CHAPTER 18

Place-Based Policies

David Neumark*, **Helen Simpson**†
*UCI, NBER, and IZA, Irvine, CA, USA
†University of Bristol, CMPO, OUCBT and CEPR, Bristol, UK

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Abstract

Place-based policies commonly target underperforming areas, such as deteriorating downtown business districts and disadvantaged regions. Principal examples include enterprise zones, European Union Structural Funds, and industrial cluster policies. Place-based policies are rationalized by various hypotheses in urban and labor economics, such as agglomeration economies and spatial mismatch—hypotheses that entail market failures and often predict overlap between poor economic performance and disadvantaged residents. The evidence on enterprise zones is very mixed. We need to know more about what features of enterprise zone policies make them more effective or less effective, who gains and who loses from these policies, and how we can reconcile the existing findings. Some evidence points to positive benefits of infrastructure expenditure and also investment in higher education and university research—likely because of the public-goods nature of these policies. However, to better guide policy, we need to know more about what policies create self-sustaining longer run gains.

Keywords

Place-based policies, Employment, Enterprise zones, Discretionary grants, Higher education, Industrial clusters, Infrastructure

JEL Classification Codes

R12, R38, J68, H25

18.1. INTRODUCTION

Broadly speaking, place-based policies refer to government efforts to enhance the economic performance of an area within its jurisdiction, typically in the form of more job opportunities and higher wages. Best known, perhaps, are place-based policies that target underperforming areas, such as deteriorating downtown business districts or, within the European Union, relatively disadvantaged areas eligible for regional development aid. Alternatively, place-based policies may seek to enhance even further the economic performance of areas that are already doing well.

Ladd (1994) distinguished a subset of place-based policies or strategies that she labeled "place-based people strategies." These are policies that are geographically targeted, but with the intent and structure of helping disadvantaged residents in them—for example, enterprise zone programs that seek to create jobs in or near areas where poor people live and job prospects are weak. In contrast, some place-based policies target areas irrespective of whether there are disadvantaged people living in those areas, or even many people at all, such as efforts to revitalize a downtown business district including real-estate development or initiatives to help strengthen an industrial cluster in a region.

Place-based people strategies, in particular, can be contrasted with "people-based" policies that try to help the disadvantaged without regard to where they live or how concentrated they are. Examples include welfare and working tax credits (such as the earned income tax credit in the United States). People-based policies are the more traditional purview of public finance and are not covered in this chapter. Rather, the chapter focuses on a wide range of place-based policies—including pure place-based policies and place-based people policies.

Place-based policies that also focus on people can be categorized as direct or indirect. Direct forms of place-based policies seek to increase economic activity and strengthen labor markets where disadvantaged people currently live, while indirect policies may instead seek to increase access of those people to locations where labor markets are stronger. Enterprise zones can be viewed as direct, since they typically create incentives for hiring, or economic activity more generally, in or near areas where disadvantaged people live. The Gautreaux Project and Moving to Opportunity program in the United States, as well as transportation-based policies intended to increase access to jobs outside of areas where the disadvantaged tend to reside (in the United States, the urban core)—that is, intended to reduce spatial mismatch—are examples of indirect policies. However, this chapter focuses on direct policies.[1]

Place-based policies targeting the disadvantaged, including indirect policies, are often rationalized in part by hypotheses that seek to explain the overlap between areas with poor economic performance and disadvantaged residents, coupled with market failures of one form or another. The standard arguments considered in the urban economics literature to rationalize pure place-based policies are generally efficiency arguments pertaining to the existence of agglomeration externalities. But this literature also calls into question whether policies that aim to stimulate economic activity in one place rather than another deliver any aggregate benefits and whether place-based people policies will ultimately help those individuals they target.

In our view, other market imperfections that have been highlighted in the labor economics literature may also justify place-based policies of both types. One is the spatial mismatch hypothesis, wherein minorities or low-skilled workers in some urban areas may face long-term disadvantage spurred by declines in employment opportunities as manufacturing jobs left the cities, coupled with housing discrimination or other constraints that restrict their mobility to locations with better employment opportunities. A second is positive externalities stemming from network effects, whereby employment

[1] There are many excellent summaries of the details of both the Gautreaux Project and MTO program designs, and there are a number of comprehensive reviews of findings of studies of either or both programs; see, e.g., Duncan and Zuberi (2006), Rosenbaum and Zuberi (2010), and Ludwig et al. (2013).

of residents can help other residents find jobs (e.g., Hellerstein et al., 2011). Either the externalities from network effects or the mobility constraints implied by spatial mismatch can potentially justify geographically targeted policies to increase employment. This chapter reviews evidence on these labor-market hypotheses that can potentially rationalize place-based policies, with a more cursory discussion of the standard urban economics hypotheses regarding agglomeration and spillovers, on which plenty of work already exists.

The majority of the chapter focuses on the research evidence on impacts of place-based policies and discusses issues arising in the empirical identification of causal effects in this setting.[2] In the remainder of this section, we provide more details on the types of place-based policies we consider and emphasize the intended recipients and the stated goals of these policies. Later in the chapter, in both the context of the theoretical basis for these interventions and the evidence on their effects, we consider whether these goals are met. Due to space constraints, we limit our coverage throughout to place-based policies in the United States and in Europe. This focus allows us to contrast evidence on similar types of policies implemented in both locations and, where the evaluation literature has examined comparable outcomes using similar empirical approaches, enables us to draw conclusions that are more general. In turn, this means that we necessarily exclude interventions in developing countries, such as Special Economic Zones in China (see Alder et al., 2013, and Wang, 2013) and India's National Investment and Manufacturing Zones.

We also exclude policies that result from political or fiscal decentralization and that apply across whole jurisdictions (and, therefore, without regard to the characteristics of the areas where the incentives apply or the people who live in them), rather than to areas *within* a jurisdiction. Examples include discretionary programs, such as the Michigan Economic Growth Authority (MEGA), which provides tax credits to businesses in the state's export industries (Bartik and Erickcek, 2010), and broader policies on which jurisdictions may compete to attract businesses. There is a large literature on tax competition between areas to attract firms—such as through research and development tax credits (e.g., Wilson, 2009; Chang, 2013), covered by Brülhart et al. (2015). And states and cities are often viewed as competing on a number of dimensions including taxes, regulations, and quality of life, which are often captured and summarized in business climate indexes (Kolko et al., 2013). Bartik (2003) also discussed the potential role of customized economic development services for businesses. Because these kinds of policies and dimensions of competition fall outside of the usual definition of place-based policies that

[2] Kline and Moretti (2014a) provided a very useful complementary review article on place-based policies that focuses largely on a theoretical discussion of the welfare economics of local economic development programs, with a very limited discussion of the evidence. In contrast, our goal is to provide a comprehensive overview and evaluation of the evidence base.

try to reallocate economic activity across areas within a jurisdiction or stimulate activity in very specific areas within a jurisdiction, they are not covered in this chapter.

The place-based policy that has attracted the most attention from researchers is enterprise zones. In the United States, these exist at both the federal and state levels.[3] For example, under the federal Empowerment Zone Program in the United States, authorized in 1993, local governments could submit proposals for zones made up of relatively poor, high-unemployment Census tracts.[4] The federal Enterprise Community program, also authorized in 1993, had the same criteria. Far more Enterprise Communities than Empowerment Zones were created. The former had much smaller benefits—grants of just under $3 million versus $100 million ($40 million) for urban (rural) Empowerment Zones (US Government Accountability Office, 2006)—and much less generous hiring credits.[5] Spending through 2000 in the first round of the federal enterprise zone program totaled nearly $400 million in block grants and $200 million in employment credits. Federal expenditures via hiring credits and block grants for the first 6 years of the program are estimated at about $850 per zone resident.

There is a plethora of state enterprise zones programs in the United States—40 as of 2008 (Ham et al., 2011). These vary in size (some even cover the entire state!), the number of zones in each state, and the benefits available. As an example of targeting, however, consider the case of California, whose state enterprise zone program has been studied most extensively. In California, enterprise zones are supposed to be areas with job creation potential that are near and can overlap with Targeted Employment Areas (TEAs), consisting of Census tracts where more than half the population earns less than 80% of median area income, according to the 1980 Census.[6] The most significant benefit is a hiring credit to businesses located in zones. A worker living in a TEA qualifies for the hiring credit regardless of their characteristics. Clearly, both federal enterprise zone programs and this state program (and the same is true of many others) target areas based on the characteristics of people who live in them.

[3] Bartik (2003) noted that earlier related programs focusing on distressed communities include "Urban Renewal" in the 1940s and 1950s, "Model Cities" during the War on Poverty, and Community Development Block Grants.

[4] All tracts in the zone had to have poverty rates above 20%, with at least 90% of tracts above 25%, and 50% of tracts above 35%. In addition, unemployment rates in each tract had to exceed the 1990 national average of 6.3% (US Government Accountability Office, 2006; Busso et al., 2013).

[5] The Enterprise Communities were created among applicant areas that did not receive Empowerment Zone designation, leading Busso et al. (2013) to characterize the Enterprise Communities as "consolation prizes." The rejected status of these areas figures prominently in research discussed later. In 2000, an additional program (Renewal Communities), with related but different criteria, was established, offering a hiring credit and other benefits. See http://portal.hud.gov/hudportal/documents/huddoc?id=19132_actof2000.pdf (viewed 11 July 2013).

[6] Other studies describe similar types of criteria for federal programs (e.g., Hanson, 2009) and programs in other states (e.g., Lynch and Zax, 2011).

Enterprise zone policies are also used in some European countries. France introduced an enterprise zone program in 1997 (Zones Franches Urbaines (ZFUs)), targeting municipalities or groups of municipalities facing acute unemployment, as well as high poverty and other economic challenges. The criteria used to define these areas included population, population aged under 25, unemployment rate, fraction of the population with no skills, and the fiscal potential of the area, which is related to income (Gobillon et al., 2012). The policy offered relief on property taxes, corporate income taxes, and wages and aimed to increase local employment by making the wage tax relief conditional on hiring at least 20% of employees locally. The United Kingdom ran a program of enterprise zones from 1981 to the mid-1990s covering areas of derelict industrial land in locations that had been hit by industrial decline (Papke, 1993) and that aimed to create local jobs through new businesses and inward foreign direct investment. The policy offered incentives for business investment including more generous tax allowances for capital investment, exemptions from business rates (a local tax on commercial property), and relaxation of planning regulations.[7]

A quite different type of place-based policy that also targets economically disadvantaged areas is a larger scale government effort to help economic development through infrastructure investment. A prime example is the Tennessee Valley Authority, a federal initiative to modernize the economy of the Tennessee Valley Region, encompassing most of Tennessee and parts of Kentucky, Alabama, and Mississippi. The program entailed large public infrastructure spending with an emphasis on hydroelectric dams to generate power sold locally to encourage manufacturing and other spending on, for example, schools and flood controls (Kline and Moretti, 2014b). Another example is the Appalachian Regional Commission (discussed by Glaeser and Gottlieb, 2008) that provided assistance focused on transportation for a large swath of states extending from Mississippi to New York, beginning in 1963.

Within the European Union (EU), Structural Funds—comprising the European Regional Development Fund (ERDF) and European Social Fund (ESF)—support a wide range of initiatives aimed at economic development and increasing labor-market participation and skills; these policies also generally target disadvantaged areas.[8] Expenditure under the ERDF can include investment in transport or telecommunications infrastructure or investment linked to innovation, the environment, or energy. The ESF is used to provide funding for programs aimed at reducing unemployment, increasing human capital, and increasing social integration of disadvantaged groups. The bulk of Structural Funds expenditure flows to the so-called Objective 1 areas. These are regions within

[7] During the 1980s, Spain implemented a reindustrialization zone policy and Belgium a program of employment zones. France also operated an earlier enterprise zone policy in the late 1980s.

[8] For 2007–2013, expenditure on Structural Funds was 278 billion euros, a significant fraction of the European Community budget (see http://europa.eu/legislation_summaries/glossary/structural_cohesion_fund_en.htm, viewed 6 January 2014).

EU member states with GDP per capita less than 75% of the European Community average. For 2007–2013, many new member countries such as Poland and Romania were entirely classified as Objective 1 areas. Other examples include peripheral regions such as in southern Italy, southern Spain, and Portugal and some lagging regions in the United Kingdom and (former East) Germany.

Under EU legislation, European governments can also offer subsidies to private-sector firms within these areas. Since the 1970s, the United Kingdom has run a number of discretionary grant schemes (e.g., Regional Selective Assistance, Regional Development Grants, and Enterprise Grants) that subsidize new capital investment with explicit aims of creating or safeguarding jobs and attracting inward investment. The grants are available in designated, relatively disadvantaged "Assisted Areas" within the United Kingdom, with area eligibility determined by GDP per capita and unemployment rate indictors relative to the EU average. The subsidy rate allowable varies with area characteristics, with Objective 1 areas eligible for the highest subsidy rates.[9] A similar grant program has been in operation in France (the Prime d'Aménagement du Territoire), and the Italian government operates a scheme known as Law 488. Although on paper the direct recipients of these subsidies are businesses, the ultimate intended beneficiaries are individuals residing in these lagging regions; hence, these programs have a people-based flavor.

There are other European place-based policies, directly aimed at firms, which do not necessarily have a people-based component: for example, support for industrial clusters outside of relatively deprived areas. The current UK enterprise zone policy, which began in 2011, aims to increase new business start-ups and growth and to create new jobs. Within England, there are now 24 designated areas not only offering some of the same tax incentives as the previous scheme but also aiming to promote clusters of businesses within the same industrial sector and emphasizing location-specific amenities including access to transport infrastructure such as rail and ports. The motivation for cluster policies often comes from evidence on productivity benefits arising from industry localization or on the observed colocation of some high-tech clusters with higher education institutions. In Sweden, the government has explicitly tried to use the location of new universities as a regional policy tool to both increase local labor force skills and potentially exploit knowledge spillovers from university research as a means of attracting private-sector activity to an area and boosting local productivity.

As this discussion suggests, there is a large variety of policies that can be considered under the general rubric of place-based policies. Table 18.1 provides summary information on the general types of place-based policies that exist, as well as some details on specific examples. Some have been mentioned already, and others will be discussed in the sections that follow.

[9] Eligible areas are revised every 7 years. The precise set of economic indicators and geographic units used to define eligible areas have varied over time.

Table 18.1 Place-based policies

Type of policy	Enterprise zone	Business development, attraction, and retention	Cluster promotion	Infrastructure investment		Discretionary grants	Community development and locally led initiatives
Specific examples	California enterprise zone program; US Federal Empowerment Zones; US Federal Enterprise Communities; French enterprise zones	UK enterprise zones (2011)	French Local Productive Systems; Bavarian High-Tech Offensive	Tennessee Valley Authority; Appalachian Regional Commission	EU Structural Funds: European Regional Development Fund (ERDF), European Social Fund (ESF)	UK Regional Selective Assistance; Italian Law 488	Low-Income Housing Tax Credit; redevelopment areas; New Markets Tax Credit
Policy goals	Job creation	New business creation; job creation; industry clustering	Increase collaboration and cooperation between firms, and between firms and public-sector research institutions	Economic modernization	ERDF: economic development ESF: increased labor-market participation	Job creation and safeguarding; inward investment	Affordable housing; urban redevelopment; economic development
Targeting	Areas with higher concentrations of poverty, unemployment	New businesses within government-designated areas	France: no restriction on local areas that could participate Bavaria: whole state	Poor areas of region	Areas with relatively low GDP per capita/ high unemployment relative to the EU average	Areas with relatively low GDP per capita/high unemployment relative to the EU average	Low-income neighborhoods or low-income housing units
Incentives	Hiring tax credits; corporate and personal income tax credits; sales	Reduced business rates; relaxed planning regulation;	France: subsidy for a project, e.g., to boost exports of	Reduced electricity rates; other infrastructure improvements	ERDF: transport, telecommunications infrastructure, and investment linked to innovation or energy	Subsidy on new investment in physical capital by firms, linked to jobs targets	Tax credits to investors or real-estate developers; tax

	and use tax credits; tax-exempt financing; community block grants; property, corporate, and wage tax relief	enhanced capital allowances in some cases	participating firms Bavaria: access to public research facilities, venture capital funding, and science parks		ESF: training programs		increment financing
Recipients of support	Mainly businesses; sometimes workers; communities	Businesses	France: businesses in a common industry Bavaria: targeted five high-tech sectors	Broad	Broad	Primarily manufacturing businesses	Real-estate developers or other businesses

18.2. THEORETICAL BASIS FOR PLACE-BASED POLICIES

In assessing the welfare effects of place-based policies, theory highlights some important factors, which in turn can be used to direct empirical analysis of policy effects. Key questions include the following: Can policy exploit agglomeration externalities or solve other market failures to generate long-term gains for targeted areas? If so, does intervention come at a cost to other areas, and are there any aggregate national benefits of location-specific interventions? Does policy that targets specific places create distortions to capital and labor mobility, lowering efficiency by reducing incentives of firms or individuals to move to other more productive locations? And how does geographic mobility affect outcomes for those originally resident in the targeted areas, as well as the eventual incidence of a place-based policy? In short, can intervention be justified, and what potential effects of place-based policies should empirical analysis aim to identify?

Before considering potential efficiency rationales for intervention, it is worth starting from the benchmark of the absence of market failures. With perfect labor mobility combined with inelastic housing supply in the targeted area, theory implies that, as a result of in-migration and increased demand for housing, landowners benefit from a location-specific policy, rather than local residents, with the benefits being capitalized into rents. With less than perfect labor mobility, local residents may benefit, but these benefits should be weighed against any costs to nontargeted areas and the deadweight costs of taxation. Place-based policies may be justified in the context of market failures that have a spatial dimension. In the rest of this section, we outline possible arguments why place-based policies may help overcome specific market imperfections or take advantage of externalities and consider the case for redistribution or equity-motivated policies that target disadvantaged areas and not just disadvantaged people.

18.2.1 Agglomeration economies

The efficiency-related argument for place-based policies that is most central to urban economics is that there exist agglomeration economies, through which the dense population of urban areas has an independent effect on the productivity of resources. Agglomeration economies may arise via a number of mechanisms, which Duranton and Puga (2004) categorized as "sharing, matching, and learning." Moretti (2010) emphasized the role of thick labor markets, which can lead to better worker–firm matches, inducing more investment by workers and firms. Thick labor markets can also provide better insurance against local demand shocks by reducing the risk or cost of unemployment, which can act as a compensating differential that lowers labor costs. Moretti also emphasized thick markets for intermediate inputs, especially those that are specialized and nontradable. Examples are professional services, such as computer programming, legal support, and venture capital.[10] If a firm needs these inputs from other companies, it has an incentive to locate in a city with other firms that use the same inputs.

Agglomeration economies imply positive externalities, because bringing additional people or firms to an urban area increases the productivity of other individuals or firms in that area, but these gains are not captured by those deciding whether to move to that location. Thus, there may be a rationale to subsidize in-migration or growth, to raise the private returns closer to the social returns.[11] Moretti (2010) argued that the rationale for place-based policies to exploit agglomeration economies may be quite strong, especially in a dynamic setting with multiple equilibria, in which externalities can generate benefits from drawing economic activity to any single one of a set of ex ante similar locations. In such cases, the gains from moving from a low-employment, low-density equilibrium to a high one at a particular location may far exceed the costs of the policy (such as a temporary, but large-scale intervention as in Kline, 2010). Hence, there may be a case for place-based policy to jump-start growth in a specific area. Nevertheless, two further questions need to be addressed: Which areas should policy target? And will the gains to those areas be offset by losses to others?

