainly in line with a large body of theoretical and wider empirical literature, and taken together, this wider literature and the evaluation evidence urge some caution in the area specific targeting of firms that provide non-traded goods and services.

This review of ABIs reinforces two other overarching issues for policymakers in the UK:

- Objectives of any area based policy must be very clearly defined, and the more specifically they can be targeted in terms of outcomes the better. The likely impacts of incentives across the targeted area, on adjacent areas, and over time, must be considered in the light of local conditions and objectives.

- We must make progress in the evaluation of the UK-style EZs if we are to say with confidence that they are providing good value for money.

Helping to fill the evidence gaps: improving evaluation and appraisal

As should be clear from this review, there are many things that we do not know about the impact of Enterprise Zones and other Area Based Initiatives.

Evaluations of future programmes of this type in the UK could be carried out much more successfully if evaluation is built into the policy design. Firstly, for schemes involving multiple types of interventions, there is potential for policy design to help with selection bias. For example, a degree of randomisation could be considered for programmes that are oversubscribed. Further, if randomisation is not acceptable, a cut-off point in eligibility criteria could be introduced in order to help the implementation of regression discontinuity approaches. The eligibility criteria should be clearly defined and strictly implemented. Our other reviews on specific areas of expenditure provide plenty of further discussion about improving the evaluation of specific types of expenditure

Secondly, more specifically for the EZ-type schemes, we need to undertake evaluation of current support that uses control areas for comparison (to parallel the higher quality evidence from the US and France or for other ABIs discussed above). Approaches for doing this are already well developed and could easily be implemented in the UK using secondary data sources. We also need to do more work on developing and implementing methods to understand the extent of displacement, especially from nearby local areas.

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.



References

- Beekmans J, Ploegmakers H, Martens K, and van der Krabben E. (2015) Countering decline of industrial sites: Do local economic development policies target the neediest places? *Urban Studies* online first, doi:10.1177/004209801560328.
- Busso M, Gregory J and Kline P. (2013) Assessing the Incidence and Efficiency of a Prominent Place Based Policy. *American Economic Review* 103: 897-947.
- Cheshire P, Nathan M and Overman H. (2014) *Urban Economics and Urban Policy*, Cheltenham: Edward Elgar.
- Gibbons S, Nathan M and Overman H. (2014) *Evaluating spatial policies*. SERC Policy Paper SERCPP12. London: LSE.
- Glaeser E and Gottlieb JD. (2008) *The Economics of Place-Making Policies*. *Brookings Papers on Economic Activity* 2008: 155-239.
- Kline P and Moretti E. (2013) *Place Based Policies with Unemployment*. *The American Economic Review* 103: 238-243.
- Larkin K and Wilcox Z. (2011) *What would Maggie do?* London: Centre for Cities.
- Lawless P, Overman HG and Tyler P. (2011) *Strategies for Underperforming Places*. *Spatial Economics Research Centre Policy Paper SERCPP006*. London: SERC.
- Neumark D and Simpson H. (2014) *Place-Based Policies*. NBER Working Paper 20049. Cambridge, MA: NBER.
- Sherman LW, Gottfredson DC, MacKenzie DL, et al. (1998) *Preventing Crime: What Works, What Doesn't, What's Promising*. National Institute of Justice Research Brief, . Washington DC: US Department of Justice.

Appendix A: Findings for other outcomes for other ABIs

A number of studies look at other aspects of the 'other ABI group'. We give a flavour of the findings here. Where possible, we focus on programmes where a number of studies exist, shedding light on the aggregate programme impacts.

Several papers look at various aspects of the Regional Selective Assistance scheme besides those reported in the main body of the review. Studies 1301 and 1314 look at the effects of RSA on recipient firms' total factor productivity; both find negative effects. By contrast, on other outcomes the policy's effect is positive: on investment by receiving firms (study 1314), and more broadly on firm location patterns (study 1298) and FDI (study 1306, which also covers the Merit R&D programme for SMEs). These 'input' measures are arguably less important than outcome measures like productivity or employment.

