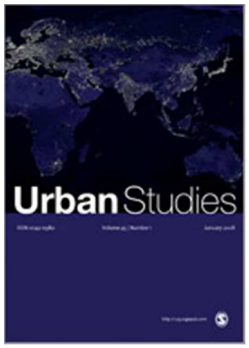

Evaluation of the Local Employment Impacts of Enterprise Zones: A Critique

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Evaluation of the local employment impacts of enterprise zones: A critique

Abstract

Enterprise zone policy is a potential tool for the regeneration of distressed areas, based primarily on tax incentives to businesses locating in the target areas. The tool has been tested in several countries over more than 35 years but there is no consensus on whether or not it is effective and efficient in creating jobs and reducing unemployment in targeted localities. This paper reviews seminal enterprise zone evaluations in the United Kingdom, United States and France. More than one-half of the studies reported local employment benefits but the others reported none and information is limited on what affects policy success. The paper argues that typically narrow-focus research designs and a-theoretical evaluation have contributed to the lack of consensus and policy insight, potentially exacerbated by non-exact data. It proposes richer evaluations with explicit theoretical frameworks, such as the one presented in the paper, more comparative work and use of more accurate data.

Keywords

Enterprise Zones, Evaluation, Employment, Tax Incentives, Local Development

1. Introduction

Enterprise zone policy aims to stimulate economic development in localities where market forces have not been able to bring about regeneration. Its central feature is the award of tax incentives to businesses within designated zone locations, typically for capital investment or employment. Simplified regulation may also be included, notably on land use planning. The policy has operated in various forms and countries for more than 35 years. Recently, OECD (2016) identified 16 OECD countries operating special economic zone policies, including enterprise zone policies in the United Kingdom, the United States, France, Korea and Poland.

This paper reviews seminal evaluations of the local employment impacts of enterprise zones in the UK, the USA and France. Despite a significant evaluation literature, no consensus has emerged on whether or not enterprise zones are effective or efficient in delivering local employment benefits. On the one hand, some researchers have made negative summaries, such as: "enterprise zones have not been successful" (Peters and Fisher, 2002); "at least at the historical level of expenditures, enterprise zones are not an effective way of increasing the probability that the residents of distressed areas are employed" (Elvery, 2009); and "while enterprise zones have been studied extensively, there is little evidence that they have succeeded" (Greenbaum and Landers, 2009). On the other hand, more than one-half of the seminal papers reviewed here have identified positive local employment impacts from enterprise zone programmes, and several suggest that reasonable value for public money has been obtained (O'Keefe, 2004; Rubin, 1990; Rubin and Wilder, 1989; PACEC, 1987, 1995; Busso, Gregory and Kline, 2013; Freedman, 2012; Papke, 1993; Erickson and Friedman, 1990a, 1990b, 1991). This paper considers what might be behind the discrepancies in findings on the local employment impacts of enterprise zones and how future research might deliver greater consensus and policy insight.

The paper starts by describing the objectives and origins of enterprise zone policy and its operation in the UK, USA and France. It then offers a theoretical framework to illustrate a range of processes through which enterprise zones may influence local employment, suggesting issues that should be considered by evaluation. Evaluation findings are then reviewed on employment impacts and the factors influencing them, including consideration of evidence gaps. Some data weaknesses are then highlighted. The paper concludes with a call for more theoretically-based evaluations, more comparisons across zone designs and contexts, and the use of richer and more precise data.

2. Objectives, origins and operation of enterprise zone policy

Objectives

Governments tend to view enterprise zone policy as a means of stimulating growth in places in which market forces have been unable to secure recovery from shock, recognising a potential to improve national efficiency and equity as well as local outcomes. Its appropriateness to the challenge largely depends on how far it can remedy the market and institutional failures underlying the local problems. The spatial mismatch hypothesis suggests that unemployed inner-city job seekers may lack access to non-local job vacancies – for example because of missing information, networks, or transport (Gobillon, Selod and Zenou, 2007; Ihlanfeldt and Sjoquist, 1998). Renewed labour demand may be impeded by barriers to business investment, such as negative externalities from past decline (e.g. loss of skills and work readiness, out-migration of skilled workers); a poor match between the competences of displaced workers and new jobs; downwardly sticky wages; or high local business tax rates. Property markets may leave local sites and premises redundant as a result of negative externalities from dereliction; lack of information on property values following redevelopment; indivisibilities and scale economies in development; and costly, slow and uncertain planning procedures (PACEC, 1995).

Enterprise zones seek to respond by using investment and employment subsidies for businesses located in zones together with property development subsidies and regulatory changes. They may be able to address labour market failures by creating jobs in locations where they can be accessed by job seekers; increasing wages to market clearing levels; removing negative externalities affecting worker competences and business perceptions of investment opportunities; and reducing local business taxes. They might address property market failures by raising the rate of return to property investment and reducing planning constraints. The policy can be seen largely as a demand-side, place-based approach. It can be contrasted with people-based policies (focused on increasing employment opportunities wherever people live) and supply-side place-based policies (e.g. training and job matching for displaced workers).

Origins and operation

The origins of enterprise zone policy lie in a 1977 address by Sir Peter Hall to the British Royal Town Planning Institute in which he suggested an enterprise zone experiment as a possible 'last ditch' solution to Britain's inner-city crisis. It would pare back business taxation and regulations to a minimum in a few inner-city locations with severe unemployment and derelict land so as to attract business relocations and stimulate small firm development (Hall, 1982, 417). The relocations would take activity from other places, but they could deliver a net benefit by drawing unemployed inner-city residents into the labour market as displaced workers in more prosperous areas found alternative jobs. The zones might also generate new activity by stimulating small business creation and growth, and gradually progress inner-city residents up a skills and incomes ladder.

The idea was taken up by the UK's Thatcher government, which established 23 enterprise zones between 1981 and 1984. The zones offered business taxation incentives and simplified planning regulations for 10 years in a mix of inner city, suburban and rural areas with high unemployment and vacant sites. Further designations followed from the mid-1980s to 1996. The UK reintroduced enterprise zones in a modified form in 2012, designating 35 zones initially. The Mark II zones offer a lower value and duration of incentives, restrict benefits to small firms and new-to-zone activities (excluding local relocations), and target areas with capacity for growth in priority sectors as well as regeneration need.

[Insert Table 1 here]

US state governments started to create enterprise zones from the early 1980s and most states have operated zones since then, typically using mixtures of employment and investment incentives. In 2017, 21 states operated zones. US federal government has also been active, operating 40 Empowerment Zones, 20 Enterprise Communities and 40 Renewal Communities in the 1990s and early 2000s, and creating 22 Promise Zones in 2014.

The French government also operates enterprise zones. This was initially in the form of 100 Zones Franches Urbaines, which ran from 1997 to 2014 in urban areas with very high unemployment offering reduced corporate taxes, property taxes and social security contributions. The programme was extended until 2020 in the less generous form of Territoires Entrepreneurs.

Further details of the programmes are given in Table 1.

3. A theoretical framework for enterprise zone evaluation

Although some enterprise zone evaluations have followed comprehensive theoretical frameworks (Busso, Gregory and Kline, 2013; PACEC, 1987, 1995), most of the employment-focused evaluations have concentrated on the relationship between zone status and headline employment outcomes. Richer evaluations will require more detailed theoretical frameworks. Figure 1 offers a framework exploring neoclassical firm and place equilibrium effects and the influence of factor mobility, substitution and price elasticity, although other theoretical views could be taken. The processes in this framework may be influenced by differences in zone programme designs and contexts, and hence potentially help explain differences across the literature in evaluation findings.

[Insert Figure 1 here]

The top of the Figure illustrates channels through which enterprise zone incentives could increase employment and property demand. It classes the incentives into employment, capital equipment and real estate subsidies. A key channel involves reduced unit output costs for firms from employment and capital subsidies, and from real estate subsidies if the firm owns its own premises. The increased profitability may lead to net firm in-migration and enable pre-existing establishments to reduce prices or increase investment (in products, equipment, training, marketing etc.), hence increasing their competitiveness and stimulating expansion. This may increase demand for labour and property.

The framework suggests some potential mediating influences. If capital subsidies are large compared to employment subsidies then labour demand growth could be counterbalanced by capital-labour substitution, which could be a particular problem in manufacturing-dominated zones, since substitution may be easier in manufacturing. Incumbent establishments could also respond to increased profitability by distributing profits rather than reducing prices or investing, although limiting subsidies to new recruitment or new-to-zone firms might address

the issue. Third, reduced business operating costs could be capitalised by property owners in increased rents and prices, particularly if supply is constrained.