As Glaeser and Gottlieb (2008) emphasized, in choosing *between* locations in which to encourage growth, policymakers should do so in areas where the elasticity of productivity with respect to agglomeration is higher (which may well not be the most deprived areas), exploiting spatial variation in the relationship between productivity and size or density. In practice, they argued—given the challenges in estimating how the magnitude of agglomeration economies varies across regions—that policymakers may have little or no knowledge of how this elasticity varies spatially and hence little basis for preferring one place over another. In addition, if there is no variation in the elasticity across areas, then there will be no aggregate benefits from redistributing activity geographically. Moretti, however, suggested that when there is spatial heterogeneity in the value of this externality, competition among local governments can be an efficient source of place-based policies. He argued that when local governments know the value of the local externality and set locally financed incentives based on it, competition to attract businesses may increase national welfare, despite the potential zero-sum game of attracting businesses to one location rather than another. The reason is that this local policy competition may ultimately arrive at the correct valuation of the externality. Of course, there may be reasons for

[10] Zhang (2007) suggested that venture capital, which might be thought of as supplying capital in a national market, actually tends to favor local industry—looking at the specific context of Silicon Valley.

[11] Place-based policies that aim to address a coordination failure and target city size may still only be a second-best response, even in the best-case scenario in which policymakers know the optimal size to capture, for example, the externalities between firms from colocation in the same area. It is more difficult to imagine a policy that fully addresses inefficiencies conditional on location, for example, due to uninternalized increasing returns such as through spillovers, which may depend on the scale or type of the firms' investment.

skepticism about local government motivations and incentives to attract businesses, as other factors—such as the salience of attracting new businesses for winning elections—can easily come into play.

18.2.2 Knowledge spillovers and the knowledge economy

A frequently posited source of agglomeration externalities, which has a long history in urban economics (e.g., Marshall, 1890; Jacobs, 1961), is knowledge spillovers—or learning in the Duranton and Puga (2004) typology. The hypothesis is that densely populated, diverse urban areas foster experimentation and innovation and facilitate face-to-face interactions that aid the spread of new ideas. More generally, the human capital of others in close proximity can raise everyone's human capital and increase firm productivity, through sharing of knowledge and faster adoption of innovations (Moretti, 2010). Because knowledge is more likely to spill over from more highly educated workers, due to the knowledge they possess and perhaps the work they do, knowledge spillovers can have more specific predictions than agglomeration economies per se—in particular, that locations more dense in educated workers will be more successful.[12]

Knowledge spillovers can provide a rationale for local policymakers to try to produce or attract skilled workers—for example, through creating or supporting educational institutions, perhaps in particular universities. The public-good characteristics of basic knowledge rationalize public subsidies to research universities in general, but the potential for local knowledge spillovers can rationalize place-based policy. If spillovers increase with geographic proximity and firms are aware of this, then investment in universities may serve to attract innovative firms to the locality. Local governments may take additional steps to increase knowledge spillovers from publicly financed research, such as the creation of business incubators and science parks near research universities or encouraging interactions between universities and businesses, potentially overcoming information or coordination failures.

In addition to the potential value of generating knowledge spillovers from attracting high-skilled workers, Moretti (2012) argued that attracting skilled workers in the knowledge-intensive high-tech sector has large local multipliers relative to other industries. This can occur because of high pay in these jobs, because of demand for business services from this industry, and because high-tech firms appear to attract other high-tech firms. Moreover, there are gains to earnings of others, according to Moretti, from human capital externalities, faster technology adoption, and complementarity with less-skilled labor.

[12] In that sense, knowledge spillovers are viewed as a particular type of agglomeration externality. But the hypothesis has received enough attention in the literature that it merits separate consideration.

Echoing the discussion of agglomeration economies generally, Glaeser and Gottlieb (2008) raised questions about the aggregate welfare implications of policies to try to exploit knowledge spillovers by encouraging moves of educated workers from one place to another or the creation of educated workers in one place rather than another. The argument is similar: There has to be a nonlinear relationship between the density of skilled workers and productivity, so that moving skilled people from one location to another increases productivity more in the target area than it decreases productivity at the origin, and policymakers must know the nature of this relationship. In addition, worker mobility can dissipate the effects of some local policies to exploit human capital spillovers, such as subsidizing education. This may be particularly problematic for higher education as more educated workers are more mobile.[13]

18.2.3 Industry localization

Many of the arguments about matching, sharing, and learning can be applied and may even be more persuasive at the industry level, since the localization of industry employment has been systematically documented for specific tradable sectors for a number of countries. For example, knowledge spillovers may actually have to do more with the presence of workers in the same or related industries, rather than skilled workers, per se, in the locality. Other sources of agglomeration economies may be stronger within industries, because the thick labor markets or thick intermediate input markets that may be the engine of agglomeration may operate more within than across industries. Such industry-level externalities may rationalize government policy to try to establish or enhance industry clusters.[14]

However, the observation that such industrial clusters exist, and the potential presence of externalities, is not sufficient to justify intervention. For example, for some sectors, access to natural resources rather than the presence of agglomeration economies may drive the location of clusters. Duranton (2011) analyzed the theoretical basis for industrial cluster policies and questioned the magnitude of the returns to clustering for local welfare. His argument that the case for policy is weak rests on the complex nature of the agglomeration externalities and on the costs and benefits of intervention in practice (i.e., on the potential weakness of policy levers if firm and worker mobility is limited and on the evidence of only small-scale effects of clustering on local productivity and wages). Moreover, the optimal size of a cluster, which policy would want to target, is hard to determine in practice and would require knowledge of both the higher benefits

[13] In this chapter, we do not review the evidence on general education subsidies (see, e.g., Bound et al., 2004; Bartik, 2009). One might imagine mobility to be less of a problem for education policies that target provision of skills specific to a local industry, such as at the community-college level.

[14] Bartik (2003) and Matouschek and Robert-Nicoud (2005) discussed market imperfections in investment in training that might occur when firms are clustered, noting that in some cases, government involvement in the provision of (industry-specific) training may be justified.

from increasing returns as the cluster grows and the increased costs due to limits on land availability and externalities such as congestion. On the other hand, if a cluster policy were to be pursued, then competition for plants between localities may be more likely to lead to an improved spatial distribution of industry activity, paralleling the argument in Moretti (2010).

18.2.4 Spatial mismatch

Other types of market imperfections, such as frictions in labor or housing markets resulting in involuntary unemployment and reduced mobility, can sometimes rationalize place-based policies, although these may not serve as the standard motivations. One prominent example is the spatial mismatch hypothesis, which—as applied to the United States—argues that the lower employment rate of disadvantaged minorities in urban cores is in part attributable to there being fewer jobs per worker in these areas (Ihlanfeldt and Sjoquist, 1998, p. 851). This can emerge because of the exit of jobs from these areas with the changing industrial structure (Wilson, 1987) and can persist because of exogenous residential segregation attributable at least in part to discrimination in housing markets.[15]

Issues of residential segregation of minorities, immigrants, and the economically disadvantaged generally in areas with diminished job opportunities are hardly limited to the United States, although we know less about this in other countries. Recent studies point to a link between residential segregation and employment or unemployment in France (Gobillon and Selod, 2007), Belgium (Dujardin et al., 2008), Sweden (Åslund et al., 2006), and the United Kingdom (Fieldhouse, 1999).

The segregation of disadvantaged groups in areas with fewer jobs implies that the wage net of commuting costs for these groups is more likely to be below their reservation wage, so fewer residents of such areas will choose to work, especially among the less-skilled for whom commuting costs represent a larger share of earnings. Customer discrimination against minorities, employer discrimination that deters employers from moving to urban minority areas where wages are lower, and poor information about jobs in other areas (Ihlanfeldt and Sjoquist, 1998) can reinforce the effects of spatial mismatch.

The gist of the spatial mismatch hypothesis is that the mobility usually assumed in urban economics may be restricted; hence, out-of-equilibrium behavior may persist for a long time. This lack of mobility may undermine some of the concerns expressed in the urban economics literature that place-based policies—often motivated by equity concerns (discussed more below)—can be harmful by inducing poor people to remain in poor areas, if they are likely to remain there anyway.

[15] Gobillon et al. (2007) reviewed theoretical models and hypotheses regarding spatial mismatch.

18.2.5 Network effects

Network effects in labor markets may influence the impact of place-based policies. In network models, employment of some residents increases the flow of information about job opportunities to other residents or the flow of information about workers, reducing search costs and increasing employment (e.g., Montgomery, 1991). Networks are likely to have a spatial dimension—for example, connecting neighbors. Hellerstein et al. (2011, 2014) and Bayer et al. (2008) reported evidence suggesting that network connections between coresidents (of the same Census tract or even smaller areas) are important. Residence-based labor-market networks can exacerbate the adverse effects of residential segregation on labor-market outcomes for some groups: for example, when social networks are racially (or ethnically) stratified or stratified based on skills.

Network effects do not conventionally arise in discussions of place-based policies. Part of the motivation for a broader perspective that considers this factor is that it may counter some of the criticisms of place-based policies, such as the arguments (discussed more below) that these policies discourage the migration of the disadvantaged to areas with better economic opportunities and that many of the benefits may go to commuters and new residents who have the skills to take advantage of newly created employment opportunities (Glaeser, 2007).

Coupled with spatial mismatch, network effects may strengthen the case for place-based policies focusing on areas of concentrated disadvantage, because the multipliers that network effects create can amplify the effects of these policies, more so in areas with low employment and perhaps also more so in minority areas where stratification of labor-market networks may imply particularly poor labor-market information.[16] However, even absent the constraints on mobility assumed by the spatial mismatch hypothesis, high concentrations of low-employment areas may help justify policies targeting these areas. For example, one could imagine that in an area with low employment and high crime, utility is not necessarily low enough to induce outward mobility to higher employment, low-crime areas. But that crime surely imposes costs on others, and hence, subsidizing employment of one person to exploit the positive externalities on others' employment (and on crime)—because of networks, for example—can be a prudent policy and more cost-effective in areas with low employment.

This strikes us as a commonsense rationale for place-based policies. Because of crime spillovers between neighborhoods and the location of consumption of urban amenities,

[16] Peer or neighborhood effects can also imply externalities between individuals (see Topa and Zenou, 2015). For example, the presence of unemployed residents might lead other residents to remain unemployed by changing their norms of behavior (Wilson, 1987), and, conversely, creating some employment can have virtuous effects on others. Network effects could also diminish the effects of place-based policies. For example, a policy that leads employers to relocate to an area may do little to boost employment opportunities of local residents if the employees of the relocating companies are not networked to local residents.

many city (and suburban) residents—and not only residents of targeted neighborhoods—may be made better off by policies that increase job opportunities in disadvantaged areas. In addition, if network (or peer or neighborhood) effects are important, it may be efficient to target such policies to areas with large concentrations of unemployed people so that the multipliers from these effects can have a greater impact. Viewed this way, network effects may offer a public good that many can take advantage of when some employment opportunities are created. This dovetails with other arguments that place-based policies can in part be justified by the need to correct the underprovision of public goods in poor areas, often because the tax base is insufficient to provide these goods (Crane and Manville, 2008).

18.2.6 Equity motivations for place-based policies

The equity motivation for many place-based policies is to redistribute jobs and income to places where jobs are scarce and incomes are low. Urban economics teaches us that the success of such policies in redistributing jobs and income is complex. It may seem natural, for example, that a state that is concerned with low job opportunities in a specific urban area would try to spur job creation there by using tax or other incentives—such as enterprise zones. However, mobility of people and capital can complicate the effects and potentially undo most or all of the gains from such redistributive policies.

Moretti (2010) developed this argument in some detail. If we think about an enterprise zone type of policy, the subsidies to employment will result in higher wages unless labor supply is infinitely elastic. If labor is mobile, some workers will move to the subsidized area, and as long as housing supply is not infinitely elastic, housing prices and rents will increase, offsetting at least some of the gains to the original residents.[17] Of course, some people in the targeted areas may own property, and for them, the increase in housing prices is a gain. In the extreme case of perfect mobility of labor, utility of each individual is equated across locations both before and after the policy intervention, and the only effect is on land prices that capitalize the place-based subsidy. However, we probably should not consider landowners as the target population for place-based policies based on equity goals.

Thus, other than unlikely knife-edge cases—like infinitely elastic labor supply that implies no wage increases, infinitely elastic housing supply that implies no change in housing prices, or perfect mobility that undoes all gains from place-based policies—mobility probably will partly but not fully undermine the effects of redistributive place-based policies. Nonetheless, the welfare effects can be other than intended. For example, if we rule out perfect mobility of labor and assume that some people have geographic preferences for location, then it is only the marginal workers for whom utility is

[17] Although policy may or may not require workers to live in the area where the subsidies apply, the subsidies will presumably generate some mobility of people into or near those areas.

equated across locations. However, in this case, who gains from the policy may have little to do with the intended effects. Inframarginal workers in the target area gain and those in the other areas (that are taxed) lose, while marginal workers are unaffected. Depending on who these inframarginal workers are, the redistributive effects in terms of welfare may or may not be what policymakers intended. For example, there may be no good reason to believe that the inframarginal workers in the targeted area are the lowest income individuals.

This echoes a broader concern about the targeting of benefits to the disadvantaged via place-based policies. As Crane and Manville (2008) emphasized, given mobility and land-price responses, the jobs created (if they are created) may go to nonpoor residents or migrants, and the gains from land prices seem unlikely to accrue to the poor. At the same time, they suggest that it may be possible (if somewhat utopian) to create institutional arrangements so that the increase in land values is captured by the public and redistributed, to some extent, to the intended beneficiaries. They refer to "Community Benefits Agreements" that specify, for example, that developers who capture the higher land values devote resources to higher wages, affordable housing, social services, etc.

Glaeser and Gottlieb (2008) raised the issue of whether it makes sense to put incentives in place that encourage poor people to stay in poor areas, rather than migrating to places with better economic opportunities. For example, they said, "it is not clear why the federal government spent over $100 billion after Hurricane Katrina to bring people back to New Orleans, a city that was hardly a beacon of economic opportunity before the storm" (p. 197). This, however, might be an unusual case. If we think, instead, about people living in a poor area who have preferences to stay in that area, then if we could determine that these inframarginal people are the ones we want to help through a place-based policy, one could in principle justify such a policy on equity grounds. Nonetheless, aside from the difficulties of knowing who is and who is not inframarginal, it is not clear that such a policy would be more efficient than subsidizing migration to other areas and perhaps doing more to break down the kinds of barriers to residential mobility emphasized by the spatial mismatch model—if indeed such barriers are important.

18.2.7 Summary and implications for empirical analysis

Two comprehensive reviews of the economics underlying place-based policies, by Moretti (2010) and Glaeser and Gottlieb (2008), disagree to some extent on the efficiency-based rationales for place-based policies, with Moretti taking more a favorable view under some circumstances. In addition, we have suggested some additional efficiency-based arguments that may rationalize place-based policies. But both of these extensive reviews raise serious questions about the equity arguments for place-based policies, with Moretti, for example, concluding that "from the equity point of view, location-based policies aim[ed] at redistributing income from areas with high level of economic activity to areas with low level of economic activity . . . are unlikely to be effective" (Moretti, 2010, p. 1242). When workers are mobile, it may be better to target people rather than places. It is also important to recognize that equity and efficiency goals in place-based policymaking can end up in conflict. For example, Glaeser and Gottlieb (2008) presented some evidence suggesting that the nonlinearities with regard to knowledge spillovers may be convex, so that subsidizing human capital investment (or in-migration) may be most effective where human capital is already high. Such a policy would tend to increase income disparities between areas.

The preceding discussion highlights some lessons for empirical research even if theory cannot fully pin down a single rationale for the existence of place-based policies. A first test of whether a policy results in welfare gains is whether it generates benefits for the targeted area and, in addition, for the targeted residents. The discussion above points out that the effective incidence of a policy can depend on factors such as the degree of in-migration to an area and the degree of slack in local housing markets. Hence, evaluation should look beyond evidence of effects on local employment to evidence on local unemployment and whether local residents have moved into jobs or whether there have been changes in commuting patterns. We also need to look beyond the effects on local wages, to effects on rents and house prices, to better assess impacts on individuals' welfare and, further, whether there are heterogeneous effects according to whether people are homeowners or renters or, more generally, by skill or income level. As discussed in Section 18.4, the fact that policy can affect the location incentives of both firms and workers also has practical implications for evaluation methods and the choice of control areas, since displacement can potentially lead to biased estimates of policy effects as well as being of interest as a policy response in its own right.

Two further points emerge that can help guide empirical work. First, local welfare effects might differ substantially from those at an aggregate level. Exploiting agglomeration externalities in one location might come at the expense of (possibly greater) losses of agglomeration benefits in other areas and distortions to the efficient location of economic activity. Second, any local benefits themselves might not be long-lasting. While theory suggests policy could induce a location to shift to a new higher productivity equilibrium, whether this works in practice or whether areas revert to their previous steady state is an important question.

However, it is questionable—based on our own experience with policymakers—that comprehensive welfare statements or calculations carry significant weight in many if not most policy decisions. Rather, policymakers are more likely to start with a goal such as "bring jobs to Detroit." If we, as urban economists, can simply provide them with rigorous evidence on whether a given policy achieves its stated goal and what other trade-offs—including distributional ones—it entails, we are doing a valuable service and can still help winnow out many policies that do not achieve their goals or have adverse consequences that policymakers do not intend. As a result, most of the rest of this chapter focuses on estimating the causal effects of place-based policies on their targeted outcomes. However, we touch on evidence on the broader effects of such policies where possible and highlight areas where evidence on the wider welfare implications of these interventions is available.

Even ignoring explicit welfare estimates and calculations, however, there is a potentially significant disconnect between the focus of much empirical research (and, we suspect, policymaker attention) on jobs and the importance of effects on wages for delivering welfare gains to residents of places targeted by place-based policies, because a larger employment response can imply greater deadweight loss from distortions in behavior (Busso et al., 2013). As the theoretical discussion earlier noted, it is when labor is immobile—and hence there is less scope for employment increases in targeted areas—that the welfare gains are more likely to accrue to residents (workers, specifically), rather than property owners. This disconnect may, of course, simply reflect the fact that policymakers place a priority on job creation in specific areas. Alternatively, as Kline and Moretti (2014a) pointed out, when there are labor-market frictions that generate spatial heterogeneity in unemployment, place-based policies like hiring subsidies in certain locations can increase employment (lower unemployment) in the targeted area and increase welfare, so the focus on job creation may be better aligned with effects on welfare.

18.3. EVIDENCE ON THEORETICAL MOTIVATIONS AND BEHAVIORAL HYPOTHESES UNDERLYING PLACE-BASED POLICIES

Glaeser and Gottlieb (2008) and Moretti (2010) provided reviews of evidence regarding the conventional urban economics arguments such as agglomeration economies and knowledge spillovers. We outline some recent evidence briefly but focus on new evidence on the other hypotheses that we believe should be considered in the context of place-based policies, including spatial mismatch and network effects.

18.3.1 Evidence on agglomeration economies

There is now a considerable body of evidence in support of the idea that increased density of economic activity both across and within industries generates positive externalities. Rosenthal and Strange (2004) provided a summary of the evidence on the underlying sources of agglomeration economies. A range of papers have sought to estimate the elasticity of productivity with respect to a measure of the density of employment and generally find elasticities ranging from around 0.01 to 0.10 (see Melo et al., 2009, for a meta-analysis of a wide set of findings). Some recent, but quite distinct, contributions in this area are Combes et al. (2010, 2012) and Greenstone et al. (2010).[18]

[18] Much work addresses productivity differences across cities, but some considers agglomeration economies within cities (Rosenthal and Strange, 2003; Fu and Ross, 2013). Some evidence suggests agglomeration economies can attenuate quite rapidly with distance, which is relevant for place-based policies that target small areas.