Two studies look at various area-level impacts of the US New Markets Tax Credit programme, which provides tax incentives to businesses, in order to promote business investment in low-income neighbourhoods. Study 1286 finds that average worker commuting distance rose in treated areas: specifically, the authors find a 0.5% drop in the number of workers living in treated census tracts, which implies more commuting in. Consistent with this, the study also finds changes in job composition in treated areas (more medium and high wage jobs), and a decline in the share of employment taken by residents, which appears to be driven by a drop in the share of low paid jobs. Study 1317 finds rising household turnover and a falling poverty rate, which might imply some resident displacement (especially given the other study result on resident employment). However, this evaluation also finds zero effect on house prices.

Table A1: Findings by outcome: EZ

Outcome	Total evaluated	Positive	Zero	Negative	Mixed
Labour market					
Employment	23	1220, 1225, 1251, 1257, 1258, 1262, 1263, 1264, 1288, 1289, 1294	1217, 1235, 1256, 1290, 1313		1230, 1232, 1245, 1248, 1255, 1265, 1267
Unemployment	7	1220, 1225, 1238, 1260, 1264	1234, 1236		
Wages	11	1225, 1238, 1258, 1285	1219, 1220, 1232, 1234, 1236, 1313		1251
Poverty rate	8	1220, 1225, 1238	1234, 1285, 1290	1230	1236
Firm performance					
Number of Firms (birth + survival + relocation)	4	1262, 1263		1235	1289
New Firms (birth + relocation)	2	1257, 1262			
Of which firm birth	3	1257, 1262, 1263			
Firm Relocation	4	1257, 1262, 1263	1263		
Firm Survival	1	1263			
Firm income/sales	1		1263		
Business location	2				1233, 1267
Shipment	2	1288			1265
Capital Investment	1	1288 ³⁰			
Housing / property					
House prices	4	1220, 1230, 1290		1234	
Vacancy Rate	1		1236		
Home Ownership	1		1236		
Residential Mobility	1		1236		
Rent	2	1220, 1285			
Other area outcomes					
Educational attainment	3	1220	1236	1230	
Residents on public assistance	2		1238	1234	
Population	1			1230	
Occupational Status	1		1236		

30 Paper 1288 also examines the effect of programme characteristics on investment and productivity (shipment) outcomes, reporting that programmes with strategic planning requirements are more effective in promoting production and investment growth in existing target businesses

Table A2: Findings by outcome: other ABI

Outcome	Total evaluated	Positive	Zero	Negative	Mixed
Main outcomes					
Employment	7	1294, 1314, 1315, 1317	1254, 1286, 1292		
Unemployment	3	1314, 1315, 1317			
Wages	1		1317		
Number of Establishments	1	1315			
Net Entry	1	1314			
Number of Plants	1		1254		
Other firm outcomes					
Productivity	2		1301, 1314		
Firm Location	1	1298			
Returns on Equity	1		1292		
Investment	1	1314			
FDI Location	1	1306			
Other outcomes					
House Prices	1		1317		
Commercial Property Values	1		1310		
Commuting Distance	1			1286 ³¹	
Poverty Rate	1	1317			
Household Turnover ³²	1				1317 ³³

31 The paper finds that the policy has the effect of increasing commuting distances. This has been interpreted as representing a negative outcome, but this interpretation is open to discussion.

32 Household Turnover = Households changing hands, not household income.

33 Although the coefficients are positive, in reality, this result indicates that any improvements in neighbourhood characteristics are not driven by improvements in the conditions of existing residents. There is therefore an argument that this might be a negative outcome in reality.