The bottom of the Figure illustrates how increased labour and property demand, and increased returns on property investment brought about by real estate subsidies, may stimulate growth by increasing the effective supply of labour and land. The emphasis is on reductions in long-term unemployment, which is seen as net of macroeconomic crowding out and hence as a national gain rather than a spatial redistribution. Self-reinforcing local agglomeration benefits could also be generated. The framework also suggests a possible impact on equity as employment and income outcomes improve for poorer localities and people.

The framework also suggests some further potential explanations for the discrepancies that have emerged in enterprise zone evaluation findings. For example, new employment could go to inactive labour, commuters or in-migrants rather than the unemployed, and the extent to which this happens may be affected by local context (e.g. large urban areas may see more inward commuting). Wage growth might also reduce employment growth, particularly in places and periods of constrained labour supply. A displacement of long-term unemployment to neighbouring areas could also occur, which would be damaging if those areas also have high long-term unemployment. The importance of these effects may vary with local context and with programme design, potentially helping explain variations in evaluation results across different programmes.

4. Key evaluation findings

Do enterprise zones increase local employment or reduce unemployment?

[Insert Table 2 here]

Table 2 provides summary information on the local employment findings of enterprise zone evaluations. It clearly reveals the discrepancies. Of 34 evaluations, 21 found that enterprise zone intervention increased employment or reduced unemployment, whereas 12 found that

intervention had no impact on employment or unemployment levels. One reported quite mixed findings.

Employment impacts

Several evaluations of US state and federal interventions found no impact on local employment levels, one found a negative employment impact (Lambert and Coomes, 2001), and one found mixed results (Lynch and Zax, 2011). In contrast, several evaluations found substantial employment benefits. PACEC (1995) found that UK Mark 1 enterprise zones generated a near three-fold increase in local employment levels over 10 years. Two US federal empowerment zone evaluations put the policy-generated increase in local employment at 34% (Ham et al, 2011) and 15% (Busso, Gregory and Kline, 2013). Among US state programme evaluations, policy was found to have increased local employment by more than one-third in Indiana (Rubin and Wilder, 1989), by an average of 10% across 17 states (Erickson and Friedman, 1990a, 1990b, 1991) and by 10% in Texas (Freedman, 2012). In France, Rathelot and Sillard (2008) found that zones had stimulated a local employment increase of approximately 15%, and Mayer, Mayneris and Py (2017) found a local employment increase of 24%. In between those evaluations showing substantial employment impacts and those estimating no benefits, there are several studies that found benefits that were relatively modest in scale. One US federal empowerment zone evaluation found an increase of only 130 jobs (Hanson and Rohlin, 2011, 2013), another found a modal increase across census tracts of approximately 13 jobs (Rich and Stoker, 2010), an evaluation of the Colorado state programme found an employment increase of 4% (Billings, 2009) and an evaluation of the California state programme found an employment increase in the order of 5%. In France, Givord, Rathelot and Sillard (2013) found increases of between 3 and 12 percentage points in employment and hours worked on zones.

Unemployment impacts

The majority of the studies that investigated impacts on local unemployment found benefits, although the precise measures varied. Ham et al. (2011) found that federal empowerment zones reduced the zone unemployment rate by an average of 9 percentage points and that a range of state enterprise zone programmes reduced the zone unemployment rate by an

average of 1.6 percentage points. Sridhar (2000) found that one state programme had reduced local unemployment by 2.9 percentage points. On other measures, Papke (1994) found that a US state programme had reduced numbers of local unemployed people by 19%, Gobillon et al (2010) found that French zones increased the exit rate from unemployment into a job by 3% per semester for local residents, and Rich and Stoker (2010) found that US federal empowerment zones reduced unemployment in one-half of the cities they evaluated. On the other hand, Oakley and Tsao (2006) found that US empowerment zones had no impact on local unemployment while Rogers and Tao (2004) detected no statistically significant impact of Florida small city zones on the unemployed-to-population ratio.

Are enterprise zones cost effective?

Although many evaluations found employment benefits, only 10 compared the benefits with costs so as to permit some assessment of whether the policy could be considered cost effective. The majority of these found public costs per job created that might be considered to be very broadly in line with results achieved by similar interventions such as UK regional policy or US job subsidies. Five found cost-per-job created below approximately 8 000 USD per annum in 2016 prices (O'Keefe, 2004; Rubin, 1990; Rubin and Wilder, 1989; PACEC, 1987, 1995). Three found cost-per-job created of between approximately 8 000 USD and 20 000 USD per annum in 2016 prices (Busso, Gregory and Kline, 2013; Freedman, 2012; Papke, 1993). Erickson and Friedman (1990a, 1990b; 1991) and Rubin (1990) both concluded that enterprise zone policy had a negative cost per job once additional tax revenues generated had been taken into account. On the other hand, Rathelot and Sillard (2008) and Hanson and Rohlin (2011) estimated very high costs per job created, while of course several evaluations found no employment benefits that could be weighed against costs incurred.

What factors influence the employment impacts of enterprise zones?

Although in a few cases evaluations have produced different findings for essentially the same zone programmes in the same places and at the same times, the various evaluation studies are generally associated with different programmes and different contexts. Indeed there has been a richness of policy experimentation that might offer important insights in how to strengthen enterprise zone policy design by clarifying how different policy designs and contexts interact

with processes affecting local employment outcomes, such as those suggested in Figure 1. Regrettably, few evaluations have systematically investigated these potential influences, but several evaluations offer useful indications that certain processes highlighted in the Figure merit more evaluation attention.

Capital-labour substitution

The extent to which zone employment generation is impeded by capital-labour substitution in existing zone firms and high capital intensity in new-to-zone firms could vary with factors such as the relative value of capital and labour subsidies in zone packages, whether or not subsidies are conditioned on recruitment or capital investment and the relative importance within zones of industries with high capital-labour substitutability. Certain studies offer insights. Papke (1993, 1994) found that a capital-weighted subsidy resulted in increased employment as well as capital use. Greenbaum and Engberg (2004) and Bondonio and Greenbaum (2007) found that increased capital use by incumbent firms did not explain the absence of employment generation across the range of zone programmes they studied. On the other hand, Lynch and Zax (2011) argued that greater capital-labour substitution among establishments on urban zones might explain why rural zones had generated employment increase while urban zones did not, and could have been related to lower rural wage rates.

Labour and wage elasticity

If labour supply is constrained, increased employment demand generated by zone intervention might have much stronger impacts on wage rate growth than employment growth. The evidence from the evaluations is not very clear on how far this is an issue. There are few studies showing that an increase in wage rates has reduced employment growth, perhaps reflecting a genuine targeting of zones on areas with labour surplus. For example, O'Keefe (2004), Givord, Rathelot and Sillard (2013) and Mayer, Mayneris and Py (2017) all found that the zone employment growth that occurred was in situations where zone wage rates remained stable overall. Indeed, the latter authors found that the wage rates of non-low-wage workers fell, reflecting reduced relative demand for these workers. Other studies found that zone wage rates and employment volumes moved hand-in-hand (Busso, Gregory and Kline, 2013; Ham et al, 2011) or did not move at all (Oakley and Tsao, 2006). Greenbaum

and Engberg (2004) and Bondonio and Greenbaum (2007) found that zones reduced average wages (possibly due to requirements in some states that new hires are zone residents) but still did not increase employment.

Capitalisation of subsidies

One of the processes that might explain why enterprise zones sometimes do not create employment is a potential capitalisation of enterprise zone subsidies by property investors, developers and landowners. They may be able to react to the increased property demand by increasing the sale or rental prices of land and premises on zones, until the profitability of firms is equal on and off zones, leaving firms indifferent to an on-zone or off-zone location (Bond, Gardiner and Tyler, 2013; Landers, 2006). The scale of capitalisation could be influenced by factors such as the scale of availability of vacant local premises and the share of tenants and owner-occupiers on zones. The evaluations provide some evidence of capitalisation. PACEC (1995) found an accrual of subsidy values to landlords through rental appreciation of between 20% and 50% on the majority of UK Mark 1 zones. The rate was highest in tight property markets and fell towards the end of the zone lifetimes. Similarly, Hanson (2009) found that US Empowerment Zones had a substantial positive impact on median property values, which increased by over USD 100 000. On the other hand, Boarnet and Bogart (1996) found that New Jersey enterprise zones did not affect property values.

Distribution of job gains

The share of new zone jobs going to in-commuters, new residents and people who were formerly inactive in the labour market rather than long-term unemployed residents could vary with factors such as the size of zones relative to their travel-to-work areas and whether or not the incentives are tied to hiring long-term unemployed residents. Some evaluations found that quite high proportions of jobs went to zone residents and the unemployed. On Mark 1 UK zones, approximately 90% of non-managerial/professional recruits were local residents and approximately 34% of recruits were previously unemployed (PACEC, 1995). Erickson and Friedman (1990a, 1990b, 1991) estimated that approximately 61% of jobs went to residents and approximately 48% to the unemployed on US state zones. On Texas zones, workplace employment growth only slightly exceeded resident employment growth (Freedman, 2012).