Combes et al. (2010) carried out a careful analysis addressing identification problems in estimating the relationship between the density of economic activity and productivity. They addressed the issues that a positive relationship between productivity and density may be driven, at least in part, by omitted variables correlated with both the density of employment and productivity, by workers choosing to locate in more productive regions, and by those workers choosing to do so being disproportionately high-skill. Hence, both the quantity and quality of labor are likely to be endogenous. To address the possibility that the quantity of labor may be endogenous, they employed an IV strategy, instrumenting population with historical measures of population density dating back to 1831 and with measures of local geological features including characteristics of the soil and of the terrain, measured by variation in altitude—features that might be expected to have determined where population settlements occurred and how successful they were. To deal with the endogeneity of the quality of labor, they used individual panel data on wages that allowed them to separate location effects from both observed and unobserved worker characteristics. Starting from a benchmark elasticity of around 0.05 between wages and density, they found that controlling for both of these factors led to an estimate of 0.027. In addition, including a measure of market potential (an inverse distance-weighted measure of density across all other areas), to allow for the fact that agglomeration effects may spill across area boundaries, results in their preferred estimate of 0.02, with an elasticity of total factor productivity (TFP) with respect to density of around 0.035.

Combes et al. (2012) examined the extent to which firm selection drives the observed positive relationship between city size and productivity. If competition is increasing in city size, we might expect that low-productivity firms are less likely to survive in larger cities, leading to a positive correlation between city size and average firm productivity, due to greater truncation of the lower tail of the productivity distribution in larger cities. Agglomeration externalities, on the other hand, might be expected to lead to a shift of the observed firm productivity distribution outward as city size increases, as all firms benefit from agglomeration economies, and, if the most productive firms also derive the largest gains, a widening of the distribution at the upper tail.

Their empirical approach estimates the differences in observed firm productivity distributions along these dimensions across more and less dense areas. Their main finding is that selection does not seem to be an important factor in explaining TFP differentials across areas with different employment densities. In addition, they find evidence that firms that are more productive gain more from being in denser environments. Defining denser areas as those with above-median employment density, they find that compared to less dense areas, the productivity gains for firms in the top quartile of the log TFP distribution are approximately 14.4%. In contrast, the gains to firms in the lower quartile from being in denser areas are only 4.8%, implying heterogeneity at the firm level in the degree to which firms might benefit from urbanization externalities. They also find a very similar elasticity of TFP with respect to employment density to Combes et al. (2010), of 0.032.

Greenstone et al. (2010) provided estimates of the magnitude of agglomeration externalities by exploiting a subsidy policy aimed at attracting very large new plants to specific locations in the United States. We discuss the implications of their findings for this category of place-based policy later. Their estimation strategy uses information on runner-up locations as control areas, and their estimates imply that the plant openings resulted in very large productivity spillovers, with TFP in incumbent plants 12% higher than in plants in control areas after 5 years. Of course, as the authors acknowledged, these estimates come from a very specific setting, the opening of a very large new manufacturing plant, for which the winning county may have made the highest bid in anticipation of significant spillover benefits. Effects of this magnitude are therefore not necessarily applicable outside of this policy setting, but are certainly of relevance to the debate about the effects of this type of place-based policy. Greenstone et al. also found evidence of considerable heterogeneity in the magnitude of these externalities both across different locations and across industries. In particular, productivity spillovers are found to be greater in industries that are more similar to the new plant in terms of technologies and human capital requirements, suggesting a role for worker flows between firms and knowledge spillovers (potentially as a result of the former) as sources of agglomeration economies.

Finally, with regard to whether the magnitude of the elasticity of productivity with respect to density varies with the degree of density of economic activity, and hence varies spatially, the evidence described in Section 18.5.5—using an intervention that is perhaps more generalizable (the Tennessee Valley Authority)—does not support the kind of heterogeneity in agglomeration externalities across locations that theory suggests can rationalize place-based policies.

18.3.2 Is there spatial mismatch?

Research testing spatial mismatch in the US context tries to incorporate direct information on access to jobs that is related to either travel time or the extent of jobs (or job growth) nearby (e.g., Ellwood, 1986; Ihlanfeldt and Sjoquist, 1990; Raphael, 1998; Weinberg, 2000). These studies tend to show that blacks live in places with fewer jobs per person and that this lower job access can help explain lower black employment rates, perhaps through the mechanism of blacks facing longer commute times to jobs and hence lower net wages (although Ellwood suggested that the differences may not be large). Evidence of longer commute times for blacks does not necessarily point to spatial mismatch, as simple employment discrimination against blacks can imply fewer job offers and hence on average longer commute times for blacks even if they live in the same places as whites. Overall, two comprehensive reviews argue that there is a good deal of evidence consistent with the spatial mismatch hypothesis (Holzer, 1991; Ihlanfeldt and Sjoquist, 1998), although Jencks and Mayer (1990) provided a more negative assessment of the hypothesis.

Recent work raises questions about the spatial mismatch hypothesis (Hellerstein et al., 2008). In relation to race, the pure spatial mismatch hypothesis implies that it is only the location of jobs, irrespective of whether they are held by blacks or whites (but perhaps conditional on skill), which affects employment prospects. However, if race affects employment—through, for example, discrimination or labor-market networks in which race matters—then even if an area is dense with jobs, black job opportunities may be low. An urban area with large concentrations of black residents, for example, may also be one into which whites tend to commute to work and employers are less likely to hire blacks. In this case, employment problems of low-skilled blacks may not reflect an absence of jobs where they live so much as an absence of jobs available to blacks, which Hellerstein et al. termed as "racial mismatch."

The authors estimated models for employment including measures of job density not only by location and skill but also by race, using confidential Census information on place of residence and place of work.[19] The evidence is far more consistent with racial mismatch than with simple spatial mismatch. Black job density (the ratio of local jobs held by *blacks* to *black* residents) strongly affects black employment, whereas white job density (the ratio of local jobs held by *whites* to *black* residents) does not. In addition, the own-race relationship is stronger at low skill levels. In a number of specifications, the estimated coefficient on the black job density measure is larger than that of the nonblack or white job density measure by a factor of about 10; the magnitudes are, respectively, about 0.001 and 0.01, with the latter implying that a 10 percentage point increase in black job density raises the employment rate of black men by 1 percentage point. This evidence indicates that for blacks, the spatial distribution of jobs alone is not an important determinant of black urban employment, but rather it is the interaction of the spatial distribution of jobs combined with a racial dimension in hiring, or "racial mismatch," that matters. In other words, even if blacks reside in areas that are dense in jobs (or dense in jobs at their skill level, as other analyses reveal), if whites tend to hold these jobs, the employment of black residents can be quite low. Reflecting on this, descriptive statistics reported in Hellerstein et al. (2008) show that the density of jobs where blacks live is in fact quite high, even at blacks' skill levels, suggesting that what is more important is which group is more likely to get hired. And a simple simulation they report showed that if low-skilled blacks were geographically distributed to live where low-skilled whites lived, the black–white employment rate differential would be only marginally smaller (by 0.025, relative to a gap of 0.231). This is precisely because the effect on black employment of white job density—which is the density that would increase most sharply if blacks lived where

[19] These regressions are not plagued by the classic reflection problem that would arise if individual employment were regressed on the local employment rate, because the numerators of the job density measures include both residents and nonresidents (who work but do not live in the area).

whites lived—is so small.[20] More recent research establishes that the results are very similar for Hispanics in the US labor market (Hellerstein et al., 2010).[21]

There is evidence for European countries, in the studies cited in Section 18.2.4, which is consistent with spatial mismatch. One of the more compelling studies is Åslund et al. (2006), who studied a refugee settlement policy in Sweden that generates exogenous variation in location, finding that employment rates were lower among those allocated to areas with lower employment rates. However, this evidence typically does not separately consider the density of jobs where people live and the density of jobs for a particular group, as in the racial mismatch analysis. If evidence consistent with spatial mismatch is largely generated by low hiring for minority or ethnic groups, rather than low job availability per se, the case for place-based policies may be weaker than is implied by the spatial mismatch hypothesis. It would therefore be informative to have evidence on spatial versus racial (or ethnic) mismatch for other countries.

18.3.3 Are there important network effects in urban labor markets?

Bayer et al. (2008) presented evidence of labor-market network connections among nearby residents in urban areas. They found that individuals living on the same Census block in Boston are more likely to work on the same Census block than those individuals who do not live on the same block but live in the same block group (a small set of blocks). Because people within block groups are quite homogeneous, their interpretation is that the higher likelihood of working on the same block for those who live on the same block reflects informal labor-market networks based on network connections between those living on the same block (rather than sorting by place of residence and place of work).

Hellerstein et al. (2011) looked instead at whether neighbors work in the same establishment, to test the conjecture that neighborhood labor-market networks operate in part via referrals of neighbors to the employers of those in their network. The method compares the share of an individual's coworkers who are residential neighbors, relative to the share that would result if the establishment hired workers randomly from the geographic areas where *all* individuals who work in the Census tract reside, using matched

[20] In a structural model of labor and housing markets focusing on black-white unemployment rate differences in the United States (and African-French differences in France), Gobillon et al. (2013) suggested that spatial factors explain only 10–17.5% of the unemployment rate gap between blacks and whites.

[21] Andersson et al. (2014) studied the relationship between unemployment duration of workers who experienced mass layoffs and measures of job accessibility, finding that greater job accessibility is associated with shorter durations. The focus on mass layoffs is intended to reduce the correlation between unobserved characteristics of individuals and the accessibility to jobs where they live. The study compares estimates for blacks, for example, using either a general or a race-specific job density measure. The estimated strength of the relationship between accessibility and search duration is similar for both measures. However, it does not estimate a specification including both measures of accessibility simultaneously, as in Hellerstein et al. (2008), without which there is no way to tell whether the race-specific accessibility measure dominates the generic measure.

employer–employee data at the establishment level for the United States (Hellerstein and Neumark, 2003). Labor networks based on the place of residence would imply a higher share of neighbors among a worker's coworkers than would result from the random hiring process, which in turn simply reflects the likelihood that neighbors tend to work near where they live and hence near other neighbors, irrespective of any connections between them. This difference is normalized by an upper bound for the clustering of neighbors in the same establishment, which arises because, given the size distribution of establishments, perfect sorting by residence-based networks across establishments typically cannot occur.

The evidence indicates that residence-based labor-market networks play an important role in hiring. The "excess clustering" of neighbors in establishments—which is measured as the percent of the maximum systematic sorting of neighbors into the same establishment that could occur that is actually observed—is about 10% for blacks and whites. Controlling for establishment size, this network measure is nearly twice as large for blacks as for whites. Residence-based networks are considerably more important for Hispanics, with the measure rising to 22%, and to around 40% for Hispanic immigrants and those with poor English skills who are less integrated into the labor market and about whom employers may have less reliable information.[22]

Labor-market networks that are stratified by race or ethnicity could help explain the racial mismatch evidence and be relevant for place-based policies. Hellerstein et al. (2011) tested for this stratification by constructing the network measure in two different ways: first, treating the relevant set of a black worker's neighbors and coworkers as including either blacks or whites and hence measuring the extent to which black workers are clustered in establishments with black *or white* neighbors and, second, doing the same computations using only neighbors of the same race. If networks are racially stratified, then the likelihood that a black works with a neighbor regardless of race should be smaller than the likelihood that a black works with a black neighbor—exactly what the evidence suggests. Specifically, the network measure is 40% lower when disregarding the race of neighbors and coworkers, suggesting that labor-market information is less likely to flow between, e.g., black and white coresidents than between coresidents of the same race.

Hellerstein et al. (2010) presented a different kind of analysis, showing that Hispanic job density is most predictive of Hispanic employment in cities in which the Hispanic immigrant population has arrived and grown recently. These are cities in which network contacts may have been especially important in securing employment for new migrants, given that the local economies did not have long histories of Hispanic employment and employers in these areas did not have much experience with Hispanic workers, especially

[22] Evidence reported in the paper indicates that the place of residence can be treated as predetermined, potentially influencing place of work, rather than being determined by people who work together choosing to live near each other.

poor English speakers. This study provides further evidence of stratified networks and illustrates how stratified networks can generate evidence of racial or ethnic mismatch.

There is other evidence consistent with ethnically stratified networks. Kasinitz and Rosenberg (1996) studied the Red Hook section of Brooklyn, an area of high unemployment that is populated largely by low-income blacks (and to some extent Hispanics) but with a large number of local jobs in the shipping industry. They found that many local employers hire workers almost exclusively from outside of Red Hook, recruiting employees via social networks within specific (nonblack) ethnic groups. Turning to other countries, Patacchini and Zenou (2012) found that, in the United Kingdom, the probability that one finds a job through social networks is higher if there is a larger share employed among an individual's ethnic group living nearby (accounting for sorting in a couple of ways). Damm (2014), taking advantage of a quasi-experiment involving the settlement of refugee immigrants in Denmark, found that those who were settled in areas with higher overall employment rates of non-Western immigrants and conationals had a greater probability of finding employment and had higher annual earnings if employed.[23]

The implications for place-based policies are potentially complex, because racial mismatch or racially stratified networks imply that job creation policies, per se, may do little to help residents in target areas. Effective place-based policies may need to do more to exploit linkages between residents and workers in targeted areas.[24]

18.4. IDENTIFYING THE EFFECTS OF PLACE-BASED POLICIES

Empirical research on place-based policies focuses, naturally, on estimating the causal effects of these policies on the outcomes of interest. In many respects, the econometric challenges to reliably estimating these effects are similar to the standard program evaluation literature, such as the choice of counterfactuals and the potential endogeneity of where policies are adopted.[25] However, there are also a number of issues that are more specific to the analysis of place-based policies. In this section, we discuss these challenges and provide examples of how researchers have addressed them.

[23] Recent research on residential labor market networks using Longitudinal Employer-Household Dynamics (LEHD) data, however, finds less evidence of this kind of ethnic stratification of residence-based labor market networks in the United States (Hellerstein et al., 2014).

[24] Ananat et al. (2013) suggested a potential link between racial mismatch or racially stratified labor market networks and agglomeration economies—presenting evidence that wages rise more with the density of employment in one's industry in the local labor market when measured in terms of workers of the same race and ethnic group.

[25] See Baum-Snow and Ferreira (2015) for an overview of identification strategies used to uncover causal effects in urban economics research.

18.4.1 Measuring local areas where policies are implemented and economic outcomes in those areas

One unique challenge is that place-based policies often apply in geographic locations that do not directly map into geographic areas delineated in existing data sources. This issue poses a particular challenge in research on enterprise zones. In California, for example, enterprise zone boundaries do not follow boundaries of Census tracts, zip codes, etc., but are defined by streets and addresses. But because of data availability, tracts or zip codes have often been used to approximate enterprise zone boundaries (e.g., O'Keefe, 2004; Bondonio and Greenbaum, 2007). This introduces measurement error by incorrectly assigning areas (and the workers or businesses in them) as inside or outside the zones (Papke, 1993). Elvery (2009) noted that in California and Florida, if enterprise zones are defined as the areas encompassing all zip codes that overlap with enterprise zones, then the resulting definitions are 6 times larger than the actual zones, and less than half of the population residing in Census tracts that include enterprise zones actually live in enterprise zones. Random incorrect classification of locations creates a bias towards finding no effect of enterprise zones.

Neumark and Kolko (2010), in a study of the California enterprise zones, developed a method of precisely identifying enterprise zone boundaries over time. They start with official lists of street address ranges and the years they were included in the zone and then use GIS software to precisely identify the location of enterprise zones (and appropriate control groups) in each year of their sample.

Once boundaries are defined, data are needed on outcomes of interest within those boundaries and in control areas. Again, this can pose a challenge depending on the geographic information available on workers or firms. Estimating effects for California enterprise zones requires identifying the location of business establishments as inside or outside the zones, because enterprise zone benefits for businesses are based on this location. Neumark and Kolko used a new data source—the National Establishment Time-Series (NETS) (see, e.g., Neumark et al., 2005b)—that provides exact street addresses for establishments in every year. These addresses are then geocoded to obtain precise longitude and latitude, which permits the placement of these establishments in quite exact locations within their enterprise zone (and control area) maps.

18.4.2 Accounting for selective geographic targeting of policies

A second challenge is selecting appropriate control groups for place-based policies. Again, the research on enterprise zones, in which there are three approaches used, is instructive. The first is to identify control areas that are similar to the enterprise zones but where enterprise zone policies did not apply. The second is to use areas that were targeted for enterprise zone designation, but where enterprise zones either were not created or were created at a future date. And the third is to try to deal more explicitly with the endogenous selection of areas for zone designation.

Some studies have used broad control areas where enterprise zone policies did not apply, such as the remaining area of states that are not in enterprise zones (Peters and Fisher, 2002; Lynch and Zax, 2011). However, such broad control areas seem unlikely to provide a valid counterfactual for enterprise zone designation. Others have matched enterprise zone areas to control areas based on the characteristics of the zones or simply nearness to the zone. O'Keefe (2004) and Elvery (2009) matched Census tracts that approximate enterprise zone boundaries to other Census tracts using propensity score matching based on residential and employment characteristics. Of course, propensity score matching does not account for unobservable sources of differences in job growth that may underlie zone designation. None of these studies make use of comparisons of areas observed both before and after enterprise zones were established, while other studies use these matching strategies with before and after comparisons.[26]

More recent research tries to construct more reliable control groups by using more detailed geographic information on narrow areas. Billings (2009) used a spatial discontinuity model, looking at employment growth in Colorado's enterprise zones within ¼ mile of the zone boundary and using the area outside the zones within ¼ mile of the zone boundary as the control group.[27] Neumark and Kolko (2010) used their detailed GIS maps of the California enterprise zones to pick out a very narrow control ring (1000 ft wide) around the zone, on the presumption that economic conditions, aside from the effects of the enterprise zone, are likely to be very similar in the treated enterprise zone area and the closely surrounding control area. However, nearby and narrow control areas could be subject to displacement effects relative to enterprise zones; this issue is discussed in Section 18.4.4.

Geographic proximity of control areas does not preclude unobserved differences relative to treatment areas, which were the basis of zone designation in the first place. For example, zone areas could have been selected based on responsiveness to zone incentives, in which case the estimation may identify the average treatment effect on the treated (ATT), rather than the average treatment effect (ATE), and the ATT may provide much less reliable guidance to policymakers about the effects of extending the policy to untreated areas. Of course, invalid controls could imply that even the ATT is not identified.

A second approach that may better account for the selection of zones on unobservables is to use as controls geographic areas that were considered or qualified for the treatment or even designated as zones in other periods. For example, Neumark and Kolko (2010) exploited the expansion of zones in their data to compare changes in employment when an area of a zone is designated relative to contemporaneous changes

[26] See Papke (1994), Greenbaum and Engberg (2004), and Ham et al. (2011).

[27] Freedman (2012) exploited a discontinuity based on poverty eligibility thresholds for tracts.

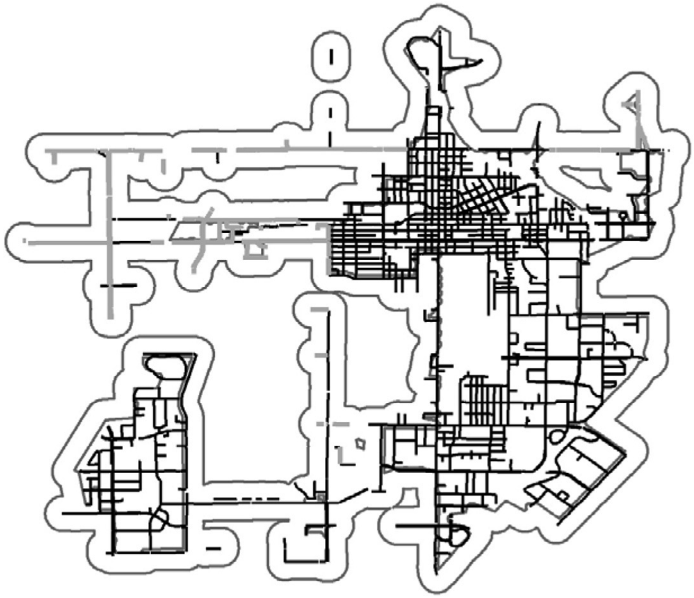

Figure 18.1 Santa Ana Enterprise Zone, initial 1993 designation (thick black lines), 1994 expansion (light gray lines), and control ring (dark gray outer envelope).

in areas that were designated earlier or will be designated later. To illustrate, Figure 18.1 is an example from Neumark and Kolko's study, showing the map for the Santa Ana Enterprise Zone, displaying the initially designated streets, the expansion streets, and the 1000 foot control ring discussed earlier. Identifying effects from comparisons to areas designated at other times can be more reliable than using close areas as controls, because it has been demonstrated through the policy process that the former were appropriate for enterprise zone designation. Busso et al. (2013) used similar strategies, comparing residential employment outcomes in Census tracts that became part of federal Empowerment Zones with outcomes in Census tracts that submitted unsuccessful applications to be designated Empowerment Zones and—paralleling Neumark and Kolko more closely—making comparisons with areas that become parts of zones in the future.