Appendix B: Evidence Reviewed

Ref	Reference
1217	Bondonio, D and Engberg, J (2000). Enterprise Zones and Local Employment: Evidence from the States' Programs. <i>Regional Science and Urban Economics</i> . 30(5), 519-549.
1219	Bostic, RW and Prohofsky, AC (2006). Enterprise Zones and individual Welfare: A Case Study of California. <i>Journal of Regional Science</i> . 46(2), 175-203.
1220	Busso, M and Kline, P (2008). Do local Development Programs Work? Evidence from the Federal Empowerment Zone Program. Cowles Foundation Discussion Paper No. 1638 Yale Economics Department Working Paper 36.
1225	Ham, JC; Swenson, C; Imrohorglu, A and Song, H (2011). Government Programs can Improve Local Labor markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community. <i>Journal of Public Economics</i> . 95(7-8), 779-797.
1230	Krupka, DJ and Noonan, DS (2009). Neighborhood Dynamics and the Housing Price Effects of Spatially Targeted Economic Development Policy. IZA Discussion Paper 4308.
1232	Lynch, D and Zax, JS (2011). Incidence and Substitution in Enterprise Zone Programs: The Case of Colorado. <i>Public Finance Review</i> . 39(2), 226-255.
1233	Mayer, T; Mayneris, F and Py, L (2013). The Impact of Urban Enterprise Zones on Establishment Location Decisions: Evidence from French ZFUs. Banque de France Working Paper 458.
1234	Montgomery, N (2010). The Effect of Enterprise Community Designation for Rural Areas. University of Michigan PhD Thesis.
1235	Neumark, D and Kolko, J (2010). Do Enterprise Zones Create Jobs? Evidence from California's Enterprise Zone Program. <i>Journal of Urban Economics</i> . 68, 1-19
1236	Oakley, D and Tsao, HS (2006). A New Way of Revitalizing Distressed Urban Communities? Assessing the Impact of the Federal Empowerment Zone Program. <i>Journal of Urban Affairs</i> . 28(5), 443-471.
1238	Oakley, D and Tsao, HS (2007). Socioeconomic Gains and Spillover Effects of Geographically Targeted Initiatives to Combat Economic Distress: An Examination of Chicago's Empowerment Zone. <i>Cities</i> . 24(1), 43-59.
1245	Rich, MJ and Stoker, RP (2010). Rethinking Empowerment: Evidence from Local Empowerment Zone Programs. <i>Urban Affairs Review</i> . 45(6), 775-796.
1248	Smith, R (2015). Empowerment for Whom? The Impact of Community Renewal Tax Incentives on Jobs and Businesses. <i>Urban Studies</i> . 52(4), 702-720.
1251	Tong, P; Zhou, L (2014). The Impact of Place-Based Employment Tax Credits on Local Labor: Evidence from Tax Data. University of Alberta Working Paper 2014-06.
1254	Accetturo, A and De Blasio, G (2012). Policies for Local Development: An Evaluation of Italy's "Patti Territoriali". <i>Regional Science and Urban Economics</i> . 42(1-2), 15-26.
1255	Billings, S (2007). Do Enterprise Zones Work? An Analysis at the Borders. University of Colorado - Center for Economic Analysis Working Paper 07-09.
1256	Boarnet, MG and Bogart, WT (1996). Enterprise Zones and Employment: Evidence from New Jersey. <i>Journal of Urban Economics</i> . 40(2), 198-215.
1257	Briant, A; Lafourcade, M; Schmutz, B (2013). Can Tax Breaks Beat Geography? Lessons from the French Enterprise Zone Experience. <i>American Economic Journal: Economic Policy</i> . 7(2), 88-124.
1258	Busso, M; Gregory, J and Kline, PM (2013). Assessing the Incidence and Efficiency of a Prominent Place Based Policy. <i>American Economic Review</i> . 103(2), 897-947.
1260	Charnoz, P (2015). Do Place-Based Policies Help People? Evidence from French Enterprise Zones. Crest Working Paper 2014-44.