On the other hand, Busso, Gregory and Kline (2013) found that one-half of new jobs on US urban empowerment zones went to commuters and Papke (1993) found that only 15% of jobs created by Indiana zones went to zone residents.

Displacement of activity from outside of zones

Relocation or displacement of economic activity onto zones that would otherwise go elsewhere could be expected to offset the job creation benefits of zone policies, at least in so far as the activity is displaced from other high unemployment areas. The degree of displacement could be affected by factors such as the distance of zones from other distressed areas and whether or not zone incentives are available for relocations. There is evidence from some of the evaluations that displacement can be a significant problem. By mid-lifetime on the UK Mark 1 zones, approximately 25% of new jobs had been displaced from other high-unemployment areas through establishment relocations; a further 31% of jobs were in inward investors that had chosen enterprise zones over alternative locations (which could include other high unemployment areas) (PACEC, 1987). At the end of the UK Mark 1 zone lifetimes, the net job loss to the areas surrounding the zones was estimated at 51% of the jobs created within the zones (PACEC, 1995). Hanson and Rohlin (2011, 2013) found even larger displacement onto US urban empowerment zones from neighbouring and similar areas, which nearly completely offset the employment benefits generated within the zones. In France, Mayer, Mayneris and Py (2017) found that all zone employment growth was the result of relocations or diversion of new establishment creations from the rest of the municipality hosting a zone; i.e. the policy generated no additional activity for municipalities hosting zones overall. Similarly, Givord, Rathelot and Sillard (2013) found negative spillovers from French zones on establishment stocks in the 300-metre rings surrounding zones, which nearly fully counterbalanced the growth in the on-zone establishment stock. On the other hand, Rathelot and Sillard (2008) and Gobillon, Magnac and Selod (2010) did not find important displacement effects on neighbouring municipalities from French urban zones. Furthermore, various US state enterprise zone policy evaluations found no displacement from other local areas (Greenbaum and Engberg, 2004; Neumark and Kolko, 2010; Freedman, 2012), while Ham et al (2011) found that the limited local spillovers that did exist were positive.

Sites and premises availability

The ability of certain enterprise zones to offer large volumes of available sites and premises to accommodate new business activity could have an important influence on the scale of employment impacts. Much of the success of the UK Mark 1 zones was attributed to an increase of 60% in the floor space available on the zones between their designation and the mid-point of their lifetimes. This was the result of the presence of large empty and redundant sites within designated zone areas combined with public investments in removal of dereliction and landscaping, streamlining of planning procedures, incentives for property investors, and subsidies to premises occupants (PACEC, 1987). Enterprise zone job creation effects could be more limited in places where land and premises are more constrained.

What is the influence of different zone programme designs and zone contexts?

[Insert Table 3 here]

The above discussion points to a number of potentially important processes affecting the local employment effects of enterprise zone policy that could be influenced by various aspects of zone programme designs and zone contexts. Mayneris and Py (2014) make a similar argument, focusing on the possible influences of initial conditions of zones in terms of density of existing firms and accessibility to workers and consumers, zone exposure to industries where firm relocation costs are lower, such as professional services, and the amount and range of tax incentives offered by the policy. Table 3 summarises some key variations across the evaluations in the nature of the zone programme designs and zone contexts evaluated. It shows that there are a number of variations in the focus of the evaluations that might be exploited for comparative analysis. These variations include whether or not the evaluated programmes made zone incentives conditional on new hiring, designated zones solely on grounds of economic distress, placed zones solely in urban locations, and operated in periods of strong or weak national labour market performance.

Unfortunately, there has been relatively little deliberate comparative assessment within individual evaluations of the influences of different zone programme designs and different zone intervention contexts on local employment impacts, although some evaluations have done this. Furthermore, the data reported in the individual evaluations do not lend themselves

to a formal meta-analysis or meta-regression on how such variations affect employment impacts because of numerous differences in the nature and definitions of the explanatory and response variables that have been used. We undertook bivariate analyses for this paper, but they showed no clear relationships between whether or not the zones generated employment benefits and variations in zone incentive tying, levels of distress, urban-rural context or national labour market performance.

On the other hand, there are some indications from specific evaluations that some of these aspects of zone programme design and context may be influencing employment impacts. One of the major criticisms of enterprise zone policy is that it may offer important windfall gains to pre-existing businesses on zones if they are able to access employment or capital subsidies intended to encourage growth without changing their behaviour (Bartik, 2001; Bartik and Eberts, 2012; Bond, Gardiner and Tyler, 2013; Neumark and Grijalva, 2013; Neumark and Simpson, 2014). For example, Neumark and Kolko (2010) highlight a situation involving Californian zones, whereby firms could retroactively claim hiring tax credits up to four years after hiring took place, implying the possibility of significant windfalls. They found that if zone managers concentrated on marketing retroactive credits to existing firms their zones created fewer jobs. Givord, Rathelot and Sillard (2013) also illustrate the windfall issue, showing that there was no impact on the economic activity of incumbent firms in French zones although they were eligible for most of the tax incentives by their simple presence in the zone. Programme designs that make subsidies conditional on new hiring might reduce this windfall effect. However, one of the few studies that compared programmes tying incentives to job creation or capital investment with those that did not found that conditioning of incentives made no difference to aggregate zone employment creation (Bondonio and Engberg, 2000).

One of the issues that been subject to significant comparative attention, at least in a minority of evaluations, is the influence of geographical context on zone employment impacts. Erikson and Friedman (1990b) found that zones were more successful if they were in ‘retrievable’ areas rather than severely economically distressed areas. Moore (2003) found that rural zones in California were more likely to grow than urban zones. PACEC (1995) found that employment growth was greatest in accessible suburban areas, and to a lesser extent in rural areas, and performance was weakest in the most distressed urban core areas. Lynch and Zax (2011) found that while urban zones in Colorado had no positive employment impacts, there were positive impacts in rural zones, possibly reflecting availability of an additional

complementary subsidy there and a lower probability of capital-labour substitution. Mayer, Mayneris and Py (2017) found that policy impact was stronger in zones with larger establishment densities, suggesting that policy impact may also be influenced by agglomeration effects.

None of the evaluations included much discussion of the extent to which zone employment effects vary between periods of strong and weak overall labour market performance, although this might be expected to be a significant factor in zone performance. On the other hand, a number of other potentially important zone design and context features are highlighted by certain evaluations. Notably, the positive employment impacts of zones might be greater: in areas with more capable local development agencies (Rich and Stoker, 2010) or where an area development plan was required (Bondonio and Greenbaum, 2007); in programmes that offer a greater value or wider range of incentives (Erikson and Friedman, 1990b: Beck, 2001) and complementary job training and community development support (Beck, 2001); in zones with smaller land areas (Bondonio and Greenbaum, 2007; Erickson and Friedman, 1990b); and in zones with lower shares of manufacturing, linked to greater capital-labour substitution opportunities in manufacturing than in services (Neumark and Kolko, 2010).

Given the relatively disparate nature of the current evidence, more studies are needed that examine and report on the potential influences on zone success and which of them are important and which ways. In particular, more systematic comparative evaluations of the impact of variations in programme designs and zone contexts would be very valuable in helping inform future enterprise zone policy design.

5. Improving data quality

[Insert Table 4 here]

Table 4 summarises key methodological features of the reviewed evaluations. It distinguishes between a few (generally older) studies that estimated impact by surveying managers of zone-based firms and a vast majority of econometric or shift-share analyses typically comparing employment changes between treatment and control areas. It provides brief

information on the methodologies applied. A potential weakness of the beneficiary survey methodologies is that their self-assessment impact estimates may not be reliable (Greene, 2008). Less well recognised is a potential weakness with many of the econometric studies, which, as indicated in Table 4, frequently use treatment and control data that do not fully match the policy-on and policy-off situations required for modelling.