Hanson (2009) also compared employment outcomes in federal Empowerment Zones with unsuccessful applicant areas. However, he also instrumented for zone applicant success based on the political influence of the zone's Congressional representative. The potential advantage of this approach is that nearby control areas or areas that applied for but were not awarded Empowerment Zone status may differ in unmeasured ways that bias the estimated effects. An instrumental variable that predicts which zones succeeded, but does not directly affect the outcomes of interest, mitigates this problem.

18.4.3 Identifying the effects of specific policies when areas are subject to multiple interventions

Place-based policies like enterprise zones may cover areas that are also affected by other geographically targeted policies, sometimes run by different levels of government, and these need to be separated out to estimate the effects of any one policy. Many US cities, for example, have city- or state-designated redevelopment areas that encourage property development to remove urban blight. In California, as an example, hundreds of city and county redevelopment areas overlapped with or were adjacent to enterprise zones in the period covered by the Neumark and Kolko study and hence could affect both treatment and control areas. In addition, the three federal programs—Renewal Communities, Enterprise Communities, and Empowerment Zones—with a variety of benefits similar to those in state enterprise zones, overlapped with state enterprise zones.

To address this problem, the study also used digitized maps of the areas affected by redevelopment policies and federal enterprise zones and incorporated separate identification of these areas into the analysis to isolate the effects of state enterprise zones. Some other studies of enterprise zones pay attention to overlapping federal and state zones, but not redevelopment areas.

18.4.4 Accounting for displacement effects

A potentially serious problem in studying the effects of place-based policies is spillover effects between areas. For example, evidence that enterprise zone designation led to job growth might be regarded quite differently depending on whether the zone created new jobs or employers moved from one area to another to take advantage of enterprise zone credits—which reflects negative spillovers on other areas. Earlier research on the UK enterprise zones found that between 50% and 80% of enterprise zone businesses had relocated into the zones, prompting the British government to phase out the program (Papke, 1993). Of course, relocation does not necessarily imply that a program has not succeeded, because there may have been a number of reasons—reviewed earlier—to try to increase employment in a particular area even at the expense of other areas. Regardless, policymakers should value information on whether job creation in target areas comes at the expense of other areas, or via net job creation.

There can also be positive spillovers. For example, an enterprise zone may increase traffic in a geographic area, spurring demand and hence job growth in nearby areas. In this case, for some research designs, we might find no effect of enterprise zones on employment—or the estimate may simply be biased towards zero—because we are comparing enterprise zones to neighboring areas that were positively affected.

It is difficult to obtain estimates net of spillovers. The usual difference-in-differences approach captures relative effects of a policy on treatment versus control groups, with the assumption that the change over time in the control group was not due to the effect of the policy. One way to garner evidence on spillover effects is to posit differences across control areas in the likelihood of these effects arising. For example, it seems plausible that positive spillovers are confined to a very narrow geographic area near enterprise zone boundaries. Neumark and Kolko (2010) therefore compared results using a 2500 foot control ring instead of a 1000 foot control ring to see if the estimates of employment effects are stronger using the larger ring in which positive spillovers should be weaker. Similarly, they revert to the 1000 foot control ring but exclude a 100 foot buffer (in any direction) from the enterprise zone boundary. It is less clear, though, that these kinds of approaches are useful in ruling out negative spillovers, since these spillovers may also come from further away, with employers making longer distance moves (although still perhaps within the same city) to take advantage of zone benefits.

18.4.5 Studying the effects of discretionary policies targeting specific firms

Some place-based policies have a discretionary nature—for example, providing subsidies to specific firms to boost investment and employment. Such interventions may be restricted to businesses within targeted geographic areas, but the key characteristic—that not all businesses within the area receive the support—poses an additional identification problem. One question is whether such subsidies are effective in generating additional activity in recipient firms. However, comparisons to nonrecipient firms can be problematic because both the decision to apply for subsidies and the award decision can be endogenous. Those firms that apply may be performing poorly or anticipating a future deterioration in performance, or nonrecipient firms may be judged by government officials to offer less scope for generating additional investment or employment. Hence, nonapplicant and nonrecipient firms will likely have different characteristics than recipient firms, some of them unobservable.

A second question is whether discretionary policies generate benefits external to the recipient firm at the area level. Again, it may be difficult to find suitable controls outside the eligible areas, if those areas in which discretionary subsidies are available have been selected based on specific economic characteristics. In addition, there may be spillovers (positive or negative) from the policy to nonsubsidized firms both within and outside the eligible areas.

Criscuolo et al. (2012) analyzed the effects of a discretionary subsidy policy—Regional Selective Assistance (RSA) in Great Britain. The authors exploited the fact that the set of areas eligible for discretionary subsidies is revised every 7 years according to European Union (EU) state aid rules. Under these rules, subsidies could only be provided in designated areas and then only up to an area-specific maximum subsidy rate. Area eligibility is based on a set of criteria such as GDP per capita, which are measured relative to the EU average. A range of indicators of economic characteristics are used, and hence, areas can change eligibility status due to changes in the prevailing economic conditions in an area, changes in the indicators used by the European Union to determine eligibility, and changes in economic conditions in other EU member states that will affect the EU average used as a benchmark. The final two of these reasons can be considered exogenous with respect to unobserved characteristics of the areas.

To address the issue that firm eligibility is endogenous with respect to the characteristics of the area in which it is located, the authors used specific features of the eligibility rules as instruments for receipt of an RSA grant. In the estimation, they instrument a posttreatment plant- or firm-level indicator of participation in the program (i.e., grant receipt) with an area-time varying measure of the maximum subsidy rate allowable under EU regulations. They also include plant- or firm-level fixed effects to try to deal with the endogeneity of participation, although this will not deal with problems of time-varying unobservables. This IV strategy likely provides estimates of the ATT, for example in terms of the effects of the subsidy on investment and employment. They also estimate reduced-form specifications, for example regressing log employment at the plant level on the instrument—the maximum subsidy rate at the area level—providing an estimate of the intention-to-treat effect (all plants in an area where the maximum subsidy rate is nonzero being in principle eligible to apply).

The authors also used data aggregated to the area level, for example on employment, to capture any effects due to net entry, in addition to any changes in plant employment at the intensive margin, or due to spillovers across plants within areas. To do this, they regress area-level outcomes on the maximum grant rate determined by the policy rules. They also address the issue of between-area spillovers, for example due to a geographic shift in area eligibility that might lead to displacement of employment to newly eligible areas from contiguous locations, by using a broader geographic aggregation of the data.

Greenstone et al. (2010), as discussed above, provided evidence on the magnitude of agglomeration externalities generated by the opening of a new manufacturing plant. The paper provides a partial evaluation of the benefits of discretionary subsidies offered by local governments in the United States, by examining effects on incumbent plants' TFP growth, net plant entry, and area labor costs. As a novel identification strategy to deal with the endogeneity of the location decision, they exploited information on runner-up localities that narrowly lost out on each plant opening and used these as a counterfactual paired with the winning location. In terms of observed, pre-plant opening trends, the treated and counterfactual sites are highly comparable, much more so than a comparison to all other possible locations. They argued that the use of these near-miss locations as controls should eliminate problems of omitted variables that might otherwise bias comparisons of outcomes across treated and a wide set of nontreated locations.[28]

[28] The identifying assumption is that, conditional on observables, outcomes in the winning and near-miss areas would have evolved identically in the absence of the new plant opening. This rules out other unobserved area-time varying shocks that might differentially affect the paired locations.

For example, if location choices were made based on unobservable characteristics that also positively affect TFP growth, then this form of unobserved heterogeneity across locations would lead to upward-biased estimates of the effect of a new plant opening on this outcome. The use of paired counterfactuals can be considered as a form of one-to-one matching but with the matches determined directly from information on firms' decision-making processes. The authors estimated spillover effects on incumbent plants' TFP in treated counties by estimating plant-level production functions that include dummy variables for each winner–loser county pairing to ensure that the identification of spillovers in the period after plant opening is within each matched pair.

A number of papers evaluate the effects of Law 488, a capital investment subsidy program in Italy, by exploiting a specific feature of the grant allocation process. Applications to the scheme are given a normalized score on the basis of known criteria and then ranked on their score within each region and year. Each region has a preallocated amount of expenditure under the program each year, and hence, projects are funded in rank order until the funding pot is exhausted. These papers exploit the lower ranked, unfunded projects as a control group to address the endogeneity of participation. In a sense, this approach using "near-miss" applicants is analogous to the near-miss locations used by Greenstone et al. (2010) to deal with the endogenous selection of locations.

The fact that unsuccessful projects received a lower ranking means that they differ in their characteristics from the successful applicants. To control for observable characteristics that affect the probability of receiving a subsidy, Bernini and Pellegrini (2011) exploited detailed data on the actual variables used to construct the project-ranking scores. They used this as part of a propensity score matching exercise to control for selection on observables and to ensure common support in observable characteristics across the treatment and control groups. In addition, the authors argued that the fact that the ranking is carried out *within* regions and years and that each region has a different budget for the program in each year generates exogenous variation for a pooled sample of all applicants in the likelihood of being above or below the funding cutoff across these dimensions. Start-up projects are also given priority, and hence for an existing firm making an application, the probability of being funded will also depend on the number of start-ups applying for funding in their host region and year. Hence, it is quite possible for firms with very similar characteristics, and very similar scores, to receive the subsidy in some region-years but not in others.

To control for unobservable time-invariant characteristics, the authors employed a difference-in-differences approach using data on firms in their preapplication year compared with the year after the subsidized project is completed. Clearly, for the control group that did not receive the subsidy, the date at which the project would have been completed needs to be approximated. This is imputed using information on the average completion time, by year, industry, and investment type, from the subsidy recipients.

The authors also argued that spillover effects from subsidized to nonsubsidized firms are unlikely to confound the estimates since subsidized firms make up a very small fraction (around 3%) of manufacturing firms in the eligible regions.

Bronzini and de Blasio (2006) also looked at the effects of Law 488 using a difference-in-differences estimator and using applicants who did not receive a subsidy as controls. Since they found that those firms that score highly and receive a subsidy are a nonrandom sample of all applicants, they tried to address this problem by also adopting an approach akin to a regression discontinuity (RD) design (see Lee and Lemieux, 2010; Baum-Snow and Ferreira, 2015). To do this, they used narrower groups of treated and control firms that are close to the funding cutoff threshold and that have similar scores in the ranking process. These groups are defined as bands, for example firms within plus or minus 30 or 10 percentiles of the ranking distribution of firms around the cutoff threshold. Pelligrini and Muccigrosso (2013) also aimed to identify the impact of Law 488 on the survival of recipient firms using an RD approach. They argued that receipt or nonreceipt of a subsidy close to the budget cutoff point, as in Bernini and Pellegrini (2011), can essentially be considered as random.

18.4.6 Relative versus absolute effects

A final issue is whether empirical research can shed light on aggregate effects of place-based policies and, in particular, whether they result in a zero-sum game, simply relocating activity spatially. Applications of panel data estimators (or other methods of causal inference) can only identify the relative effect of the policy on treated versus control areas, where the latter are by definition assumed to be unaffected by the policy. Hence, such approaches cannot provide information about potential effects of the policy on the control areas, which would let us determine whether the policy had a net positive effect or not across both treated and control areas. Studies that look at displacement or spillover effects (as discussed in Section 18.4.4) can tell us something about impacts on areas not treated directly, but they typically estimate effects for a nearby (often small) area and, to do so, require some other control area that is in turn assumed to be unaffected by the policy.

It possible to make more headway on aggregate effects by relying more on theory. For example, as discussed in Section 18.2, if there are agglomeration externalities that are nonlinear, then moving economic activity can increase aggregate output (assuming activity moves to locations where the externalities are greater). Some evidence on this question comes up in Kline and Moretti's (2014b) evaluation of the Tennessee Valley Authority—a very large-scale place-based policy. As a second example, with enough theoretical structure to estimate welfare effects, one can get evidence on the aggregate effects of a policy (analogous to what we can learn from structural versus nonstructural approaches in other areas of economics). Busso et al.'s (2013) analysis of federal Empowerment Zones presents such an approach and estimates.

18.5. EVIDENCE ON IMPACTS OF POLICY INTERVENTIONS

We now come to the evidence from evaluations of place-based policies. We discuss a variety of types of place-based policies, beginning with enterprise zones. One common theme that emerges across all these types of intervention is that precise policy design matters for the behavioral responses that the policy ultimately delivers, and that some theoretical characterizations of place-based policies as simply setting an optimal city size or delivering a substantial but temporary policy or "big push" that could generate longer run, self-sustaining gains in the presence of agglomeration economies, are far removed from the multifaceted set of incentives that place-based policies provide in practice.

18.5.1 Enterprise zones

The results from earlier studies of enterprise zones varied widely. Many studies failed to find employment effects of enterprise zones, although some of the work (e.g., O'Keefe, 2004, and research reviewed in Wilder and Rubin, 1996) concluded that there are positive employment effects, at least in the short run. Relatively recent overviews of the literature conclude that it is difficult to find evidence of positive employment effects of enterprise zones (Elvery, 2009; Ham et al., 2011; Lynch and Zax, 2011).

However, in the past few years, there have been numerous studies of enterprise zones making creative use of both data and econometric methods to overcome some of the empirical problems involved in evaluating place-based policies in general and enterprise zones in particular. In this section, we discuss this recent research. We begin by discussing the Neumark and Kolko (2010) study of California enterprise zones as an example addressing many of these problems. We then turn to concurrent or more recent evidence, highlighting how other studies address the same research challenges and also trying to resolve what the extensive new literature says and identify the important questions for further research.

As the earlier discussion indicated, the multiple challenges that arise in studying place-based policies imply that the details of the analysis can be quite important. Hence, in some cases, we delve into these details to illustrate the issues that arise and how researchers have addressed these issues and the potential consequences of some of these choices; these are lessons that apply beyond the specific study of enterprise zones. In other cases, the discussion is more cursory and one has to refer back to the original paper for more details.

18.5.1.1 The California enterprise zone program

The California enterprise zone program had multiple goals—not only primarily attracting jobs and businesses and raising employment but also reducing poverty and unemployment and raising incomes in target areas. The program provided a variety of tax incentives to businesses located in designated areas to try to encourage the hiring of economically disadvantaged workers and to spur the creation of businesses. The largest incentive accounting for the lion's share of the cost was a state tax credit equal to 50% of qualified wages (up to 150% of the minimum wage) in the first year, falling by 10 percentage points each year until reaching zero after 5 years. The main criterion for getting the credit was hiring workers who resided in a Targeted Employment Area (TEA)—a Census tract with low income. However, TEA residents qualified for the hiring credit regardless of the worker's characteristics, and many TEA residents in mixed-income neighborhoods are not disadvantaged. Nevertheless, given that disadvantaged workers earn lower wages, the tax credit could result in a larger relative reduction in the cost of hiring low-skill labor.

Localities applied to the Department of Housing and Community Development to have a geographic area designated as an enterprise zone. Eligibility criteria include job-generating capacity and the level of economic distress measured along a number of dimensions. The area also had to include an industrial or commercial area "contiguous or adjacent to" the distressed area. In addition, the application for enterprise zone status required the preparation of an economic development plan (including marketing, finance and administration of the plan, other local incentives, infrastructure development plans, and information management).[29] The hiring credit was paid to firms located in the enterprise zone, but businesses in an enterprise zone could claim hiring credits for employees living in a TEA, which need not be coincident with the enterprise zone. Hence, the program has to be evaluated for businesses located in the zones (or TEA residents), rather than zone residents.

As noted above, Neumark and Kolko exploited the expansion of original zones to construct control areas, while alternative control areas come from very narrow geographic rings around the zone. They defined the original zone and each expansion area, as well as the control rings (when used), as unique "subzones," constructing an observation on each subzone–year pair. They specified regression models for log employment, which include a dummy variable for enterprise zone status and dummy variables for each subzone and year; the year effects account for the possibility that enterprise zones were established in periods of either particularly high- or low-employment growth across all of the regions in the sample. They also included a full set of enterprise zone–year interactions, which allow for an arbitrary pattern of changes over time across the broad area covered by a zone, its expansions, and the associated control ring (when included). Given that the effect of enterprise zone designation is identified off of subzone-level variation, even with these arbitrary changes over time for each enterprise zone, the effect of enterprise zone designation is identified. They also estimated models including subzone-specific linear time trends and models that allow enterprise zone designation to shift the growth rate of employment.

[29] The California enterprise zone program was substantially changed in 2013, including eliminating the hiring credit.

Other geographically targeted policies are accounted for in two steps. First, subzone–year pairs are redefined to represent status with regard to not only whether and when they became part of an enterprise zone but also whether and when they became part of a redevelopment area or federal zone, resulting in far more subzones. Second, the specifications are modified to include dummy variables indicating whether each subzone is in a redevelopment area or federal zone in each year.

Across a variety of specifications, there is no evidence that enterprise zones affect employment. The estimates (summarized in Table 18.2, along with the estimates from other studies discussed here) are small, statistically insignificant, and negative as often as they are positive. The statistical power of the evidence is modest, as the confidence intervals for the estimated employment effects are rather large. The baseline model for employment was also estimated with many leads and lags of the enterprise zone dummy variable, to see whether, for example, enterprise zones tended to be established in areas that had transitory downturns in employment relative to other areas, in which case the finding of no effect would be strengthened (because the mean reversion would look like a positive treatment effect). Alternatively, if zones are established in areas doing particularly well just before designation, perhaps because such areas have better organized constituents for capturing an enterprise zone, then the estimated effects from the simple model might fail to detect longer run positive effects of enterprise zone designation on the rate of job growth. Similarly, the many lags allow the data to reveal whether effects of enterprise zones emerge over the longer term. The resulting estimates do not exhibit any evidence of leading or lagged effects, but instead cement the view that enterprise zones in California did not affect employment.

If the enterprise zone program has positive spillovers, encouraging employment growth not only within zone boundaries but also outside zone boundaries, then there might be no evidence of an effect of enterprise zones on employment because enterprise zones are compared to immediately neighboring areas. But the evidence is similar using the larger (2500 foot) control rings. What about negative spillovers, with enterprise zones pulling jobs and businesses away from nearby areas? The similarity of results with and without control rings undermines this possibility. Moreover, such negative spillovers would tend to produce evidence that enterprise zones *do* encourage job growth relative to control areas. Thus, if there were negative spillovers, the conclusion that there are no positive employment effects would only be reinforced. Finally, in the analysis accounting for the overlap between state enterprise zones and redevelopment areas or federal zones, there is similarly no evidence that enterprise zones have positive employment effects, whether or not they are combined with these other local policies.