1262	Givord, P; Quantin, S and Trevien, C (2012). A Long-Term Evaluation of the First Generation of the French Enterprise Zones. Direction des Études et Synthèses Économiques Working Paper 2012/01.
1263	Givord, P; Rathelot, R and Sillard, P (2013). Place-based Tax Exemptions and Displacement Effects: An Evaluation of the 'Zones Franches Urbaines' Program. Regional Science and Urban Economics.
1264	Gobillon, L; Magnac, T and Selod, H (2012). Do Unemployed Workers Benefit from Enterprise Zones? The French Experience. IZA Discussion Paper 6357.
1265	Greenbaum, RT and Engberg, JB (2004). The Impact of State Enterprise Zones on Urban Manufacturing Establishments. Journal of Policy Analysis and Management. 23(2), 315-339.
1267	Hanson, A and Rohlin, S (2011). The Effect of Location-Based Tax Incentives on Establishment Location and Employment Across Industry Sectors. Public Finance Review. 39(2), 195-225.
1285	Lockwood-Reynolds, C. and Rohlin, S. (2015) The Effects of Location-Based Tax Policies on the Distribution of Household Income: Evidence from the Federal Empowerment Zone Programme. Journal of Economics. 88, 1-15.
1286	Freedman, M (2015). Place-based Programs and the Geographic Dispersion of Employment. Regional Science and Urban Economics. 53, 1-19.
1288	Bondonio, D (2002). Evaluating Decentralized Policies A Method to Compare the Performance of Economic Development Programmes Across Different Regions or States. Evaluation. 8(1), 101-124.
1289	Hanson, A and Rohlin, S (2013). Do Spatially Targeted Regeneration Programs Spillover? Regional Science and Urban Economics. 43, 86-100.
1290	Hanson, A (2009). Local Employment, Poverty, and Property Value Effects of Geographically Targeted Tax Incentives: An Instrumental Variables Approach. Regional Science and Urban Economics. 39(6), 721-731.
1292	Ankarhem, M; Daunfeldt, SO; Quoreshi, S and Rudholm, N (2010). Do Regional Investment Grants Improve Firm Performance? Evidence from Sweden. Technology and Investment. 1(3), 221-227.
1294	Bollinger, CR; Ihlanfeldt, KR (2003). The Intraurban Spatial Distribution of Employment: which Government Interventions Make a Difference? Journal of Urban Economics. 53(3), 396-412.
1298	Devereux, MP; Griffith, R and Simpson, H (2007). Firm Location Decisions, Regional Grants and Agglomeration Externalities. Journal of Public Economics. 91, 413-435.
1301	Harris, R and Robinson, C (2004). Industrial Policy in Great Britain and its Effect on Total Factor Productivity in Manufacturing Plants, 1990–1998. Scottish Journal of Political Economy. 51(4), 528-543.
1306	Jones, J; and Wren, C (2011). Assessing the Regional Impact of Grants on FDI Location: Evidence from UK Regional Policy, 1985-05. Journal of Regional Science. 51(3), 497-517.
1313	Behaghel, L; Lorenceau, A; Quantin, S (2015). Replacing Churches and Mason Lodges? Tax Exemptions and Rural Development. Journal of Public Economics. 125, 1-15.
1314	Criscuolo, C; Martin, R; Overman, H and Van Reenen, J (2012). The Causal Effects of An Industrial Policy. CEP Discussion Paper 1113.
1315	Einio, E and Overman, HG (2012). The Effects of Spatially Targeted Enterprise Initiatives: Evidence from UK LEGI. Working Paper.
1317	Freedman, M (2012). Teaching New Markets Old Tricks: The Effects of Subsidized Investment on Low-income Neighborhoods. Journal of Public Economics. 96(11-12), 1000-1014.

Find the full list of search terms we used to search for evaluations on our website here:

www.whatworksgrowth.org/policies/area-based-initiatives/search-terms.

The What Works Centre for Local Economic Growth is a collaboration between the London School of Economics and Political Science (LSE), Centre for Cities and Arup.

www.whatworksgrowth.org



THE LONDON SCHOOL
OF ECONOMICS AND
POLITICAL SCIENCE ■



ARUP

This work is published by the What Works Centre for Local Economic Growth, which is funded by a grant from the Economic and Social Research Council, the Department for Business, Innovation and Skills and the Department of Communities and Local Government. The support of the Funders is acknowledged. The views expressed are those of the Centre and do not represent the views of the Funders.

Every effort has been made to ensure the accuracy of the report, but no legal responsibility is accepted for any errors omissions or misleading statements.

The report includes reference to research and publications of third parties; the what works centre is not responsible for, and cannot guarantee the accuracy of, those third party materials or any related material.

January 2016
What Works Centre for Local
Economic Growth

info@whatworksgrowth.org
@whatworksgrowth

www.whatworksgrowth.org



HM Government



© What Works Centre for Local
Economic Growth 2016