There are three main issues. First, approximately one-half of the econometric evaluations used treatment data that did not entirely match the zone geography, generally by approximating zones with larger units that included some non-zone territory. Further, approximately one-third used control area data that included some zone territory. These imprecisions could affect the accuracy of results, particularly if zones have important spatial spillovers. Indeed, Mayneris and Py (2014) argue that poor delineation of zone boundaries together with endogeneity issues involving time-varying unobservable factors that are not picked up by difference-in-difference and propensity score matching can explain part of the conflicting results of enterprise zone evaluations to date. To help address the problems some recent studies have used precise GIS coding to attribute firms to zone and non-zone areas, whilst US federal empowerment zones boundaries were drawn up to match with census areas. Second, nearly one-half of the econometric studies used data that did not match the time periods of treatment, generally including by some non-treatment years and excluding some treatment years. Moreover, several studies examined impacts only a short time (e.g. 1 to 3 years) after zone establishment, although zones may build up jobs gradually, while very few studies took a sufficiently long view to assess whether zones have durable impacts after de-designation. Third, several evaluations used only data for manufacturing, although zones also typically support service sector firms and there may be differences in the ways that services establishments and manufacturing establishment respond to incentives, particularly concerning capital-labour substitution. It is also worth recognising that other area-based policy interventions often operate in areas targeted by enterprise zone programmes and enterprise zone evaluations have not always sought to disentangle enterprise zone impacts from those of the other interventions.

As well as showing the estimated employment impacts of the different evaluations, Table 2 also presented a very simple characterisation of the closeness of fit of the control and treatment data used in each study. It highlights several areas in which the data used in the evaluations have not fully matched the treatment or non-treatment situations. Only around five of the evaluations were able to apply fully matching data for both the treatment and

controls. Only 20 of the 34 studies reviewed here used control group areas that were both quite similar in economic conditions to the treatment areas and unaffected by potential spillovers. It is important to address these data issues in order to increase confidence in enterprise zone evaluation results and the policy conclusions that can be drawn from them.

6. Conclusions

The aggregate evaluation evidence is currently divided on whether or not enterprise zone policy is an effective and efficient tool for local employment development. While problems with the quality of data used for some evaluations may be an issue, it is likely that the major explanation for discrepancies in findings across evaluations is to do with differences in the programme designs and operating contexts of the zones they have evaluated. It is therefore a priority to increase understanding of the influence of enterprise zone programme designs and application contexts. Building the evidence required implies developing more theoretically-driven studies that seek to identify the range of factors and channels that influence the degree of enterprise zone policy success in local employment development and how they could be affected by enterprise zone policy designs and contexts. More comparative evaluations would also help, seeking to cover multiple programme designs and contexts in the same studies. At the same time, confidence in evaluation results could be increased by efforts to improve the match between the treatment and control data and the geographies and timings of the zone interventions. A boosted enterprise zone evaluation agenda of this kind would help governments make more informed decisions about enterprise zone policy and other place-based tax incentive driven interventions for local employment development.

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Table 1: Key design features of enterprise zone programmes in the UK, France and USA

Location and period	Instruments	Target enterprises	Life time of a zone	Number of zones	Types of locations
ACTIVE NATIONAL PROGRAMMES					
UK 2012+	100% business rate discount up to GBP 275 000 Tax allowances for capital expenditure in zones located in assisted regions Simplified local authority planning (including automatic planning permission for certain development) Investment in super-fast broadband Business rate growth allocated to local authorities for reinvestment	New activity in firms on zones, excluding relocations	5 years	45	Urban and rural areas with both distress and economic potential
France 2015+	Corporate tax incentives (100% in first 5 years falling to 20% in years 8-9) of up to EUR 50 000 per year plus EUR 5 000 for each full-time hiring of a local resident	Small firms (up to 50 employees) employing local residents (one-third of employees or new hires)	2015-2020	100	Distressed urban areas
USA Federal Promise Zones 2014+	Provision for tax incentives similar to previous Empowerment Zones, if enacted by Congress Preferences for certain federal grant programmes Five staff to recruit and manage volunteers and strengthen economic development capacity	All firms	10 years	22	Distressed urban, rural and tribal communities

EXPIRED NATIONAL PROGRAMMES					
UK 1981-2006	100% allowances for capital expenditure against corporation and income tax Exemption from local property taxes Simplified planning including an automatic right of development for specified land uses	All firms	10 years	32	Distressed areas
France 1997-2014	Exemption for 5 years from local business rates, corporate income taxes, and property taxes. Exemption for 5 years from employer social security and health contributions on the salary component below 1.4 times the minimum wage, if at least one-third of the workforce is resident in the surrounding urban development priority area. Prolonged exemption from local business tax for up to 9 years depending on business size	Firms with less than 50 employees	Until expiry decision	100	Distressed urban areas
USA federal empowerment zones 1994-2013	Employment tax credits of up to 20% of annual wages (up to wage of USD 15 000) earned by zone residents Capital gains tax exemptions, tax-exempt bond financing and increased depreciation allowances for business and property investments Social Services Block Grant funds of USD 100 million per zone for business support, training programmes, education, housing etc.	All firms	10 years plus extension	40	Distressed areas
ACTIVE US STATE PROGRAMMES[1]					

Alabama	Exemptions from sales and use tax on machinery and equipment and construction materials Tax credit for 5 years for hiring new permanent employees Tax credit for new investments or improvements to existing facilities Tax credit for training new permanent employees	Manufacturing and distribution firms, excluding relocations	Until expiry decision	28	Distressed areas
Colorado	Tax credits for new employee hire, training and health insurance Tax credits for investment in equipment, vehicles, building rehabilitation and research and development Sales and use tax credits for manufacturers Tax incentives for contributions to community development projects	All firms	Until expiry decision	18	Distressed areas
Connecticut	Corporate tax credit for business formation, business expansion or renewal and hiring Property tax credits for real estate development	Manufacturing, distribution, business services	Until expiry decision	17	Distressed communities, including those with defence industry cutbacks
Georgia	Local property tax exemption Abatements or reductions on occupation taxes and regulatory fees	Firms that create jobs or economic stimulus	10 years	16	Distressed areas
Hawaii	Exemption from General Excise Tax Personal or corporate income tax and state	Non-retail firms	Until expiry decision	22	Distressed census tracts

	unemployment premium credits				
Illinois	Sales tax exemptions for building materials Sales tax exemption for machinery and equipment investment Utility tax exemption Property investment tax credit Regulatory relief Discretionary local incentives	Firms that invest in property or invest and create jobs	15 years, with possible 10 year extension	104	Distressed areas
Indiana	Employee income tax deduction Tax deductions for incremental wages paid to zone residents Tax credit to businesses making loans to enterprise zone businesses Income tax credit for individuals and businesses making equity investment in zone businesses Property tax investment credit	All firms	Until expiry decision	22	Distressed areas and closed military bases
Louisiana[2]	Job tax credit Sales and use tax rebates for machinery, equipment and materials Investment tax credit on capital investment	Firms creating at least 35% of net new jobs for enterprise zone residents or other disadvantaged state residents	Until expiry decision	20	Distressed areas
Maryland	Corporate income tax credits for eligible new employees	All firms	10 years	30	Distressed areas

	Local property tax credit for property improvement				
Minnesota	Sales tax exemption for construction equipment and materials Corporate income tax credit for additional workers Debt finance credit for property development Property tax credit for new and expanded facilities	All firms except retailing, personal services, financial institutions and public utilities	Not specified	5	Areas of between 100 and 400 acres within border cities
Mississippi	Full exemption on state income and franchise taxes Full sales and use tax exemption on equipment and machinery purchases Property tax exemption	Manufacturing, distribution and research and development businesses that create 10 or more jobs	Until expiry decision	18	Distressed counties
New Hampshire	Employee tax credit	All firms creating jobs in zones	In place until 2020	189	Areas with vacant or under-utilised industrial land and buildings
New Jersey	Reduced sales tax Tax free purchases on capital equipment and real estate Subsidised unemployment insurance for low income workers Energy sales tax exemption Tax credits for hire of employees and qualified investments	All firms	Until expiry decision	32	Distressed urban areas