Enterprise zone programs vary in the level and nature of tax credits and other incentives, as well as in other forms of assistance available to zone businesses—some of which are difficult to quantify and evaluate. This heterogeneity across programs limits how much one can generalize from the study of a single program, and heterogeneous effects

Table 18.2 Summary of evidence on enterprise zones

Study	Country	Program	Results
Neumark and Kolko (2010)	United States	California enterprise zones	No significant evidence of employment effects measured at establishments in zones: estimates range from −1.7% to +1.8% (levels), with large confidence intervals (≈−8% to +6%); no evidence of spillovers
Kolko and Neumark (2010)	United States	California enterprise zones	Zones more involved with marketing and outreach exhibited positive employment effects; zones focused on tax credits exhibited negative effects
Elvery (2009)	United States	California and Florida enterprise zones	No evidence of positive employment effects on zone residents: estimates for California range from −0.4% to −2.6% and for Florida from −1% to −4%
Freedman (2013)	United States	Texas enterprise zone program	Positive effect on employment growth among zone residents (1–2% per year, sometimes significant); employment effects concentrated in jobs paying less than $40,000 annually and in construction, manufacturing, retail, and wholesale; positive effects on job growth among zone employers (3–8% per year, rarely significant) Negative and insignificant effects on share black and with income below the poverty line Significant negative effect on vacancy rate (−4%) Significant positive effect on median home value (10.7%)
Ham et al. (2011)	United States	State enterprise zones, federal Empowerment Zones, federal Enterprise Communities	State programs: significant positive impacts on unemployment rate (−1.6 percentage points), poverty rate (−6.1 percentage points), average wage and salary income (≈1.6%), employment (≈3.7%)[a] Empowerment Zones: significant positive impacts on unemployment rate (−8.7 percentage points), poverty rate (−8.8 percentage points), average wage and salary income (≈20.6%), employment (≈34.2%) Enterprise Communities: significant positive impacts on unemployment rate (−2.6 percentage points), poverty rate

Continued

Table 18.2 Summary of evidence on enterprise zones—cont'd

Study	Country	Program	Results
			(−20.3 percentage points), fraction of households with wage and salary income (4.9 percentage points), average wage and salary income (≈12.7%), employment (≈10.7%) Positive but insignificant spillovers on neighboring Census tracts
Busso et al. (2013)	United States	Federal Empowerment Zones	Positive and significant effects on job growth in LBD (12–21%), likely concentrated among births and existing establishments with >5 employees Positive and significant effects on employment in Census data (12–19%); magnitudes generally larger for employment in zone of zone residents (15–17%) than nonzone residents (6–16%) Positive generally significant weekly wage effects on zone residents employed in zone (8–13%); magnitudes smaller for zone residents generally (3–5% and usually insignificant) and nonresidents working in zone (≈0%) No effects on rents, population, or vacancy rates; large significant positive effects on house values (28–37%)
Reynolds and Rohlin (2014)	United States	Federal Empowerment Zones	Positive effect (1.1%, insignificant) on difference between rent and wage premiums (quality of life) Positive effect (6.4%, significant) on sum of rent and wage premiums (quality of business environment)
Hanson (2009)	United States	Federal Empowerment Zones	OLS estimates: positive significant effect on employment rate (2 percentage points); negative significant effect on poverty rate (−2 percentage points) IV estimates: No effect on employment rate (0 percentage points); insignificant positive effect on poverty rate (2 percentage points)
Hanson and Rohlin (2013)	United States	Federal Empowerment Zones	Negative spillovers on Census tracts that are geographically or "economically" close to zone tracts: negative, generally significant effects on number of establishments (−15.2 to −36.5); negative, sometimes significant effects on employment (−52 to −1223, but many estimates in the range −300 to

			−600); negative spillovers roughly offset the positive effects in directly treated areas Estimates of program effects based on comparison of actual zone tracts to those that are close (using the same definitions) yield positive effects of about the same magnitude as the negative spillover effects
Reynolds and Rohlin (2013)	United States	Federal Empowerment Zones	Positive significant effects on mean household income (11%), but not on median household income (one-tenth as large) No significant effect on poverty rate (−1 percentage point); significant increase in proportion of households below one-half of poverty line (1.1 percentage points) and in households more than twice the poverty line (1.9 percentage points), coupled with significant reductions in households in between Significant increase in shares of households with income <\$10,000 and above \$100,000 Other results point to higher-skilled, higher-income people moving in: increases in proportions of households with higher education and earnings above \$100,000; increases in housing values for houses valued at \$100,000 or higher, extending above \$300,000 Evidence of spatial variation: increases in proportion of households more than twice the poverty line in areas of zone with above-median poverty rate initially, and increases in proportion below one-half of poverty line in areas of zone with below-median poverty initially
Givord et al. (2013); similar results in Mayer et al. (2012)	France	Zone Franches Urbaines	Positive effects on business creation in and relocation into zone (≈5–6%); similar estimates for employment, but imprecise; positive effects fully offset by negative effects in 300 m ring around zone Briant et al. (2012) reported more positive effects in zones with better access to transportation

[a]Approximate percent changes are calculated by dividing their estimates of effects on levels by values in zones reported for 1990.

could help explain why the extensive research literature on the employment effects of enterprise zones is not unanimous in the conclusions it reaches.

Indeed, earlier work provided suggestive evidence of substantial heterogeneity in the effects of enterprise zones, both within and across state enterprise zone programs (Erickson and Friedman, 1990; Elling and Sheldon, 1991; Dowall, 1996), and evidence that enterprise zones were more effective when tax incentives were "complemented by more traditional supports for economic development (e.g., technical assistance, location/site analysis, special staffing)" (Wilder and Rubin, 1996, p. 478). This led Ladd (1994) to suggest that supply-side tax reductions, which are generally uniform across individual zones, are ineffective, whereas interventionist components like technical assistance account for whatever success enterprise zone programs have (p. 202).

In a follow-up study, Kolko and Neumark (2010) explored the associations between the job-creating effects of California enterprise zones and (1) factors relating to the areas in which enterprise zones are established and (2) how enterprise zones are administered. The research used a survey of enterprise zone administrators that asked detailed questions about how active the zone was in using marketing, amending zoning or other local regulations, training workers or operating hiring centers, facilitating earning tax credits, encouraging the building of additional infrastructure, and offering other tax incentives, credits, or discounts on public services at the local level. The analysis uses these responses and information on characteristics of enterprise zone locations that could influence the effectiveness of the program, such as employment density, industry mix, and local demographics.

The estimates point to variation in program effectiveness across individual zones, which has potential implications for features of enterprise zone programs that policymakers and administrators might encourage via both legislation and the selection of sites as enterprise zones. Enterprise zones appear to have a more favorable effect on employment in zones that have a lower share of manufacturing and in zones where managers report doing more marketing and outreach activities. Somewhat surprisingly, a strong focus on helping firms pursue hiring credits made available by the enterprise zone program appears to run counter to job creation efforts, likely because these activities focused more on claiming the tax credits retroactively than on creating jobs currently.[30] One implication of these findings is that the overall evidence from the literature on enterprise zones may be too pessimistic and that it may be possible to find ways to make enterprise zones more effective at creating jobs.

Elvery (2009) reached similar conclusions to Neumark and Kolko (2010) for California. He focused on the effects of enterprise zones designated in the mid-1980s on employment of zone residents in the 1986–1990 period. His method matched tracts that are in zones or substantially overlap with them to nonzone tracts using propensity

[30] Moreover, until reforms in 2007, "cross-vouchering" was allowed, whereby one zone could collect fees for helping businesses from other zones get credits.

score methods. Elvery also estimated a neighborhood component of employment to capture employment differences across neighborhoods that are not related to the characteristics of individuals in those neighborhoods and also estimated the effect of enterprise zone designation on this neighborhood component, which led to more precision.

Elvery found no evidence of positive effects of enterprise zones on employment—in this case, viewed in terms of employment of residents (rather than employment at enterprise zone establishments as in Neumark and Kolko (2010), which is more appropriate given the distinction between the zones and TEAs). Indeed, his point estimates are always negative, ranging from about −0.4 to −2.6 percentage points (not statistically significant). Elvery did impose some sample restrictions based on age (18–55) and, in some analyses, looked at men only, although it is difficult to imagine that positive effects among the groups he excluded could possibly offset the negative point estimates he reported.

18.5.1.2 Other recent evidence for US state-level and federal programs

A number of recent studies of enterprise zones in the United States address the empirical challenges in different ways and take up different substantive issues, with conclusions that sometimes vary. Freedman's (2013) analysis of the Texas enterprise zone program is a good example of a study that addresses many of the key challenges in the evaluation of enterprise zones. First, the study exploits an unusual feature of the Texas program to construct an appropriate counterfactual, because Census block groups were designated as enterprise zones mechanically, based on whether the poverty rate in the 2000 Census was equal to 20% or greater. Freedman therefore used a regression discontinuity around the 20% cutoff to estimate effects on job growth (his main focus), as well as other outcomes.

Freedman also paid careful attention to estimating the effects for those who should actually be affected by the policy—which is obviously important (as also suggested with respect to California) but not always as simple as it seems. The Texas program has incentives for hiring zone residents, but the firms that hire them do not have to be located in the zone. Rather, employers are designated as "Enterprise Projects" that can claim benefits (sales and use tax refunds of up to $1.25 million over 5 years) in return for committing to hire a certain share of employees from enterprise zones or who are economically disadvantaged. Freedman suggested that firms are more likely to target workers based on where they live because that is much easier to verify. As a result of these program features, he argued that the hiring effects should be more pronounced for zone residents than for establishments in the zone, and Freedman examined both by using data from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program, which has information on workers' place of residence and place of work. Freedman also accounted for overlapping state and federal enterprise zones and, as discussed below, presented some analyses meant to avoid the effects of spillovers.

Freedman focused most of his analysis on block groups in a narrow band around the 20% poverty threshold, showing that there are no clear breaks around this threshold in characteristics of block groups other than zone status, which helps to bolster the RD design. The estimates for resident employment indicate a jump in the annual growth rate of employment of 1–2%, which is fairly large when cumulated over a number of years. For workplace employment, the evidence is less clear, consistent with Freedman's conjecture that the effects should be less evident for employment based on workplace location, although the point estimates are sometimes larger, raising the possibility that something else was occurring in the designated zones. The study also breaks out results by broad industry and broad pay categories, with perhaps the most interesting finding that the employment effects are concentrated in jobs paying less than $40,000, although because there was not a hiring credit that is a larger share of the pay for low-wage workers, it is not clear why this would necessarily be expected. The paper does not provide any of the usual RD figures for this analysis, however.

Freedman reported analyses excluding control block groups within narrow rings around zones, to avoid possibly overstating the effects because of negative spillovers. Almost none of the estimates are significant for either employment measure when this is done, and in some cases, the positive effects on resident employment become smaller or even negative, consistent with the positive findings being driven more by relocation between nearby areas.

Freedman concluded that "EZ designation has positive effects on resident employment" (p. 340). We are a bit less confident in this conclusion, and the qualification that the effects may largely reflect relocation is important. Nonetheless, this is clearly a very careful study that appropriately addresses the many challenges that arise in drawing causal inferences about the effects of enterprise zones.

Freedman also presented results using the same research design to look at other outcomes, using American Community Survey (ACS) data. Regression estimates indicate a statistically significant 11% increase in median housing values at the 20% poverty threshold, as well as a 4% decline in the share of housing units that are vacant. The point estimates also indicate an increase in population and a decrease in the share black, although these estimates are not close to statistically significant. At the same time, the data indicate no change in median household income. One interpretation—although the evidence can only be taken with a grain of salt given the lack of significant findings—is that enterprise zone designation led to some compositional shifts in the population and that the main effect seems to have been an increase in land value, a finding that arises in other studies, some of which suggest that this may be a principal effect of enterprise zones (e.g., Hanson, 2009).[31]

[31] Evidence of the effects of enterprise zones on *commercial* property might be more compelling. Burnes (2012) provided evidence of capitalization of enterprise zone benefits in California into commercial real-estate prices.

Ham et al. (2011) studied state and federal programs and concluded that the programs "have positive, statistically significant, impacts on local labor markets in terms of the unemployment rate, the poverty rate, the fraction with wage and salary income, and employment" (p. 779). Their state-level analysis looks separately at California, Florida, Massachusetts, New York, Ohio, and Oregon, as well as an aggregation of seven other states that have relatively few tracts in zones. As they noted, enterprise zone benefits vary widely across states. For example, the hiring credit—which we would think would be most relevant to job creation—was worth $35,000 over 5 years in California and was also large in Florida. At the other extreme, Ohio offered $300 per new employee and Oregon offered no hiring credit. In the federal zones, the main credit was for hiring, with both programs offering a credit of up to $2400 for 18- to 24-year-olds for the first year of employment and Empowerment Zones also offering a credit of up to $3000 per employed resident working in the zone for up to 10 years, with the credit declining over time (Busso et al., 2013).

Ham et al.'s econometric approach to the selection problem is to compute a triple-difference estimate. Because they focused only on zones established in the 1990s (or expansions of zones that took place in the same period), their baseline first difference is the difference in outcomes between 2000 and 1990 in the tracts where zones were established. They then subtracted from the 2000–1990 difference the 1990–1980 difference to pick up any differences in linear trends. From the double difference for tracts where zones were established, they then subtracted off the corresponding double difference for three different controls (always using the same years): the nearest tract that was not in a zone, the average of contiguous tracts to the zone that were not in the zone, and the average of all other nonenterprise zone tracts in the state. This estimator can be interpreted as allowing the treatment and control zones to have distinct intercepts and linear trends, but common higher order trends.

To address overlapping programs, Ham et al. restricted attention to tracts affected by only one of the three programs during the 1990s. And to address spillovers, they estimated treatment effects for the nearest nonenterprise zone tract to each enterprise zone tract, using the second-nearest nonenterprise zone tract as the comparison. A potential downside of this approach is that the comparison tract (or tracts) for the actual enterprise zone tract and the tract where there are potentially spillovers are not the same—in contrast to comparing both to a more distant tract that clearly was not affected by enterprise zone designation.

The results Ham et al. reported for the combined (average) effect of state enterprise zones and for the two types of federal zones are almost always strong and positive. As summarized in Table 18.2, across the three types of zones, the authors generally found positive and significant effects on the outcomes they considered. Moreover, they concluded that spillover effects are if anything positive, although in general the evidence is statistically weak and sometimes points in the other direction; certainly, there is no strong indication of negative spillovers. At the same time, some features of their estimates are surprising and hard to interpret. First, looking at the estimates for the federal zones and for the state zones averaging across all the states they considered (which is what Table 18.2 reports), the estimates are often implausibly large, such as an increase in employment of around 34% from federal Empowerment Zones and a reduction in the poverty rate of 20.3 percentage points from federal Enterprise Communities. More generally, the positive effects they estimate for Enterprise Communities are surprising, given that other researchers regard Enterprise Community benefits as inconsequential relative to Empowerment Zone benefits (Busso et al., 2013; Hanson and Rohlin, 2013).

Second, the cross-state variation in estimated effects is surprising. The estimated employment effect for California is small and negative, whereas only for Ohio is there a significant positive effect. Yet California had a huge hiring credit, whereas Ohio's was only $300. And Oregon, which has the second-largest point estimate for the employment effect, had no hiring credit. They do estimate a large employment effect for Florida (not statistically significant), and Florida has a large hiring credit; yet Elvery's (2009) estimates for Florida for the previous decade are consistently negative.[32] To increase precision, Ham et al. presented estimates pooling the data across states. But the large policy differences across state enterprise zone programs make this questionable. If one accepted the constrained estimate, one would be equally likely to believe it applied with no hiring credit or a $35,000 hiring credit.

These kinds of findings indicate that it would be useful for future research to try to tie the effects of enterprise zone policies to features of those policies and exploit variation in the generosity of the policy to a greater degree in evaluation. This could include information on the value of hiring credits and other features that would make such policies more likely to lead to employment increases, such as requiring that the number of employees grow in order for firms to qualify for incentives. One possibility, although it cannot reconcile all the conflicting findings, is that the effects of enterprise zones arise from something other than hiring credits; this issue resurfaces in research focusing on federal Empowerment Zones, to which we turn next.

Busso et al. (2013) studied the effect of federal Empowerment Zones. They compared outcomes in the six urban communities that were awarded Empowerment Zones with the full range of benefits and credits, to matched tracts of rejected zone applicant areas and areas in future zones. The comparison to future zones has parallels to Neumark and Kolko (2010), but differs from Ham et al. in using tracts from other cities and states as controls, rather than nearby tracts.[33] In addition to substantial hiring credits, Empowerment Zones

[32] Billings (2009) reported positive employment effects for new establishments in Colorado's enterprise zones, based on a border discontinuity design, looking at 1990–2000 data, even though Colorado's hiring credit is minor (only $500, increasing to $1000 under some circumstances, plus a job training credit).

[33] Busso et al. did not address overlap between federal and state enterprise zone programs. They also argued that spillovers are unlikely to affect their estimates because most rejected and future zones are in different cities. But that does not rule out their estimated effects reflecting the relocation of businesses into a zone from nearby.

received block grants of up to $100 million for purposes such as business assistance, infrastructure investment, and training programs. For nearly all of the cities in which zones were rejected, Enterprise Community status was awarded instead; these areas did not receive major block grants and had much more restricted hiring credits.[34] Busso et al. used 1980, 1990, and 2000 Census data including confidential information on where people live and where they work, by tract, and establishment-level data from the Longitudinal Business Database (LBD) from 1987, 1992, and 2000. In both cases, they focused on the estimated impact of Empowerment Zones designated in 1993 on changes over the 1990s.

Busso et al. found, in both the Census and LBD data, that Empowerment Zone designation appears to generate substantial job growth—around 21.3% in the LBD and 12.2% (not significant) in the Census data. Moreover, the Census data suggest that there were increases in jobs in the zone held by residents (17.6%), but less evidence of such effects for nonresidents (6.4%, not significant). The Census data also point to large increases in nonzone employment of zone residents (12.3%, not significant), which suggests that there may have been effects due to factors other than zone incentives.

They also found evidence of positive wage effects on zone residents working in the zone (12%), but no effect on wages of nonresidents working in the zone. The positive point estimate for the employment of nonresidents in the zone is consistent with zone designation increasing the productivity of labor in the zone, but we should not necessarily expect a positive wage effect because the hiring credit should not affect wages of nonzone residents relative to what they earn elsewhere (in equilibrium).

The fairly large estimated employment effects for zone residents working outside the zone suggest that the effects on zone employment that Busso et al. estimated are not fully attributable to the hiring credit. And some of the other estimates they reported (in their Table 5) of employment effects on nonzone residents—although not their preferred estimates—are quite large. The block grants are large, and there is some evidence—although Busso et al. appropriately noted that it is far from rigorous—that the block grants (or something else about the zones) may have attracted large amounts of outside private capital. This could have boosted employment in the zone of nonresidents and, perhaps through spillovers, employment of zone residents outside the zone. If in fact the block grants played a major role, this may help square the results of Busso et al. with those of Neumark and Kolko (2010), who found no effect of the California enterprise zone program that very much concentrated its incentives on hiring credits.

[34] Note that these cities are included as controls (or potential controls, subject to trimming based on propensity score matching) by Busso et al., whereas Ham et al. studied these as a separate group of treated cities and sometimes found larger effects than for Empowerment Zones. Given the potentially large benefits suggested by the Busso et al. results, this may further reduce the plausibility of the results for Enterprise Communities found by Ham et al.

The authors also developed a stylized general equilibrium model that captures the welfare implications discussed in Section 18.2—that the welfare gains from place-based policies depend on whether many agents are inframarginal in their preferences over places to live and work, which is more likely when their preferences are more heterogeneous. In their modeling framework, one can express the effects on welfare in terms of elasticities of the various marginal responses one can estimate from the data (some more precisely or convincingly than others). Adding estimated effects on housing prices, rents, and population, their welfare calculations point to potentially large gains—in large part because an absence of positive effects of zone designation on population or rents suggests that large migration responses do not dissipate the gains of the program. Despite finding no effects on rents, Busso et al. found large effects on house prices. They suspected these are inflated, in part because they did not find an effect on vacancies or on out-migration, but other estimates still point to positive effects. And they suggested that rents may be sticky in the short run because of rent control. However, other evaluations of the Empowerment Zones discussed below also report evidence of house price increases, as well as some evidence of compositional shifts towards the more skilled and a decline in vacancies, suggesting smaller welfare gains.