New York[3]	Sales or use tax refund or credit on property, utilities and certain services Corporate and income tax credit for net new jobs Personal income tax exclusion	Non-retail firms, excluding relocations, that create new jobs and align with the mission of a local higher education institution	10 years	10	On or near higher education institution campuses
Ohio	Local property tax incentives for property investments that create jobs	Non-retail projects that establish or expand operations in the state and create or maintain jobs	Until expiry decision	400	Distressed areas
Oregon	Exemption from local property taxes on plant and equipment and property investments	Non-retail businesses	In place until 2025	69	Distressed areas
Pennsylvania	20% credit against corporate income tax for expenditure on real property improvements	All firms making investments that create employment opportunities for low income individuals	7 years	8	Distressed areas
Texas[2]	Sales and use tax refunds on investment in property, machinery and equipment based on level of investment and number of jobs created	Nominated projects with at least 25% of new employees from the zone or disadvantaged	Until expiry decision	Zones cover 5000 census block groups; 23 entire distressed	Distressed block groups or counties

		populations (35% if the firm is located outside the zone)		counties are included	
Utah	Employee credit – state income tax credit for job creation Capital investment credit – state income tax credit for rehabilitating vacant buildings and investment in plant, equipment and property	All firms expanding or relocating to the zone	5 years	69	Distressed rural areas
Virginia	Job creation grant for creation of high wage full-time jobs Real Property Investment Grant Local incentives	All firms	10 years, renewable for 10 years	57	Distressed areas with offer of local incentives and economic potential
Wisconsin[4]	Job creation and retention income tax credits Capital investment income tax credits for property, machinery and equipment Environmental remediation income tax credits	All firms excluding retail, farms, financial institutions, hospitality, media outlets, primary medical care	5 years	3	Distressed cities
EXPIRED US STATE PROGRAMMES[1]					
Arizona (to 2011)	Income and premium tax credit for net increases in eligible employment Property tax benefits for manufacturers	All firms in zones	5 years	19	Various
Arkansas (to 2003)	Income tax credits for new hires Sales and use tax on machinery and equipment	Non-retail firms	Until expiry decision	450	Distressed areas

	and construction materials				
California (to 2014)	Tax credits for hiring eligible employees Sales and use tax exemptions on capital asset purchases for manufacturers and research and development enterprises Investment tax credit	All firms	6 years	42	Distressed areas
Florida (to 2015)	Corporate and sales tax credits for hiring Sales tax credits for buildings and equipment Corporate tax incentives for buildings Sales tax exemption on energy Tax incentives to businesses for contributions to community development projects	All firms	10 years	65	Distressed areas
Iowa (to 2014)	Local property tax exemption Funding for training new employees Refund of sales, service, or use taxes paid for construction Investment tax credit for machinery, equipment and property Research and development tax credit	Non-retail firms making expansions or relocations from outside the state	Until expiry decision	61	Distressed counties and cities
Kentucky (to 2008)	Machinery and equipment and building materials exempt from sales and use taxes Vehicles exempt from vehicle usage tax Tax credit of 10% of wages for employees who were unemployed or welfare recipients Optional local tax incentives	New businesses and existing businesses expanding investment or employment	10 years	10	Distressed areas

		by 20%			
Missouri (to 2013)	Income tax and insurance tax credits for new employment and investment Local property tax abatement for new projects	All firms excluding retail, leisure and social services	Until expiry decision	115	Areas with distress and economic potential
New York (to 2010)	Sales tax credits and refunds Property tax credit and abatements Corporate tax credit Wage tax credit for new hiring Tax credits for new investments Utility rate savings Tax credit for investments in community projects	All firms	Until expiry decision	82	Distressed urban neighbourhoods
Rhode Island (to 2015)	Wage tax credit of 50% to new full-time employees and 75% for enterprise zone residents (up to USD 15 000 per employee)	Businesses that increase employment by 5%	5 years	10	Groups of up to 5 census tracts with distress, economic potential and action plans

Notes: [1] A number of states have tiered incentives that are available across the whole state but vary in value according to the level of distress of the county or locality. They are excluded from this table and paper, which focus on programmes that limit incentives to designated zone areas within the state. States operating tiered incentive programmes include Arkansas, Maine, Missouri, Oklahoma, South Carolina and Tennessee. [2] In these states, firms benefiting from incentives do not have to be located within the enterprise zone but they should hire enterprise zone residents. [3] This has a somewhat different focus compared with other state enterprise zone programmes because of its emphasis on higher education institution linkages. [4] This refers to the Development Opportunity Zones. Wisconsin also operates Enterprise Zone Tax Credits but they are typically designated for individual, large-scale businesses rather than localities needing regeneration.

Table 2: Employment impact findings of seminal enterprise zone evaluation studies

Study	Location	Key findings on employment impact		Characterisation of quality of control and treatment group data[1]				
		Employment increase or unemployment reduction	Details	Control group		Treatment group		
				Similar areas	Not affected by spillovers	Exact areas	Exact timings	Exact sectors
Beck (2001)	USA: 51 zones, various states	✓	Zones generated growth in number of firms and employment. Job training and community development support was important for job growth.	✓	✗	✓	✓	✓
Billings (2009)	USA: 16 Colorado zones	✓	Increase of between 1.5 and 1.8 new jobs in new establishments and between 0.0 and 0.3 new jobs in existing establishments, representing up to a 3.6% increase in employment in total.	✓	✗	✓	✓	✓
Boarnet & Bogart (1996)	USA: 7 New Jersey zones	✗	No employment impact.	✓	✓	✗	✓	✓
Bondonio & Engberg (2000)	USA: 5 states	✗	No employment impact on zone area plus immediate surroundings. Impact does not depend on the monetary amount of the incentives or specific features of programme design.	✓	✓	✗	✓	✓

Bondonio & Greenbaum (2007)	USA: 10 states	✗	No net impact on employment: growth in new and existing establishments offset by job losses in establishments that closed or moved. Restricting the geographic extent of the programmes increases growth from establishments new to the zones. Tying incentives to job creation increases employment growth in existing establishments.	✓	✓	✗	✓	✗
Busso, Gregory and Kline (2013)	USA: 6 urban empower-ment zones	✓	Employment increase in zone establishments of approximately 15%. By dividing the annual cost of the employment tax credit by the estimate of approximately 7 300 new jobs we can derive a cost per job of approximately USD 7 500 per annum in current prices (approximately USD 10 200 in 2016 prices).	✓	✓	✓	✗	✓
Couch et al (2005)	USA: 25 Mississippi zones	✓	Increase of 1.5% in the annual share of new manufacturing jobs as a proportion of all manufacturing jobs in counties containing enterprise zones.	✓	✓	✗	✓	✗
Dowall (1996)	USA: 10 California zones	✗	No employment impact.	✓	✓	✗	✓	✓
Elvery (2009)	USA: Florida and California	✗	No impact on resident employment 38 months after designation.	✓	✓	✗	✓	✓

Erickson & Friedman (1990a, 1990b, 1991)	USA: 357 zones in 17 States	✓	Increase in employment of 10% over two years. Zone residents received 61% of jobs created. New activity generated more new local tax revenues than the cost of local taxes foregone. Number of zones per state negatively related to impact.	No control group		✓	✓	✓
Freedman (2012)	USA: zones in Texas with poverty rates around 20%	✓	Increased resident employment by 1-2% per year, averaging a 10% increase over 8 years. No spillover effects on resident employment in neighbouring localities. The jobs created are mainly in low to medium wage and skill jobs. Cost per job in the order of 6 500 USD current prices (approximately USD 7 200 in 2016 prices).	✓	✓	✓	✓	✓

Givord et al (2013)	France: 51 second round ZFU	✓	Increase of 5 to 7 percentage points in growth of establishment stock. 25% increase in establishment births, 100% increase in establishment relocations. Positive impact on number of jobs and hours worked, but significant in only one year. No impact on economic activity of incumbent businesses overall. Negative spillovers on establishment stock in immediate neighbours. Greatest impacts on business services and retailing.	✓	✓	✓	✓	✓
Gobillon et al (2010)	France: 9 ZFU in Paris region	✓	Increased rate of exit from unemployment of 3% (amounting to about 10 new exits per semester per zone). No spillover effects on neighbouring localities. The impact on unemployment is only short term (up to 3 years).	✓	✓	✗	✓	✓
Greenbaum & Engberg (2000)	USA: 6 states	✗	Zones did not positively impact on establishment employment, resident unemployment or per capita income. Zones did not increase housing prices or occupancy rates.	✓	✓	✗	✗	✓

Greenbaum & Engberg (2004)	USA: 6 states	✗	No net impact on employment: growth due to births offset by reduced growth in existing establishments. Zones did not displace activity from neighbouring locations.	✓	✓	✗	✓	✗
Ham et al (2011)	USA: state enterprise zones and federal empower-ment zones in 13 states	✓	Enterprise zones reduced the unemployment rate by 1.6 percentage points and increased employment by 4% on average. Empowerment zones reduced the unemployment rate by 8.7 percentage points and raised employment by 34% on average.	✓	✓	✗	✗	✓
Hanson (2009)	USA: 6 urban empower-ment zones	✗	No employment impacts. Subsidies appear to be absorbed by increased local property values.	✓	✓	✓	✗	✓
Hanson and Rohlin (2011, 2013)	USA: 6 urban empower-ment zones	✓	Approximately 20 new establishments and 130 jobs attracted after 5 years, at a cost per job of approximately USD 2.9 million in current prices (approximately USD 3.2 million in 2016 prices). Negative spillovers of approximately 30 establishments and 480 jobs losses on neighbouring areas and economically similar areas.	✓	✓	✓	✓	✓
Lambert & Coomes (2001)	USA: 1 Kentucky zone	✗	Negative impact on employment growth in the original zone area, although some growth in the expansion area around the airport.	✓	✓	✓	✗	✓