The authors estimated relatively little deadweight loss from the program, based on calculations and auxiliary information used to estimate jobs created in the targeted areas, although they noted that it is difficult to pin down estimates of the number of jobs created in the zones, for zone workers, in the covered sector. They estimated substantial welfare gains. The bulk of the estimated welfare gains come from positive effects on house prices in the zones, raising the question of whether the program is achieving its distributional goals. A key question is whether the block grants raised productivity of workers in the zone. Under fairly small gains (0.5%), the benefits would outweigh the costs, but we do not know the actual impact of these block grants. Another question is whether the evidence of positive employment effects is attributable to the effects of these block grants. It would therefore be useful to know how these block grants were used and whether there is corroborating evidence that they had effects that boosted employment—perhaps in conjunction with zone benefits—in light of other evidence that even generous enterprise zone hiring credits do not increase employment. This discussion again emphasizes that it would be useful, in future research, to try to parse out the effects of different dimensions of enterprise zone policies.

Reynolds and Rohlin (2014) approached the problem of evaluating the welfare consequences of these zones in a different way, by embedding the analysis in the quality of life framework standard to urban economics, estimating the effects of Empowerment Zone designation on wages and rents in hedonic equations. They found notable increases in what is termed the "quality of the business environment," which is captured in the summed effects on rents and wages. And they found modest increases in the quality of life for individuals, captured in the difference between rents and wages (what individuals are willing to pay to live in an area).[35] Moreover, Reynolds and Rohlin presented evidence suggesting that the role of the wage credit is negligible. This is potentially important because the value of the wage credit only lasts as long as the wage credit is available, whereas other sources of gains may be more permanent, such as agglomeration effects from firms moving into the area.

The impact of federal Empowerment Zones has been considered in a number of other studies, most of which are less favorable than the Busso et al. evaluation on many dimensions. Hanson (2009) extended the analysis of Empowerment Zones by considering endogenous selection from among the applicants, which can induce selection on unobservables that is not addressed by matching methods. If selection is based on unobserved improvements in economic conditions, there is a bias towards finding positive effects. The results cannot be directly compared to those of Busso et al. because there are other differences in both data and research design. It would likely be highly informative to explore the issues discussed in the various papers assessing the federal program in a single study that held the data constant and focused solely on the substantive issues addressed by different research designs.

Hanson instruments for zone designation using representation of the areas encompassing the proposed zones on the powerful US House Committee on Ways and Means. Hanson considered possible reasons the instrument may be invalid, such as if representation on the Committee Ways on Means yields other economic benefits (or costs) to the same districts. He presented some evidence suggesting this is not the case, although that begs the question of why members exert power over zone designation but not other public resources and, if members do exert other influences on economic outcomes, the IV would be invalid. The estimates without instrumenting indicate that Empowerment Zone designation increased employment significantly, by 2 percentage points, and reduced poverty significantly, also by 2 percentage points. However, the IV estimates indicate no effect on employment and a positive but insignificant effect on poverty.[36] Hanson concluded that "OLS specifications over-estimate the effect of the EZ program on increasing resident employment and decreasing poverty" (p. 728).

Hanson and Rohlin (2013) attempted to directly estimate the spillover effects of federal Empowerment Zones on nearby or similar areas—effects that could be negative

[35] This approach may not capture welfare in the sense of the aggregation of utility across individuals. But that is a very tall order and many economists are skeptical of the stylized models needed to engage in such calculations. However, by capturing these two dimensions of the effects of enterprise zones, Reynolds and Rohlin successfully encompassed most of what other research talks about as the criteria for deciding whether these policies are effective.

[36] Hanson also finds that OLS estimates of the effects on median residential property values indicate insignificant increases of around $6600, which is smaller than reported by Busso et al. or by Reynolds and Rohlin (2013), discussed below. And his IV estimates are huge, indicating implausible increases of over $100,000.

or positive. They identified tracts that are close to the Empowerment Zones, based on either geography or economic similarity, and compared changes from before and after zone designation for the close tracts to what happened in tracts that were close—on the same measure—to the rejected applicants in other cities (which became Enterprise Communities). The evidence points to negative spillover effects. For establishment counts, the estimated effect ranges from −15.2 to −36.5, with almost all of the estimates statistically significant. The employment effects are more variable and statistically significant about half the time; but many are in the range of −300 to −600.[37] Moreover, when they estimated program effects based on comparisons of the actual zone tracts to those that are close (using the same definitions), the positive effects are of about the same magnitude in absolute value as the negative spillover effects, suggesting that Empowerment Zones are, to a first-order approximation, simply creating relocation of economic activity.

A recent study by Reynolds and Rohlin (2013) instead emphasizes that evidence of positive mean effects of Empowerment Zones masks distributional effects that are much less favorable to the disadvantaged. Using similar but not identical methods and data, their results indicated that the zones were advantageous to high-skilled, high-income people who to some extent likely moved into Empowerment Zones because the program made these areas more attractive and were neutral or even harmful to the impoverished residents of these zones. They largely replicate the Busso et al. findings that Empowerment Zone designation boosted mean wages and employment—although in their case, this is documented in terms of mean annual household income, which they estimate rose by around 11%. However, the effect on median household income was only $250, and not statistically significant, and Empowerment Zone designation had no detectable impact on the poverty rate, with an insignificant 1% decline.[38]

Moreover, they find increases in the proportion of households below one-half the poverty line, commonly termed "extreme poverty," with an estimated increase of 1.1 percentage points. When they look at effects across bins of the household income distribution, the only sizable (and significant) increase occurs for households earning at least $100,000, which is unlikely to be directly attributable to Empowerment Zone incentives (since the hiring credit represents a much larger percentage of pay for low-wage workers), and an increase in the share of households with an income of less than $10,000.[39] They also present evidence of increases in the share of people with higher

[37] The authors do not report the means needed to calculate percentage effects, but these are large numbers relative to tract population, which averages about 4000.

[38] Their data are closer to Hanson (2009), although their approach is closer to Busso et al. The poverty estimate is not that different from Hanson, who found a 2 percentage point decline.

[39] They have to use income categories that are fixed in nominal terms across the years. Clearly, then, there will be some upward drift into higher income bins from inflation. However, the authors argue that since this occurs in both the Empowerment Zone and non-Empowerment Zone cities, it does not affect the triple-difference estimates.

education (some college or more), most likely consistent with inflows of higher skilled people into the areas designated as Empowerment Zones—like the increased share with high incomes. Finally, when they break up the zones into tracts with initially above- versus below-median poverty rates, they find that the positive income effects (at $100,000 or above) occur solely in the lower poverty tracts, whereas there is evidence (not quite statistically significant) that the increase in the share of households with less than $10,000 in income occurs in the high-poverty tracts.[40]

It is hard to be definitive about what reflects an impact on residents versus a composition effect. But it is unlikely that lower income households would have replaced higher income households in response to Empowerment Zone designation. In contrast, the increase in education levels (and higher incomes) seems likely to be a composition effect. Thus, these results present a more negative portrait of federal Empowerment Zones as failing to deliver on the goal of helping low-income families in these areas and make an important addition to the growing literature arguing that we need to assess the distributional effects of public policies—especially those intended to influence the distribution of income (e.g., Neumark et al., 2005a; Bitler et al., 2006). The apparently adverse distributional effects do not necessarily contradict Busso et al.'s estimates of the value created by the Empowerment Zone program. But they certainly raise questions about how to evaluate the gains from a social welfare perspective. Moreover, if the zones generated housing price gains from gentrification, it is entirely possible that there were offsetting housing price effects in areas that were unlikely to be weighted heavily as controls for the tracts designated as Empowerment Zones, as there is no reason to think that the higher skilled in-migrants came from other disadvantaged areas.

An obvious question is why Empowerment Zone designation would have mainly benefitted higher income households, including perhaps enticing some to move into the zones. One possibility is that the block grants were spent on things that had this gentrification effect, rather than on activities or programs that increased job opportunities for low-income zone residents. There does not seem to be a lot of information on how these funds were used, although a 2006 GAO study (US Government Accountability Office, 2006) gives brief summaries of what each of the zones did, noting that some Empowerment Zones and Enterprise Communities focused more on community development than economic opportunity. It also cites some specific examples that could be viewed as having these types of effects, such as contributing financial assistance to a 275,000

[40] The authors' conclusions differ from those of Freedman (2013), who suggests that "Texas' EZ Program had a positive effect on communities, but one that was largely confined to households in the lower end of the income distribution" (p. 340). However, this is not based on as comprehensive a distributional analysis as Reynolds and Rohlin's, but rather seems to derive from evidence of the positive effects discussed earlier, coupled with no effect on median income in the ACS data.

square foot retail complex in Harlem, housing development in Detroit, and cleaning up vacant lots in Philadelphia.

Whether one shares the positive assessment of Empowerment Zones of Busso et al. (2013) or the more negative assessment of Reynolds and Rohlin (2013), it would be useful to learn more about how the large amount of block grant funds was spent, since much other evidence on enterprise zones suggests that the other components of the policy, like hiring credits, had little impact. Of course, the Busso et al. and Reynolds and Rohlin studies focused on only six Empowerment Zones, so it is difficult to imagine making much headway based on differences among these zones and suggesting also that we should be wary of generalizing either set of results. However, the program was expanded to an additional 23 Empowerment Zones (21 new ones plus two that were upgraded) beyond the original six that these two papers study, so there may be more that can be done with the additional zones, although much smaller discretionary sums, rather than large block grants, were made available to these additional zones (Mulock, 2002; US Government Accountability Office, 2006).

Another question is whether enterprise zone hiring credits could be made more effective depending on how they are structured. For example, the hiring credits under the California enterprise zone rules did not do anything to require or verify job growth. In contrast, Ohio's program requires employment growth and that employment was not reduced or a facility closed outside the enterprise zone program to facilitate growth in the zone (although one might wonder how this is determined). It is conceivable that these design features matter. For example, in related work on hiring credits generally, which specifically require job or payroll growth, Neumark and Grijalva (2013) found that allowing states to claw back credits if the job creation goals are not met appears to make these credits more effective. This kind of clawback feature, with payment linked to specified job or investment targets, is used in some of the discretionary subsidy policies discussed later.

18.5.1.3 Evidence from other countries

The French Zones Franches Urbaines (ZFUs) are enterprise zones modeled similarly to US programs. Firms with fewer than 50 employees located within the zones' boundaries are exempt, for 5 years, from local business, corporate, and property taxes, as well as social security contributions on the fraction of salaries lower than 1.4 times the minimum wage; the exemptions are then phased out slowly after the initial 5 years. This program therefore appears remarkably generous and is simple in that it is based solely on location. One might expect that, given that the incentives are tied so strongly to location, the program would have the most impact on births of businesses in the zones or relocations of businesses into the zones.

In studying ZFUs, Givord et al. (2013) addressed the key challenges that arise in evaluating place-based policies. First, the paper uses rich administrative panel data on establishments with information on precise location, the creation date of the business at that location (and whether it was a birth or a relocation), and the date of bankruptcy if that occurred; the precise location information is important because enterprise zone boundaries do not coincide with existing jurisdictional boundaries.[41] The authors had administrative data on employment and salaries, fiscal records of taxes paid, and data on other business outcomes.

Second, the paper uses compelling control groups. The implementation of the ZFU program occurred in three phases. In the first, in 1977, the government defined 44 areas that were granted ZFU status. An additional 416 areas were identified as slightly less distressed economically. Then, in 2004, 41 new ZFUs were created, all chosen from the second group of 416. The authors' identification strategy compares changes in these 41 new ZFUs with changes in some of the remaining groups, known as ZRUs, which were initially identified as distressed, but not designated as ZFUs in the second round.[42] The zones designated in the second round might be expected to be relatively more distressed. But the committee selecting these zones had to follow precise guidelines based on an index calculated from population, unemployment rates, tax revenues, the proportion of youths, and the proportion of dropouts. Given that the authors can construct the same index, they are able to compare the ZFUs designated in the second round with zones having the same index value that were not designated and show that the main selection criterion was that new ZFUs were far from those established in the first round (presumably to achieve a more even geographic distribution) and near to other ZRUs (allowing mergers to achieve the minimum population size). They use propensity score matching to candidates among the ZRUs to estimate treatment effects, matching on the variables used in the index, these distance measures, and other variables including outcomes prior to ZFU designation.[43]

Third, the paper pays close attention to negative spillovers, which we might expect to be particularly important in the case of a program that simply pays incentives based on location. Indeed, this study grapples more directly with relocation than the other recent

[41] Givord et al. focused on single-establishment firms because the tax information is at the firm level, and other data supposed to be at the establishment level are sometimes aggregated across establishments. Since ZFU incentives apply to firms with fewer than 50 employees, focusing on single-establishment firms should capture most affected businesses. The discussion below refers to "businesses," but the meaning is single-establishment firms.

[42] These latter areas, called Zones de Redynamisation Urbaine (ZRUs), also have some tax incentives, but these were negligible during the study period.

[43] In a closely related paper on the same program, using the same data source, Mayer et al. (2012) compared ZFU and non-ZFU areas in the same city. As they argued, this lets them control for city-specific differences in policy, transportation infrastructure, etc. The findings in Mayer et al. are quite similar on many dimensions, both in terms of responsiveness to zone incentives, and the finding in Givord et al. that the positive effects in zones are largely offset by diversion from areas near the zone (in the case of Mayer et al., the rest of the city).

studies. Relocations are of interest as a direct manifestation of the kinds of negative spillovers discussed earlier.

Givord et al. first showed evidence not usually available—that the ZFU program resulted in substantially lower business taxes and social security contributions paid relative to the control areas. (One potential explanation for an absence of employment effects in many US studies is that most companies do not take advantage of the tax credits.) Turning to outcomes, the ZFU program positively affected the number of businesses located in the treated areas, via both births and relocations. The program boosted the number of establishments by about 5–7%. This is due roughly equally to births and relocations, but compared to average relocation versus birth rates, the relative impact on relocations is much higher—about 100% compared to 25%.

When the authors split the sample into existing businesses, there is no evidence of an employment effect; the point estimates range from −6% to 9% and are never significant. Thus, it seems likely that if there was employment growth, it came from the new businesses that moved into the ZFUs, although with the imprecise estimates we cannot be sure. Mayer et al. (2012), in their analysis of the same program, reported results indicating that only establishments with 50 or fewer workers (and with sales below the maximum to be eligible) were affected by the policy, in terms of the decision to locate in a ZFU. (They do not study employment responses among businesses already in the ZFU.)

Finally, given the evidence suggesting that the response is strongest on the relocation margin, Givord et al. explored negative spillovers on nearby areas. These spillovers could also be manifested in business births, because the incentives can influence the decision about where to open a new business. Using a procedure very similar to that of Neumark and Kolko (2010), the authors constructed 300 m rings around their ZFUs and estimate the impact of the ZFU incentives on these areas. Their comparisons are based on similar rings around their control areas (the ZRUs). Interestingly, the results mirror quite closely—albeit with opposite sign—the effects on activity *inside* the ZFUs. Fewer businesses are created (through both the birth and relocation channel, although only the estimates for the latter are statistically significant). And the employment estimates are generally negative and of similar magnitude (in absolute value) to the positive estimates inside the ZFUs, although again insignificant.[44] Although there could be gains from relocation, it seems hard to make the case that there are gains from reallocation of activity over such small geographic areas or that such gains could offset the substantial foregone tax revenue that the study documents.

[44] Mayer et al. (2012) also concluded that the ZFU program mainly led to diversion, with even stronger evidence that the response is centered on the relocation margin. In their analysis, this is based on evidence that the overall flow of establishments into municipalities was not affected by the designation of a ZFU within the municipality.

A recent paper by Briant et al. (2012) extends the analysis of ZFUs to consider heterogeneity in the effects of the program based on geographic features, including access to transportation and barriers and distance between targeted areas and main employment centers. Briant et al. used a simple difference-in-differences strategy based on geographic subunits of municipalities. The effects of ZFU designation vary in ways we might expect with geographic features of the treated areas. In particular, better access to transportation by roads or trains is associated with a larger positive effect in attracting firms. Moreover, some of these geographic features appear to do more to boost the creation of new firms rather than just relocations of existing firms, in contrast to the results for homogeneous treatment effects in Givord et al., which suggested that relocation was the primary response. The paper clearly presents some intriguing findings that bear further study in the context of enterprise zones in France, as well as other countries, and emphasizes the point made with respect to the US literature as well—that we need to learn more about when enterprise zones are more effective or less effective.

18.5.1.4 Summary of evidence on enterprise zones

The research literature on enterprise zones is noteworthy for two reasons. First, it provides a fairly comprehensive examination of the effects of this particular place-based policy. Second, it is illustrative of the kinds of analyses that need to be undertaken to evaluate place-based policies generally, exhibiting careful and creative efforts to address the numerous challenges that arise in evaluating the effects of these programs and—especially in the most recent studies—attention to issues beyond mean effects, which are needed to evaluate the distributional and perhaps even the welfare effects of these policies. In this summary section, we do not reiterate the general research contributions of this literature, which have been highlighted in the earlier discussion. But we do attempt to summarize the findings for enterprise zones specifically.

Research on three specific state programs (California, Florida, and Texas) concludes that two generate no employment effects and the third (on Texas) finds positive effects concentrated on lower pay jobs. One study looking at numerous states also finds some positive employment effects, but they do not appear to be tied in any way to hiring credits. Thus, evidence on whether these state programs created jobs is mixed, while a stronger case can be made that if they did create jobs, it was not because of the hiring credits highlighted in many state enterprise zone programs.

Evidence from analyses of the US federal Empowerment Zones program is also mixed. One study finds strong effects on job growth and wages, whereas another suggests that if we account for endogenous selection of zones, there is no evidence of beneficial effects. Moreover, if there are benefits, they appear to accrue to higher income households. If one concludes that the federal program was beneficial, it seems plausible that the large block grants associated with Empowerment Zones played an important role, although verifying that will be challenging given the small number of affected zones, and these grants may have done more to increase the attractiveness of zones to higher income people. The later round Empowerment Zones did not receive these large block grants. Thus, comparison of the effects of the later round Empowerment Zones with the first-round zones could be informative about these questions.

The evidence on spillovers is also mixed for the United States, with some studies suggesting negative spillovers that offset program benefits. There is evidence of strong spillovers in France, where zones offering significant tax breaks resulted mainly in the relocation of economic activity from nearby areas to inside the zones. There may be reasons policymakers want to relocate economic activity to some areas even if this is solely at the expense of other areas. But clearly the case for place-based policies is harder to make if this is what happens, especially for relocation over small areas. At a minimum, we would presumably want to see evidence of other beneficial effects of this relocation of economic activity. Indeed reflecting the preference for boosting firm births, for example, rather than relocations, some states have inserted provisions that bar relocating businesses from obtaining enterprise zone benefits (Wilder and Rubin (1996) and the current Ohio program).[45]

Finally, theoretical modeling of the effects of place-based policies indicates that the welfare implications depend also on the effects on housing prices and migration responses. On these issues, too, the conclusions are mixed, with Busso et al. (2013) staking out a position that rents do not increase and that there are no compositional shifts, leading to a rather strong positive evaluation, while other research emphasizes housing price increases (which may not only reflect welfare gains but also have distributional consequences), compositional shifts, and gains for higher income households to a much greater extent.