Lynch & Zax (2011)	USA: Colorado zones	Mixed	Urban zones did not increase employment per establishment. Employment impacts were negative for the large establishments and agricultural establishments in urban zones. Rural zones had small positive employment effects. Urban and rural zones had no effect on wage rates.	✔	✔	✔	✗	✔
Mayer, Mayneris & Py (2003)	France: 41 second round zones	✔	Increase of 27% in probability that an establishments will locate in the zone part of their municipality. Policy increases zone employment by 24% on average, with an increase of 25% in low-wage and 11% in non-low-wage jobs. All the zone impact is due to intra-municipality diversion. Impacts are stronger in zones with higher establishment density and for more mobile industries (medical professions, business services). No impacts on wage rates of low wage workers, wage rates of non-low-wage workers decline.	✔	✔	✗	✔	✔
Moore (2003)	USA: California, 20 zones	✔	Increase of 5% in number of firms. Firm numbers explain 80% of employment variation. Growth concentrated in business services, wholesaling and retailing.	✔	✔	✗	✔	✔

Neumark & Kolko (2010)	USA: California, 42 zones	✗	No employment increase. No shift of employment toward low-wage industries. Some reduction in number of establishments.	✓	✗	✓	✓	✓
Oakley & Tsao (2006)	USA: 4 urban Empowerment Zones	✗	No impact on unemployment overall, although decreases in Chicago.	✓	✓	✓	✗	✓
O'Keefe (2004)	USA: California 39 zones	✓	Increase in employment of 3.1% per annum for first 6 years, but the effect did not persist in later years. Total annual cost per job in line with similar programmes: USD 2 846 in 1996 and USD 4 929 in 1995 in current prices (approximately USD 4 100 and USD USD 7 300 in 2016 prices).	✓	✓	✗	✓	✓
PACEC (1987)	UK: 23 zones	✓	Creation of 13 000 net additional jobs. Cost per job of GBP 23 000 in current prices (approximately GBP 54 000 in 2016 prices).	No control group		✓	✓	✓

PACEC (1995) Potter & Moore (2000)	UK: 22 zones	✓	58 000 additional jobs created (a three-fold increase). Annual cost per job of GBP 1 700 in current prices (approximately GBP 2 500 in 2016 prices). Cost per job higher on urban zones than suburban and rural zones. Local transfers accounted for 35% of establishments and 28% of employment on zones. Most jobs were created in the middle years of the ten-year zone designation periods.	No control group		✓	✓	✓
Papke (1993, 1994)	USA: Indiana zones	✓	19% reduction in the number of unemployment claimants. Annual cost per job created for an unemployed claimant ranged across zones from USD 526 to USD 10 238 USD in current prices (approximately USD 1 000 and USD 19 000 in 2016 prices), in line with other US job subsidy schemes.	✗	✓	✗	✓	✓

Rathelot and Sillard (2008)	France: 41 zones	✓	15% increase in employment. All the employment increase occurred in the first year of the zone lifetimes. Increase of 24% in number of establishments, of which two-thirds transferred from other locations. High cost per job created: EUR 11 000 to EUR 73 000 EUR per job in current prices (approximately EUR 13 000 to EUR 83 000 in 2016 prices).	✓	✓	✗	✓	✓
Rich and Stoker (2010)	USA: 6 urban Empowerment Zones	✓	Modest increases in zone employment in five of six cities and reductions in zone unemployment in three of six cities, but impacts not statistically significant.	✓	✓	✓	✓	✓
Rogers and Tao (2004)	USA: Florida, 9 zones in small cities	✗	No statistically significant impacts on population, property values, household income or unemployed-to-population ratio.	✓	✓	✗	✗	✓
Rubin (1990)	USA: 10 New Jersey zones	✓	Employment increased by 5% over two years. Cost per new job USD 3 200 in current prices (approximately USD 5 800 in 2016 prices). Tax benefit to cost ratio 1.9:1.	No control group		✓	✓	✓

Rubin & Wilder (1989)	USA: Evansville Zone, Indiana	✔	Employment increase of 36% over three years. Average cost per new job was USD 4 117 USD over 3 years or 1 372 USD per year in current prices (approximately USD 7 900 over 3 years or USD 2 600 per year in 2016 prices). Job creation greater and cost per job lower in services than manufacturing.	✔	✔	✔	✔	✔
Sridhar (2000)	USA: 322 Ohio zones	✔	Unemployment reduced by 2.9 percentage points in the first year of operation. Unemployment impact appears to reduce in later years of the zones.	✔	✔	✗	✔	✔

Note: [1] These are broad characterisations of the approaches aimed at highlighting key issues for obtaining high quality data. It is sometimes difficult to categorise the studies but the authors have made a judgement based on the information in the relevant publication. The characterisations provided do not fully capture the extent of any mismatches (differences between minimal mismatches or major mismatches) where a mismatch is signalled. They also do not allow for the use of statistical techniques to mitigate any mismatches (e.g. difference-in-differencing). A number of studies use more than one approach, which also complicates classification. We then seek to refer to the more accurate method in the table.

Aspects of control group classification: Techniques for achieving similarity between the control group and treatment group include selecting control areas through propensity scoring, using areas that applied for but did not receive zone status, and using near neighbours. However, use of near neighbours as controls can contaminate the controls through spillovers.

Aspects of treatment group classification: The treatment group geographical areas are classed as exact if they exclude any parts of census tracts, ZIP codes etc., that are not in the zone (even if not all the zone area is included), the time periods are classed as exact if the treatment data exclude any years outside of zone lifetimes (even if excluding some years in which the zone programme was applied), and the sector match classed as exact if the data cover all the types of establishments treated (for example they are not limited to manufacturing when other sectors are affected by the treatment).

Further detail on the methods used by each study is given in Table 4.

Table 3 : Variations in key features of programme designs and contexts assessed by different evaluations

Study	Key outcome variables of the evaluation	Variety of programme design types examined	Design features of evaluated zones		Contextual features of evaluated zones	
			Employment tying of incentives	Designation criteria	National labour market trend	Geographical context
Beck (2001)	Workplace employment Number of establishments	Multiple	Included	Economic potential included	Undefined (period varies by zone)	Mixed
Billings (2009)	Workplace employment Number of establishments	Single	Included	Distress-only	Weak (1990-2000)	Urban-only
Boarnet & Bogart (1996)	Resident employment Property values	Single	Included	Economic potential included	Healthy (1982-1990)	Urban-only
Bondonio & Engberg (2000)	Workplace employment	Multiple	Included	Economic potential included	Weak (1984-1994)	Mixed
Bondonio & Greenbaum (2007)	Workplace employment Capital expenditure Sales Wage rates	Multiple	Included	Economic potential included	Weak (1982-1992)	Urban-only
Busso, Gregory and Kline (2013)	Workplace employment Resident employment Commuter employment Wages Housing rents and prices	Single	Included	Distress-only	Healthy (1994-2000)	Urban-only

Couch et al (2005)	Workplace employment	Single	Included	Distress-only	Healthy (1984-1989)	Mixed
Dowall (1996)	Workplace employment	Single	Included	Economic potential included	Healthy (1986-1990)	Mixed
Elvery (2009)	Resident employment	Multiple	Included	Economic potential included	Weak (1987-1990)	Urban only
Erickson & Friedman (1990a, 1990b, 1991)	Workplace employment Capital investment Number of establishments	Multiple	Included	Economic potential included	Weak (1985-1987)	Mixed
Freedman (2012)	Resident employment Workplace employment House values	Single	Included	Distress-only	Weak (2002-2009)	Mixed
Givord et al (2013)	Number of establishments Workplace employment Wage rates Financial strength of establishments	Single	No tying	Distress-only	Weak (2004-2007)	Urban-only
Gobillon et al (2010)	Rate of exit from unemploy-ment to a job	Single	No tying	Distress-only	Weak (1993-2003)	Urban-only

Greenbaum & Engberg (2000)	Income Employment Housing market	Multiple	Included	Economic potential included	Healthy (1980-1990)	Urban-only
Greenbaum & Engberg (2004)	Employees in manufacturing establishments Number of manufacturing establishments Turnover Wage rates Capital expenditure	Multiple	Included	Economic potential included	Weak (1984-1993)	Urban-only
Ham et al (2011)	Unemployment Resident employment Poverty rate Fraction of households with wages	Multiple	Included	Economic potential included	Healthy (1994-2000)	Mixed
Hanson (2009)	Resident employment Resident poverty Property values	Single	Included	Distress-only	Healthy (1994-2000)	Urban-only
Hanson and Rohlin (2011, 2013)	Workplace employment New establishment entry	Single	Included	Distress-only	Healthy 1994-2000	Urban only
Lambert & Coomes (2001)	Workplace employment	Single	No tying	Distress-only	Healthy (1980-1990)	Urban only