What do we make of all this? We draw two main conclusions. First, it is very hard to make the case that the research establishes the effectiveness of enterprise zones in terms of job creation or welfare gains, although there clearly are some studies pointing to positive effects. Further progress requires effort to figure out what features of these programs can make them more effective, following on some early efforts in this direction in the existing research. Second, although there have been a slew of new studies in the past few years—and even many studies focusing on the same program—there has not been enough of an attempt to reconcile the disparate evidence. This kind of careful, often painstaking, work may well help sharpen the conclusions from a research literature in which the findings remain rather at odds.

18.5.2 Place-based policies that account for network effects[46]

Earlier, we discussed how place-based policies might be more effective if they took advantage of labor-market networks involving the residents of targeted locations. Ladd (1994) echoed this point, suggesting that place-based policies should recognize

[45] See http://development.ohio.gov/bs/bs_oezp.htm (viewed 6 September 2013).
[46] The discussion in this subsection draws heavily on Hellerstein and Neumark (2012).

"the social isolation of many residents in distressed areas" that "results in incomplete knowledge of the labor market and limited exposure to people in the labor market who may serve as the informal contacts needed for successful job searches" (p. 196). This is largely an unexplored area of research, although a study of Jobs-Plus provides some of the first evidence of which we are aware (Riccio, 1999). Jobs-Plus aimed to increase labor supply incentives for public housing residents by reducing the rent increases that accompany increases in earnings. In addition to including employment-related activities and services, Jobs-Plus endeavored to encourage the formation of labor-market networks or to provide functions similar to those supplied by networks. Most sites had "job developers" on staff whose responsibilities included providing outreach to local employers, cultivating relationships with them in an effort to place Jobs-Plus participants in employment (Kato et al., 2003). The program also employed residents as "court captains" or "building captains" who maintained contact with other participants, including sharing information about employment opportunities.

Jobs-Plus had a clear place-based flavor, attempting to transform the community through a saturation strategy that targeted all nondisabled working-age residents of these projects, rather than just trying to change individual behavior. This was based on the network-related (and peer effect-related) theory that saturation can lead to tipping points, creating a critical mass of employed residents who succeed in the workforce. These employed residents would, it was hypothesized, "signal to others the feasibility and benefits of working, elevate and strengthen social norms that encourage work, foster the growth of work-supporting social networks, and ... contribute to still more residents getting and keeping jobs" (Riccio, 1999, p. 13).

There is evidence that the program delivered economic benefits in terms of both earnings and employment (Bloom et al., 2005). It is difficult, however, to draw firm conclusions about the value-added of specific efforts to build labor-market network connections due to two problems: first, the implementation of the network component of Jobs-Plus was spotty and encountered unanticipated difficulties, and second, it is hard to tell which components of the Jobs-Plus program delivered economic gains to its participants.

The reports on Jobs-Plus are replete with discussions of the problems encountered regarding building and strengthening networks. For example, Kato et al. (2003) noted that community support for work was the slowest component to develop (p. iii) and one site never developed it (p. 3). Bloom et al. (2005) noted, with regard to community support for work, that "[a]lthough many of these kinds of activities were tried at some points during the demonstration ... most did not take root. What did take root—and grow—was the idea of using a small group of residents as extension agents of Jobs-Plus" (p. 48). Issues also arose regarding difficulties in doing outreach owing to high levels of illicit activity in some developments. Residents interested in working sometimes reported that a desire to stay out of trouble, combined with criminal activity among residents, "discouraged them from interacting with other residents, for fear that their neighbors might be complicit" (Kato, 2004, p. 30). More to the point with regard to networks, there was a concern that someone you might refer could reflect badly on you.[47] Despite these difficulties, however, the description of implementation reveals numerous cases of job developers and sometimes captains developing means of linking residents to employment opportunities, likely providing labor-market contacts that many of the participants were lacking.

It is, however, difficult to attribute the gains from the Jobs-Plus program specifically to the network efforts, because the evaluations focused on the overall success of the program, rather than parsing out the effects of the separate components. Thus, at the end of the day, it is difficult to point to the evidence from Jobs-Plus as establishing the productivity of place-based policy efforts focusing on strengthening labor-market network ties. It is possible that most of the effect of Jobs-Plus came from the increased financial incentives to work generated by restructuring the effects of earning increases on rent or other components of the program.

The discussion in this section thus far has focused on how network effects might be leveraged to *help* residents of low-income, urban labor markets. There is, however, an alternative perspective, whereby networks can diminish the effectiveness of other policies. For example, as noted earlier with respect to enterprise zones, such policies may be ineffective at improving local labor markets because businesses may not hire locals in these neighborhoods. Dickens (1999) echoed this concern (p. 394), citing some case-study evidence from Kasinitz and Rosenberg (1996) suggesting that employers may even *prefer* to hire those who live farther away, because employers were worried that locals would have trouble avoiding family problems at work and could be pressured by locals to help burglarize them.

Thus, networks can be a two-edged sword, not only reducing labor-market search frictions by increasing the flow of information about jobs but also potentially introducing rigidities by making continued hiring within a particular network lower cost than going outside the network, even though the latter may be necessary to deliver benefits to residents of target areas. We believe that the design and evaluation of place-based policies that try to leverage labor-market networks to improve job market outcomes for local residents are a high priority for research and policy alike.

18.5.3 Discretionary grant-based policies

The specific aims of discretionary grant-based policies often include attracting new firms and investment to an area and either job creation or the prevention of job losses. Research

[47] This parallels findings from more systematic research by Smith (2005), who, based on in-depth interviews of low-income blacks, concludes: "Over 80 percent of respondents . . . expressed concern that job seekers in their networks were too unmotivated to accept assistance, required great expenditures of time and emotional energy, or acted too irresponsibly on the job, thereby jeopardizing contacts' own reputations in the eyes of employers and negatively affecting their already-tenuous labor market prospects" (p. 3).

has addressed whether these policies achieve these aims by evaluating effects on the targeted outcomes for subsidized firms. In some cases, a somewhat broader analysis of the welfare effects of these programs has been attempted, for example looking at effects on the productivity of participant firms and of other firms whose performance might be affected by agglomeration externalities, as well as looking at employment displacement across areas. Much of this research faces a similar problem to that confronted by the literature on enterprise zones—in this case, that the eligible areas, in particular for countries in the European Union, are simultaneously eligible for other sources of regional development assistance. It is therefore crucial to control for these other sources of support to isolate the effects of discretionary grant policies.

A number of papers have examined whether these subsidies are effective in attracting new entrants. Two, for France and the United Kingdom, estimate location choice models using firm microdata and find some evidence of statistically significant but very small effects of discretionary grants on firm location decisions. Crozet et al. (2004) estimated conditional logit and nested logit models to examine the effect of the Prime d'Aménagement du Territoire (PAT) on foreign multinational location decisions across NUTS3 areas (départements) in France.[48] PAT grants aim to create or safeguard employment in lagging regions, and the authors reported that around half of annual expenditure under this scheme went to foreign-owned firms. The authors measured the generosity of the program at each potential location using data on the allocation of grant funding at the broader NUTS2 region-year level. While they find some evidence of positive effects of these grants on location choice, this is not highly robust, and even where there is a positive effect, its magnitude is dwarfed by that of other factors such as market access and agglomeration externalities. They also examine the effect of EU regional policy funds (covered in Section 18.5.5), which may affect the attractiveness of locations for investment, but find no evidence that this EU expenditure acted to boost the appeal of the qualifying locations for foreign investors.

Devereux et al. (2007) analyzed the effect of the similar UK RSA scheme on firm location decisions. The policy provides subsidies to firms for new investment in physical capital, with an ultimate aim of creating and safeguarding jobs and attracting FDI. The scheme aims to subsidize only additional investment that is attributable to the funding. Firms apply for a subsidy under the scheme and the application is assessed before a subsidy offer—conditional on job targets being met—is made. The funding is only available in designated Assisted Areas, and the rate of the subsidy is governed by EU rules and varies

[48] The European Commission defines three levels of regional units, under the nomenclature des unités territoriales statistiques (NUTS) classification. NUTS1 regions are the largest with populations of 3–7 million, for example, England, Scotland, and Wales, within Great Britain. NUTS2 regions are groups of administrative regions within countries containing 0.8–3 million inhabitants, while NUTS3 regions contain 150–800 thousand inhabitants.

with area characteristics. While Crozet et al. used area-level data on grant expenditure, Devereux et al. used microdata on the value of subsidies offered to individual firms to form a prediction of the subsidy that each new entrant might expect to receive in each location according to their characteristics.[49] They then use these predicted subsidy offers in a conditional logit location choice model across counties within Great Britain. The study finds that the grants had a positive and statistically significant effect on the location choice of new entrants (both multinationals and new plants established by existing UK manufacturing firms). However, the magnitude of the effect was extremely small. Grants were found to be more effective in locations that had greater preexisting employment in the relevant industry—i.e., their impact on location choices was magnified in areas that might also benefit from localization economies—but the overall effect remained very small.

Overall, the evidence from both studies suggests that such incentives have little leverage in terms of influencing where firms choose to locate, a conclusion that has implications for the use and cost-effectiveness of this type of policy as a tool to try and create or enhance industrial clusters. In addition, it also suggests that unlike the evidence for enterprise zones, this type of discretionary, plant-specific and potentially heavily monitored grant policy is unlikely to result in significant relocation of activity into eligible areas. Indeed, in the United Kingdom, the policy rules have explicitly included as a requirement that the grant is only awarded if it does not involve the displacement of employment, for example, across plants in different locations within a firm.

Criscuolo et al. (2012) evaluated the same policy as Devereux et al. (2007), but analyzed its effect on employment, investment, and productivity, including displacement effects across areas. Their IV evaluation strategy was outlined in Section 18.4.5. Their findings suggest that RSA grants are effective in generating both additional employment and investment but that they do not increase productivity. Using plant-level panel data, they find a positive effect of receipt of a grant on employment. The magnitude of the estimated effect increases when they instrument participation in the RSA program with their exogenous policy instrument—the maximum subsidy rate mandated by the EU, which varies over time and by area and is zero in noneligible areas. The downward bias in the OLS estimates is consistent with the idea that there is selection both into applying to the scheme and in the successful award of a grant, by firms facing negative shocks to performance. Their estimates suggest that participation in the RSA scheme increases plant-level employment substantially, by 43%, where the mean and median plant sizes in their sample are 79 and 6 employees, respectively. They also investigate heterogeneity in the effects of the policy across plants owned by firms of different sizes. Their results

[49] They also estimate a first-stage selection equation to address the fact that in practice only certain firms apply and that the expected grant for each new entrant in each location can only be estimated from data on a set of firms that applied to the scheme and were successful in receiving an offer.

imply that positive effects are confined to plants that are part of smaller firms with fewer than 150 employees. While this is potentially in line with the idea that smaller firms are more likely to face financial constraints, which a subsidy to capital investment can alleviate, it might also be that larger firms are better able to capture rents under the scheme.

They then estimate the impact of the policy on investment and productivity at the firm level. They find positive effects on investment, which are again restricted to smaller firms. Moreover, the estimated impacts on investment are greater than the impacts on employment, in line with the design of the policy, which provides a direct subsidy to capital. There is no evidence that the policy led to increased TFP or average wages at the firm level. Therefore, since their descriptive statistics of prior characteristics show that it is less productive firms that typically receive the subsidies and since the scheme acts to increase employment in these firms, this implies a negative effect on aggregate productivity. Given the design of the policy—a subsidy to capital combined with employment targets to either create or safeguard jobs—the authors rightly argued that while the subsidy may lead to an increase in capital per worker, there is no ex ante reason for it to lead to increased TFP.

Criscuolo et al. then analyzed impacts at the area level and found that the policy acted to increase employment both within existing firms and via new firm entry, and reduced unemployment in these lagging areas. They also found little evidence of displacement effects on nonparticipant firms or for noneligible but neighboring areas. The only evidence they found that is suggestive of displacement is for plants that are part of larger firms, which is in line with larger, multiplant firms having more flexibility to move plants or employment from outside to inside an eligible area. Since some of the areas in which these grants are available also qualify for EU Structural Funds, the authors included an indicator variable for areas receiving these transfers, but found no evidence that this alternative funding acted to increase employment. Moreover, controlling for the presence of this other source of regional support did not affect the estimates of the impact of the RSA scheme.

Although they did not conduct a full cost–benefit analysis of the policy, the authors performed some additional calculations on the magnitudes of their estimated effects. Their results suggest that a 10% subsidy rate increases area employment by nearly 2.9%. At the average subsidy rate of 24%, this translates to an increase of around 111,000 jobs over the period 1986–2004 (from a basis of 1.6 million employees in manufacturing), at an estimated cost per job of nearly £4900 in 2010 prices. They reported that this is lower than the previous estimates of the cost per job under the policy and argued that this is because their IV strategy overcomes the downward bias affecting OLS results. Further calculations for the estimated number of individuals drawn out of unemployment are close to those for the number of additional jobs created, again suggesting that the policy is primarily acting to reduce unemployment rather than displacing employment from other firms or areas.

A similar subsidy program known as Law 488 exists in Italy. The policy aims to create new jobs via a subsidy to investment projects, for example, start-up physical capital investment or modernization or relocation projects. Applicant firms must specify the number of new jobs to be created alongside details of the investment project. The creation of these new jobs is binding on recipient firms in that if fewer individuals are employed, the entire value of the subsidy must be repaid. In addition, the number of jobs to be created per unit of investment is one of the criteria used to score and subsequently rank each application in terms of priority for funding. The other two main criteria are the share of project costs borne by the firm and how high the rate of subsidy requested is relative to the maximum rate allowable in the area under EU regulations (Bernini and Pellegrini, 2011, p. 254).

Bernini and Pellegrini (2011) evaluated the impact of this policy on the performance of subsidized firms using a difference-in-differences estimator with unsuccessful applicants as controls. They considered a wide set of outcome variables, including growth in output, value-added, employment, capital investment, and measures of profitability and productivity. Using data from the year before the application for the subsidy and for the project completion year, they found strong evidence that while the subsidies acted to increase output, employment, and investment growth, growth in output increased to a lesser extent than employment, and both labor productivity and TFP growth declined. Their estimates suggest that, over on average a 3.6-year period, total output growth was around 8–10% higher in subsidized firms, while employment growth was around 16–17% higher. Growth in physical capital was even higher at around 40%. Growth in labor productivity as measured by output per worker was found to be 7% lower, and TFP growth 8% lower, as a result of the subsidies. Similar to Criscuolo et al., they also found evidence that the effects on output and employment growth were greater for small firms, some evidence of lower labor productivity growth, but no evidence of lower TFP growth for this group.

While the evidence implies that the program generated positive effects on the targeted outcomes of capital investment and employment, the negative effects on productivity may be due to distortions brought about by the policy design. The subsidy may have induced firms to undertake investment projects that would not have been funded by external capital markets. If firms were not subject to external financing constraints in the absence of the policy and were optimally not choosing to undertake investment with an insufficiently high return, then any genuinely additional investment brought about by the subsidy may have been relatively unproductive. The combination of a subsidy to capital investment coupled with an explicit target for job creation (and a potential incentive to overstate the number of new jobs plus a subsequent requirement to meet this target) may create distortions to firms' optimal capital–labor ratios. In addition, the design of the scheme may even have created incentives to recruit relatively low-productivity, low-wage workers to meet the binding employment targets, potentially implying a trade-off between the stated employment goals of the program and any productivity objectives (although perhaps with positive distributional effects). It may also be the case that nonrecipient firms had stronger incentives to increase their own efficiency, potentially due to increased competition from the subsidized firms.

Bronzini and de Blasio (2006) investigated whether Law 488 generated additional investment, focusing on the time profile of investment in subsidized firms relative to unsuccessful applicants. They found evidence of an increase in investment in subsidized firms 2 years after the subsidy was awarded. This is exactly the point at which firms undertaking a 2-year investment project under the scheme would receive the second half of their funds and at which firms undertaking a longer project would receive the second third of the subsidy. The payment of these installments is conditional on firms having undertaken their specified investment projects, and hence, this is the exact point at which a positive impact would be expected. However, looking over a longer time horizon, they found that recipient firms show a decrease in investment relative to the control group at 5 years, i.e., 2–3 years after the final subsidy payment is made.

The authors interpreted this as evidence that the policy may not in fact have generated additional investment, but merely brought forward investment that would have happened eventually. Potentially in line with this argument, they found that at the 5-year point, subsidized firms exhibit lower debt, which is suggestive of firms bringing forward investment and substituting public funds for private borrowing that would otherwise finance investment at a later date. One issue is how to square the findings with those of Bernini and Pellegrini (2011), who found a larger effect on capital investment. One explanation is that the latter used information on the exact completion date of the project and did not look at investment beyond that point; by conducting the analysis over a slightly shorter time period, they miss a fall in investment relative to the control group after the subsidy has been paid.

Some policies aim explicitly at attracting very substantial new investment and influencing where large, new entrants locate. The PAT and RSA schemes discussed above have an element of this, for example, in using multimillion dollar investment grants to attract internationally mobile FDI. Greenstone et al. (2010) evaluated the effects of subsidies to attract large plants to specific US counties. Rather than focusing on the effects on the recipient plant, they looked for evidence that the new plant generates positive agglomeration externalities or productivity spillovers, the existence of which would potentially provide a rationale for the use of such subsidies. As discussed in Section 18.4.5, the study uses information on counties that narrowly missed out on winning the plant as counterfactual locations. The authors found convincing evidence that the arrival of these large new plants led to higher TFP among incumbent plants, higher net entry of new plants, and an increase in county-level labor costs, with the latter implying that part of the productivity spillover accrues to workers in the form of higher wages, rather than simply leading to increased profits for incumbent firms. Spillover effects on incumbent TFP are identified by estimating difference-in-differences specifications of plant-level production functions, which compare TFP growth in incumbent plants before and after the plant opening across paired winning and losing counties. Effects on wages are estimated using similar difference-in-differences specifications on individual-level wage data, controlling for worker characteristics.

While the main aim of the paper is to credibly identify agglomeration externalities in the context of a large-scale plant opening, the paper also provides evidence on the benefits of the subsidies. However, since the authors did not use information on the value of the subsidies given to these plants, the findings are not used in a full cost–benefit analysis. The authors found that 5 years after the opening of a new manufacturing plant, compared to plants in losing counties, incumbent plants in the winning location had on average around 12% higher TFP (equivalent to moving from the 10th to the 27th percentile of the observed TFP distribution across counties), evidence that strongly supports the existence of externalities and of substantial efficiency benefits stemming from the use of such subsidies. The authors translated this average effect into an increase of around $430 million in incumbent plant output after 5 years. The average effect on wages is estimated to be an increase of around 2.7%. Using an equivalent econometric specification for productivity spillovers that implies an effect on incumbent plant TFP of 4.8%, a 23% share of labor in total costs implies that the estimated overall cost effect via higher wages is around 13% of the TFP increase. These estimated effects reflect the direct result of the opening of the new plant, as well as indirect effects through subsequent plant entry and any changes in the industrial mix of the area, together with changes in competition for inputs that will also influence the extent of spillovers to incumbent plants and input prices.

The average productivity spillover effect is perhaps not the most useful parameter for a policymaker. The authors did not find that *all* winning locations benefit; indeed, for some counties, the estimated spillover effects are negative. The paper investigates how the magnitude of externalities varies depending on a set of measures of economic proximity between the subsidized plant and incumbents. These measures are intended to capture the classic channels through which theory and other evidence suggest that spillovers might be stronger, namely, labor-market pooling or thick labor markets at the industry level, knowledge spillovers, and input–output linkages (the presence of thick markets for intermediate inputs that can benefit firms in vertically related industries). The results support the existence of heterogeneous effects across incumbents with different characteristics relative to the new plant, but not via all three mechanisms. Spillover benefits appear to be stronger for incumbents in industries that share labor with the industry of the new plant and also for those that have technological connections, but not for incumbent plants in industries that have upstream or downstream linkages.