Lynch & Zax (2011)	Wage rates in zone establishments Employment in zone establishments	Single	Included	Economic potential included	Weak (1990-2000)	Mixed
Mayer, Mayneris & Py (2003)	Probability of an establishment locating in a zone Workplace employment Wage rates	Single	No tying	Distress-only	Weak (2004-2007)	Urban-only
Moore (2003)	Number of establishments Workplace employment Industry and size class composition	Single	Included	Distress-only	Weak (1987-1991)	Mixed
Neumark & Kolko (2010)	Workplace employment Number of establishments Industry composition of employment	Single	Included	Economic potential included	Healthy (1992-2004)	Mixed
Oakley & Tsao (2006)	Resident unemployment Household income Poverty	Single	Included	Distress-only	Healthy (1994-2000)	Urban only
O'Keefe (2004)	Workplace employment Earnings Number of establishments	Single	Included	Economic potential included	Healthy (1992-1999)	Mixed
PACEC (1987)	Workplace employment Number of establishments	Single	No tying	Distress-only	Weak (1981-1986)	Mixed

PACEC (1995) Potter & Moore (2000)	Workplace employment Number of establishments Property development	Single	No tying	Distress-only	Weak (1981-1994)	Mixed
Papke (1993, 1994)	Resident unemployment Capital equipment and inventories	Single	No tying	Economic potential included	Healthy (1983-1988)	Urban-only
Rathelot and Sillard (2008)	Workplace employment Number of establishments	Single	No tying	Distress-only	Weak (2004-2006)	Urban-only
Rich and Stoker (2010)	Resident employment Unemployment Poverty Housing value Business lending	Single	Included	Distress-only	Weak (1996-2004)	Urban-only
Rogers and Tao (2004)	Population Unemployed in population Median property value Median household income	Single	Included	Economic potential included	Healthy (1980-1990)	Mixed
Rubin (1990)	Workplace employment	Single	Included	Distress-only	Healthy (1983-1988)	Urban-only

Rubin & Wilder (1989)	Workplace employment	Single	No tying	Economic potential included	Healthy (1983-1986)	Urban-only
Sridhar (2000)	Unemployment	Single	No tying	Economic potential included	Healthy (1982-1990)	Mixed

Note: The evaluation is reported as including employment-tying if any incentives in the evaluated programmes were conditional on new recruitment or another positive employment outcome. Evaluations were characterised as focusing on distress if areas were principally selected for designation because of high unemployment and poverty. The national labour market tendency is characterised as healthy if the national unemployment rate reduced by at least two percentage points over the period of the study; otherwise it is characterised as weak. Evaluations are characterised as urban-only if all the evaluated zones were in urban areas and mixed if they included both urban and rural zones.

Table 4: Summary of key methodological and data features of enterprise zone evaluation studies

Study	Basic approach		Treatment data characteristics	Control group specification	Description of method
	Beneficiary firm survey	Employment trends comparison			
Beck (2001)		✓	Precise zone area. Precise time period from zone designation to evaluation.	Neighbouring areas: counties surrounding zones.	Employment growth comparison of the zones and surrounding counties over the lifetime of the zone.
Billings (2009)		✓	Precise zone area. Annual.	Neighbouring areas: establishments just inside the zone border matched with establishments in areas just outside the border with similar numbers of establishments or establishment death rates.	Difference-in-differences in employment growth comparing establishments inside and outside of the zones accounting for region and establishment characteristics and time.
Boarnet & Bogart (1996)		✓	Combine zone and surroundings (municipalities). Annual.	Distant-and-similar areas: municipalities containing zones matched with qualifying or applicant municipalities without zones.	Panel regression of municipal employment change against zone status with instrumental variables to account for endogeneity of zone designation.
Bondonio and Engberg (2000)		✓	Combine zone and surroundings (zip codes). Annual.	Distant-and-similar areas: zip codes containing zones or parts of zones compared with zip codes with no zone coverage with matched zone designation propensity scores.	Panel regression of employment change against zone status controlling for area-specific fixed effects and growth rates, plus a panel regression controlling for designation probability for each area.

Bondonio & Greenbaum (2007)		✓	Combine zone and surroundings (zip codes). 5-year interval. Manufacturing only.	Distant-and-similar areas: non-zone areas with matched zone designation propensity scores.	Panel data for manufacturing establishments with at least five employees for eleven states. Fixed effects model regressing employment growth rate against zone status and aspects of policy design. Propensity scores included in the growth rate regression.
Busso, Gregory and Kline (2013)		✓	Precise zone area. 10-year interval.	Distant-and-similar areas: rejected and future zone census tracts in other cities.	Difference-in-differences of outcomes between treated and untreated tracts weighted by propensity scores on zone designation.
Couch et al (2005)		✓	Combine zone and surroundings (counties). Annual. Manufacturing only.	Both neighbouring and distant areas: all counties in state without a zone	Regression analysis on impact of zone designation on share of new jobs in all manufacturing jobs.
Dowall (1996)		✓	Combine zone and surroundings (zip codes). Annual.	Both neighbouring and distant areas, covering the entire county.	Shift-share analysis identifying change in zone employment attributable to county-wide growth, industry mix and residual zone impact.
Elvery (2009)		✓	Combine zone and surroundings (census tracts). Annual.	Distant-and-similar areas: zone and non-zone areas with matched zone designation propensity scores, excluding non-zone areas bordering on a zone.	The neighbourhood component of employment growth is compared between zones and matched non-zone areas, after controlling for pre-zone characteristics of residents.
Erickson & Friedman (1990a, 1990b, 1991)	✓		Precise zone area. Annual.	No control group.	Postal survey of local zone co-ordinators identifying employment associated with new establishments, expanded establishments and closures prevented.

Freedman (2012)		✓	Precise zone area. Annual.	Distant-and-similar areas: non-zone census tracts in the state with similar poverty rates.	Regression discontinuity analysis comparing employment change in census tracts just above and below the 20% poverty threshold for zone designation controlling for demography and housing influences.
Givord et al (2013)		✓	Precise zone area. Annual.	Distant-and-similar areas: designated urban development priority areas that were not allocated a zone, with matched propensity scores on probability of zone designation and distance weighting.	Difference-in-difference estimation of outcome impacts. Spillovers assessed by comparing 300 metre ring around zones with similar rings around non-zone urban development priority areas.
Gobillon et al (2010)		✓	Combine zone and surroundings (municipalities). Monthly.	Distant-and-similar areas: muncipalities hosting a zone compared with municipalities without zones with similar propensity scores on probability of zone designation.	Difference-in-difference estimation of muncipality unemployment duration, controlling for the characteristics of individuals.
Greenbaum & Engberg (2000)		✓	Combine zone and surroundings (amalgamations of ZIP codes). 10-year interval.	Both neighbouring and distant-and-similar areas: matched sample based on matched zone designation propensity scores.	Difference-in-difference estimates of employment change.
Greenbaum & Engberg (2004)		✓	Combine zone and surroundings (amalgamations of ZIP codes). Annual. Manufacturing only.	Both neighbouring and distant-and-similar areas: matched sample groups based on propensity scoring.	Difference-in-difference estimates of employment change before and after zone designation.
Ham et al (2011)		✓	Combine zone and surroundings (census tracts). 10-year interval.	Both neighbouring and distant areas: zone census tracts compared with nearest census tract, average of contiguous census tracts and average of all non-zone census tracts.	Difference-in-difference estimates on the difference between 1980-1990 growth rates and 1990-2000 growth rates for areas that became zones and areas that did not become zones in the second period.

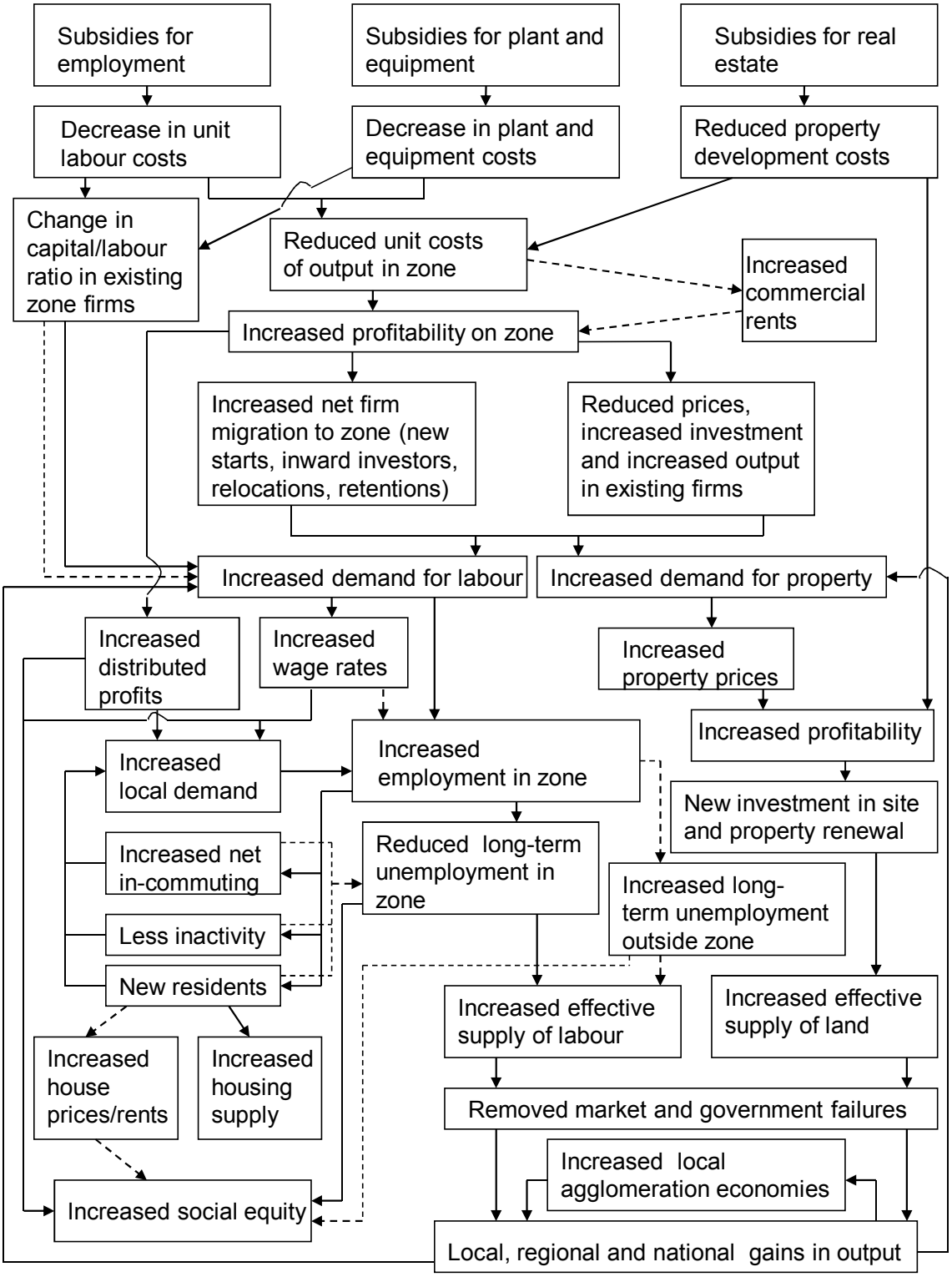
Hanson (2009)		✓	Precise zone area. 10-year interval.	Both neighbouring and distant-and-similar areas: rejected zone applicant areas.	Differencing of resident employment change 1990-2000 for zone tracts and surrounding cities in designated zones and rejected zones, with an instrumental variable to allow for selection effects.
Hanson and Rohlin (2011, 2013)		✓	Precise zone area. Trend data spans three years.	Both neighbouring and distant-and-similar areas: rejected zone applicants	Differencing of establishment employment change 1994-2000 for zone tracts and surrounding cities in designated zones and rejected zones, with an instrumental variable to allow for selection effects.
Lambert & Coomes (2001)		✓	Precise zone area. 10-year interval.	Both neighbouring and distant areas: zones compared with the county as a whole and two non-zone subcounty areas, one of which was contiguous.	Shift-share analysis controlling for industrial structure.
Lynch & Zax (2011)		✓	Precise zone area. 10-year interval. Excludes establishments moving on or off zones.	Distant-and similar areas: Matched non-zone establishments throughout the state.	Heckit and Tobit regressions on employment and wage rates in zone and non-zone establishments in 2000 (when the new policy equilibrium had time to come into effect), controlling for establishment sector, 1990 size, 1990 wage, independence, and county characteristics.

Mayer, Mayneris and Py (2017)		✓	Combine zone and surroundings ('ilot', i.e. very small census block). Annual.	Neighbouring-and-similar areas: rest of the municipality not hosting zones. Distant and similar areas: round two zones compared with future round three zones.	Comparison of trends 2002-2007 before and during policy application 2004-2007. Poisson regression comparing establishment flows in municipalities hosting second-round zones with municipalities to host third-round zones to identify municipality-level impact. Intra-municipality impact estimated with a Logit regression on probability of establishments locating in a zone or non-zone census block in a zone-hosting municipality. Difference-in-difference regression with non-zone areas and future zone areas on wage rate and employment change.
Moore (2003)		✓	Combine zone and surroundings (zip codes). Annual.	Distant-and-similar areas: zones established in 1987 compared with zones designated in 1991-92.	Two-way fixed effects ordinary least squares regression on change in number of firms 1987-91with dummies for zone status and year. Estimation of correlation between firm numbers and employment.
Neumark & Kolko (2010)		✓	Precise zone area. Annual.	Neighbouring-and-similar areas: control groups are a narrow buffer just outside the zone and areas later added to zones.	Difference-in-difference estimates of employment change in zone and non-zone areas, controlling for non-policy influences on performance.
Oakley and Tsao (2006)		✓	Precise zone area (census tracts match zone boundaries). 10-year interval.	Distant-and-similar areas: each zone census tract is matched to the non-zone census tract in the same city with the closest zone designation propensity scores.	Independent t-test of mean 1990-2000 change in employment in census and non-census tracts in each city and pooled regression across 4 zones of unemployment change 1990-2000 in matched zone and non-zone census tracts.

O'Keefe (2004)		✓	Combine zone and surroundings (census tracts). Annual.	Distant-and-similar areas: zones are matched to the non-zone areas with the closest zone designation propensity scores in the same county.	Regression of employment change in zone and matched non-zone areas, controlling for area fixed effects and with separate variables for zones with greater and less than 7 years life.
PACEC (1987)	✓		Precise zone area. One-off survey. Mid-term in zone lifetime.	No control group.	'On-zone' firm managers estimated how zone policy had affected their size and location.
PACEC (1995) Potter and Moore (2000)	✓		Precise zone area. One-off survey. End of zone lifetime.	No control group.	'On-zone' firm managers estimated how zone policy had affected their size, location and start-up decisions. Displacement, linkage, multiplier analysis used to estimate total local economy effects.
Papke (1993, 1994)		✓	Combine zone and surroundings (unemployment claims offices covering a city). Annual.	Distant areas: zones compared with randomly selected urban non-zones of comparable size within the state.	Difference-in-differences of employment change controlling for fixed and random effects.
Rathelot and Sillard (2008)		✓	Combine zone and surroundings (census tracts). Annual.	Distant-and-similar areas: zones compared with non-zone areas in surrounding urban policy target zone with matched zone designation propensity scores.	Differences-in-differences comparison of employment growth.
Rich and Stoker (2010)		✓	Precise zone area. Data for three years.	Both neighbouring and distant-and-similar areas: matched pairs of zones and eligible tracts within the city with matched zone designation propensity scores.	Treatment effect calculated as sum of change in zone area minus change in control area divided by number of treatment areas. Bootstrapping yielded a sample distribution for statistical significance estimation.

Rogers and Tao (2004)		✓	Combine zone and surroundings (census tracts). 10-year interval.	Distant-and-similar areas: small cities that qualified for zone status but did not apply.	Compared log mean change in response variable for treatment and control group and regression including political and economic controls. Does not allow for self-selection bias in decision to apply.
Rubin (1990)	✓		Precise zone area. One-off survey.	No control group.	Zone firms estimated how policy had affected their location and expansion decisions. Input-output analysis used to estimate linkage and multiplier effects.
Rubin & Wilder (1989)		✓	Precise zone area. Annual.	Both neighbouring and distant areas: metropolitan area as a whole.	Shift-share analysis identifying change in zone employment attributable to metropolitan area growth, industry mix and residual zone impact.
Sridhar (2000)		✓	Combine zone and surroundings (census tracts). Data for one year only.	Both neighbouring and distant areas: zone tracts compared with non-zone tracts in the state.	Two-stage least squares regression of unemployment change against predicted zone status controlling for socio-economic factors.

Figure 1: A theoretical framework for investigating local employment and growth effects of enterprise zones



Note: A dashed line indicates a possible negative impact channel.