A further point the authors made is that if increases in labor costs occur across all industries within a winning county, but the size of productivity spillovers are industry-specific, then the effects on incumbent plants' profits can be highly asymmetrical. The existence of such asymmetrical effects could then explain the attractiveness of certain locations for specific sectors and the observed longevity of industrial clusters. An additional issue with regard to the scope of benefits that is not covered in the analysis, but is relevant to policymakers concerned with place, is whether heterogeneous effects occur not only according to economic proximity but also geographic proximity to the subsidized plant, in terms of the spatial scale on which spillover benefits accrue.

If, as the results suggest, the externalities generated by new plant openings are heterogeneous across locations, a policy where local areas compete to attract such plants could be welfare-enhancing at an aggregate and at a local level. In these circumstances, the payment of subsidies to firms, which mean that they internalize the externalities they generate in their location decisions, can increase efficiency. However, from a cost–benefit perspective, this argument also relies on local policy authorities being able to accurately assess the scale of future benefits arising from the establishment of new plants and being able to judge how much to bid. As the results in Greenstone et al. indicate, the estimated productivity spillovers arising from these new plant openings have not always been positive.

18.5.3.1 Summary of evidence on discretionary grants

The literature looking at the effects of discretionary programs has been creative in dealing with the additional identification problem posed by the selection of businesses into the schemes, as well as dealing with issues of location-based selection common to place-based policies that almost by definition target a highly nonrandom set of areas (see Section 18.4.5). Overall, the available evidence on the effects of discretionary subsidies on their targeted outcomes of investment and employment in recipient firms is positive; Table 18.3 provides an overview of the findings. The fact that plants that receive subsidy offers have their applications pass through an initial scrutiny process, and that the targeted outcomes are often heavily monitored and that payment of the subsidy is contingent on the job and/or investment targets being met, may explain why these policies appear more successful in achieving their stated goals than, for example, enterprise zone programs. But close monitoring of these discretionary schemes will come at an administrative cost, and it would be instructive to have further evidence on how long the additional jobs created actually survived once the monitoring period expired, to form a better assessment of their longer run impact on employment.

The evidence also suggests that the design of some schemes might itself create distortions to firms' optimal capital–labor ratios and to productivity, with associated welfare effects. Indeed, to the extent that the schemes are designed to finance marginal investment projects that, absent any capital-market failures, would not be backed by private-sector finance, the subsidized investment may be relatively unproductive. Overall, the evidence does not provide any hint that these grants acted to improve productivity at

Table 18.3 Summary of evidence on discretionary grants

Study	Country	Program	Results
Crozet et al. (2004)	France	Prime d'Aménagement du Territoire	Small, nonrobust effects of PAT subsidies on foreign multinational firm location decisions
Devereux et al. (2007)	United Kingdom	Regional Selective Assistance	Small effects on location decisions of foreign multinational firms and domestic multiplant firms Heterogeneity in the effectiveness of grants in influencing location choice; grants having a greater effect in areas with higher existing employment in the firm's industry
Criscuolo et al. (2012)	United Kingdom	Regional Selective Assistance	Positive effects on plant employment (43% increase in employment for participant plants) and firm investment, but restricted to plants that are part of smaller firms (<150 firm employees); no evidence of effects on firm TFP or wages Positive effects on employment and number of plants at the area level (a 10% subsidy rate increases area employment by 2.9%) and negative effects on unemployment (a 10% subsidy rate reduces unemployment by 6.9%) No evidence, on average, of employment or plant displacement from noneligible to eligible areas, but some evidence of displacement for plants that are part of larger firms
Bernini and Pellegrini (2011)	Italy	Law 488	Output growth in subsidized firms around 8–10% higher over on average 3.6 years, employment growth 16–17% higher, and growth in physical capital around 40% higher; labor productivity growth and TFP growth 7% and 8% lower, respectively Effects on output and employment appear to be greater for small firms
Bronzini and de Blasio (2006)	Italy	Law 488	Increase in investment over the initial 2 years following receipt of the subsidy, but at 5 years, recipient firms show a decrease in investment relative to controls; program may act to bring forward investment that might otherwise have occurred at a later date, rather than subsidizing additional investment
Greenstone et al. (2010)	United States	Location subsidies for large plant entry	Substantial effects on incumbent plant productivity in successful locations; incumbent plant TFP 12% higher after 5 years Heterogeneity in magnitude of TFP effects across industries and across locations Positive effect on county-level wages (2.7%)

recipient plants, and if, as in the case of the UK evidence, recipient firms had relatively low productivity to start with and then subsequently expanded, this implies a negative effect on productivity in the aggregate.

Evidence from the United States does, however, imply that the use of subsidies to influence the location of very large new plants can result in productivity spillovers to incumbent firms. It is likely that the areas offering subsidies in the United States are quite different to those typically targeted in a European context. Greenstone et al. reported that US counties that win a new plant have superior economic characteristics, for example, in terms of incomes, population growth, and labor force participation rates, compared with the rest of the country, which could reflect Glaeser and Gottlieb's (2008) suggestion, discussed in Section 18.2.7, that efficient place-based policies may increase disparities between areas. In contrast, within the EU, the opposite is true, with subsidies applying to more economically distressed areas. In fact, even the Greenstone et al. results illustrate significant heterogeneity in the magnitude of the estimated spillover effects both across locations and across industries within those locations, implying that any benefits of the policy that are external to the recipient firm and that might provide a theoretical justification for the use of such subsidies may be highly contingent on local economic characteristics.

18.5.4 Clusters and universities

Some countries have tried to use the placement or promotion of universities as a tool to enhance local economic development. It is easy to point out that world-leading universities are typically located in economically thriving regions and cities, and many of the most famous examples of high-tech clusters are explicitly linked to, or at least located in the vicinity of, prestigious research universities, such as Silicon Valley, and Silicon Fen around Cambridge University (see also Carlino et al., 2012). But causal estimates of the effects of university presence on local economic performance are relatively scarce, and what evidence there is—which is primarily based on established universities in relatively affluent regions—does not necessarily translate to a lagging-region context.

More generally, the observation that within a number of countries, employment in a wide range of industries is geographically localized, together with evidence that such clusters appear to generate productivity spillovers, has led to debate about whether there is a role for policy in promoting cluster development. Some countries, such as France, have explicitly adopted clusters policies, whereas others have emphasized industrial clustering as part of an enterprise zone program, for example, the 2011 program in England.

18.5.4.1 Clusters policies

This section discusses the scant evidence base on the effectiveness of cluster policy initiatives that aim to improve cluster performance, and on policies that, as a side effect, may have influenced cluster development. Martin et al. (2011) evaluated the effects of a policy aimed at boosting the productivity of existing clusters in France. The program, Local Productive Systems (LPS), provided funds to aid collaboration and cooperation between groups of firms in the same industry in the same local area. Projects typically operated at the geographic level of a département (administrative areas) or a smaller employment area (local labor-market areas). An application to the program was normally made by a local public authority, and if successful, firms then signed up to participate. Projects involved the establishment of local initiatives, for example, to boost the profile of the cluster or to promote exports. The level of subsidy provided under the program was relatively small, at an average of around 40,000 euros per project. The policy ran from 1999 to the mid-2000s, when it was replaced in 2005 by a larger scale policy known as "competitiveness clusters."

The authors used an administrative dataset to identify a set of firms that participated in the LPS scheme. They used difference-in-differences and matching approaches to estimate whether the policy had any discernible effects on firm performance, such as TFP and employment. They also provided evidence on the types of firms that chose to participate. The scheme was not explicitly aimed at supporting underperforming regions and industries, but in practice, this is what occurred. Although the LPS participant firms were relatively large, they were less productive, with the latter driven by industry and location characteristics. For example, the authors found that LPS firms tended to be located in areas eligible for the PAT grant program outlined in Section 18.5.3 above.

The estimates provide little evidence of a positive effect of the policy on TFP in LPS firms and, if anything, point towards TFP falling postparticipation relative to the controls. In a separate examination of preprogram trends, the authors found that TFP in participating firms appeared to be declining relative to nonparticipants even before receipt of the funding, which suggests that this might have driven their decision to engage with the scheme. At best, the results suggest that participation might have brought about a temporary halt to this decline in TFP. The authors looked at other adjustment margins but found no effect on employment growth or on firm-level exports.

The authors acknowledged that there may be spillover effects from participant to nonparticipant firms at least within region, which may affect the firm-level estimates. They therefore carried out a second estimation exercise at the industry-département level. An industry-area is defined as participating in LPS if it contains at least one LPS firm. The results primarily confirm the picture from the firm-level estimates, the only difference being some evidence of an increase in exports, although this is not robust across specifications. The paper also finds no evidence that firm survival rates were higher in industry-areas receiving funding. Overall, the results paint a rather negative picture of the impact of the scheme. Potential explanations include that the program was too focused on lagging regions, the development of which was an objective of the government agency that administered the policy, possible rent-seeking activity by firms, and insufficient funding per project to generate discernible effects.

Falck et al. (2010) analyzed the effects of a cluster policy in the state of Bavaria in Germany, which was more directly focused on innovation in high-tech sectors. From 1999 to 2004, the Bavarian High-Tech Offensive targeted businesses operating in five technological sectors including life science, information and communication technology, and environmental technologies. Over this period, 1.35 billion euros were spent on initiatives aimed at improving firm performance and enhancing public-sector research in these fields, with around 50% of the budget spent on the latter. For example, funds were spent on public-sector research infrastructure with an explicit aim of enabling private-sector firms to access the facilities. In addition, expenditure was targeted at improving networks and cooperation between firms operating within the same technological field within the state, and state-backed venture capital funding was also made available. Other initiatives included science parks where innovative firms could locate rent-free.

The evaluation uses a difference-in-difference-in-differences design comparing changes in firm-level innovation outcomes before and after the program in affected industries in Bavaria to the before–after change in the same industries outside Bavaria, relative to the corresponding comparison of changes in nonaffected industries. The authors estimated effects on three outcomes: an indicator of whether or not a firm has introduced a product or process innovation, an indicator of whether an associated patent application was filed, and log R&D expenditures. The results imply a positive effect of the cluster initiative on the first two outcomes, but a negative effect on the third. The likelihood of innovating in the targeted sectors increased by around 4.6 percentage points from a baseline of roughly 50%, with the probability of filing for a patent increasing by 5.7 percentage points. However, the results imply that R&D expenditures fell substantially, by around 19%. Although at a first glance this appears a contradictory set of findings, Falck et al. suggested that because R&D expenditures are a measure of innovation inputs, the result could be interpreted as a reduction in the costs of innovation.

The paper also tries to address how the policy might have led to what appear to be improvements in firms' innovation productivity. Using answers to survey questions on the innovation process, they find that affected firms were less likely to report that their innovation activity was hampered by a lack of opportunities for cooperation with public research institutions, a lack of access to external expertise, or difficulties in finding R&D personnel. This suggests the program was successful in enabling private-sector access to public-sector scientific research and that it may have increased network connections between firms.

Some policies may indirectly affect cluster formation and development. Fallick et al. (2006) provided evidence that for the computer industry, aspects of California state law may have aided labor mobility, which in turn may have led to increased human capital externalities and knowledge spillovers as sources of agglomeration economies. Greater flexibility to move between employers should result in a more efficient allocation of human capital and talent across firms, and potentially as a by-product increase inter-firm diffusion of knowledge. This latter effect could reduce individual firms' returns to investment in innovation and in employee human capital and provide an incentive for them to use noncompete contracts to try to reduce labor mobility and reduce knowledge spillovers to competitor firms. However, unique historical features of California state law make such contracts unenforceable.

In support of the influence of state law, the paper finds that month-to-month transition rates to a new job for college-educated men are higher in the Silicon Valley cluster and other computer clusters within the state, than they are in computer industry clusters outside the state of California. However, the same pattern is not found for other industries, for which there is no evidence of significantly higher job transition rates in California. The authors developed a model under which, if the nature of the innovation process is one in which success is highly uncertain but with high return, it may be optimal to have many companies working independently on the same problem, such as within a cluster, and then have an ex post reallocation of labor to the successful innovator. They argued that the characteristics of innovation in the computer industry fit with this model. While only offering circumstantial evidence, the results are in line with California state labor law acting to aid labor mobility and increase knowledge spillovers.

18.5.4.2 Universities

While Carlino and Kerr (2015) cover agglomeration and innovation more generally, here, we focus on evidence on the effects of universities on private-sector activity, including industrial clusters and wider effects on local employment and wages. The main empirical challenges in assessing the impact of universities on local economic conditions come from the fact that the location, characteristics, and scale of universities are nonrandom across geographic space; that factors such as business innovation and productivity and the industrial and skill composition of an area will very likely themselves influence university performance; and that unobservable local productivity shocks will likely affect both private-sector activity and university-sector activity.

Kantor and Whalley (2014) attempted to overcome these identification issues using an IV strategy to analyze the effect of knowledge spillovers from universities on noneducation-sector labor income in US urban counties. Their empirical analysis examines the relationship between noneducation-sector wages and university expenditure. As an instrument for the latter, they exploited exogenous variation brought about by national stock market shocks, which affect the market value of universities' endowments and, through this, university expenditure.

The authors used a county–industry panel from 1981 to 1996, with a basic regression specification relating the long difference, $t-x$ to t, in log non-education labor income at the county–industry level, to the long difference in per capita university expenditure at the county level, and a set of year fixed effects. The instrument for changes in per capita university expenditure is constructed as an interaction between the lagged market value of universities' endowments within a county (dated $t-x-1$) and the change in the S&P

500 Index over the period $t - x$ to t. Since universities follow fixed expenditure rules, spending a set fraction of around 4–5% of the market value of their endowments per annum, shocks to the stock market will directly, and differentially, affect expenditure for each university, dependent on the initial value of their endowment. The national stock market shocks, and lagged university endowment levels, must also be exogenous with respect to growth in local noneducation labor income.[50]

The IV estimates imply that an increase in university expenditure of 10% increases wages for workers in the noneducation sector by 0.8%. However, this relatively small average effect masks some heterogeneity. The authors distinguished responses across different types of industries, focusing on those with closer links to the university sector and across counties with more or less research-intensive universities. They found that industries that are technologically closer to university research (measured by the propensity for industry patents to cite university patents), and hence more likely to benefit from knowledge spillovers from higher education, receive higher spillovers, as do industries that employ a higher fraction of graduates, and industries with higher worker transitions into and out of the higher education sector. In addition, locations with more research-intensive universities (measured by the fraction of university students who are graduate level) generate higher spillovers. Their results suggest that the skill and industry mix of an area and the type of university it hosts are important determinants of the degree to which locations can both benefit from, and generate, externalities from higher education. This plausible pattern of heterogeneity in effects suggests that the results are more likely to be causal.

More descriptive evidence consistent with effects of universities on industrial clusters includes Abramovsky et al. (2007) and Abramovsky and Simpson (2011), who analyzed whether the presence of universities, and in particular highly rated university research departments within universities, is associated with the location of firms' R&D facilities. They exploited data from the UK Research Assessment Exercise, which is used to allocate the main publicly funded grant for research and which provides ratings of individual university research departments, with the highest rated departments deemed to be carrying out internationally leading, frontier research. The authors linked this information at the area level to data on private-sector R&D labs. They found that for some industries, but by no means all, the geographic distribution of R&D labs is skewed towards locations with highly rated, industrially relevant university research departments. For example, Abramovsky and Simpson (2011) found that pharmaceutical firms tend to locate their R&D labs disproportionately within 10 km of world-class chemistry research departments. Similar evidence is found for R&D service firms—many of which serve the

[50] The authors carry out robustness checks to address the issue of unobservables that might be correlated with past endowment values and affect future income growth: e.g., the possibility that firms are differentially affected by stock market shocks and that the location of particular types of firms is correlated with initial endowment values.

pharmaceutical industry. However, their results cannot be interpreted as causal. They found that many of these positive relationships can be explained by the presence of science parks. While it is quite likely that the science parks result from the universities' research standing and also from private-sector demand for premises in proximity to these universities, it remains difficult to untangle the direction of causality.[51]

One study that assesses the effects of a deliberate policy of using the geographic location of higher education as a lever for regional development is Andersson et al. (2004). From 1987, Sweden engaged in a substantial expansion of higher education that involved an increase in student enrollment, the establishment of new colleges, and four existing colleges gaining university status. Part of the aim was to make higher education less centralized and more available to students across all locations in Sweden including more remote regions, with a view to increasing participation. However, the policy could also have had effects on the demand for skilled labor in these regions through increased employment in the higher education sector and, depending on migration patterns, the supply of graduates in these locations. It may also have generated effects through innovation or human capital externalities from higher education to the private sector.

Andersson et al. estimated the effects of higher education presence on local labor productivity (output per worker) using a municipality-level panel. Identification comes from within-municipality variation over time in two measures of the scale of higher education in the area—the number of researchers employed at higher education institutions and the number of students enrolled. The authors differentiated between newer and older established institutions (the original six universities in operation) and considered whether effects vary with geographic distance. Overall, they found evidence in line with positive effects on local labor productivity from the expansion of higher education, that the effects associated with university researchers are greater than those associated with expansion of student numbers, that the effects are greater with respect to newer institutions for both measures of university presence, and finally that the effects are strongly spatially concentrated. The latter results imply that over half of the estimated productivity gains accrue within 20 km of the border of the institution's host municipality. However, the study is unable to disentangle the underlying causes of effects on labor productivity. In principle, one would want, for example, to be able to distinguish between effects driven by changes in the composition of the workforce (for example, through increased skill levels) or through externalities from spillovers from university research leading to higher noneducation-sector TFP.

[51] Other evidence in a similar vein includes Woodward et al. (2006), who found a positive but fairly weak relationship between proximity to university research, measured by total university R&D expenditure in science and engineering, and numbers of high-tech start-ups. Unlike Abramovsky et al. (2007) and Abramovsky and Simpson (2011), who found considerable heterogeneity across industries, the Woodward et al. findings are consistent with knowledge spillovers from university research across a number of high-tech sectors.

The same authors go some way to addressing this in Andersson et al. (2009) and revisit the effects of the policy. They extend the analysis to look at innovation as measured by patents granted and look at aggregate effects on labor productivity and output. Given that the locations chosen for university expansion may have been related to underlying economic variables such as their scope for future productivity growth, the paper also presents IV estimates, instrumenting for university presence and scale with measures of preexisting facilities in the area including nursing schools and military facilities (since the buildings were used as sites for the new institutions), the fraction of the local population turning 18, and the fraction of voters voting for different political parties (some of which were strong supporters of university decentralization).

Their findings for labor productivity are in line with the previous paper, with gains if anything estimated panel data to be even more localized. In addition, they estimated panel data count models relating numbers of patents granted in an area, which might better capture knowledge spillovers from university research, to numbers of research staff employed at old and new institutions. The results suggest a positive relationship between the measures of investment in higher education and innovative outcomes. Effects for both labor productivity and innovation are found to be increasing in the fraction of individuals in the area who hold doctorates (measured contemporaneously). This suggests that the benefits of the decentralization policy were asymmetrical across areas according to their human capital endowments, although the latter may also have evolved endogenously because of the policy.

As a final exercise, the paper estimates the net innovation and productivity gains at the national level by constructing a no-decentralization counterfactual whereby researchers based at new institutions created post-1987 are allocated proportionately to preexisting institutions and recalculating the levels of innovation and productivity in each region. The results of this exercise imply an aggregate zero effect on patents generated, suggesting that the spatial redistribution of university research staff did not lead to any aggregate gain in innovative activity. However, using the labor productivity estimates, the authors backed out an estimate of GDP gains from the policy of between 0.01% and 0.10%. While the authors did not attempt to reconcile these findings, the findings would seem to imply that the aggregate GDP gains are driven by human capital improvements and agglomeration externalities working through channels other than purely innovation. However, the noncitation-weighted patent measure will likely be an imperfect measure of innovative activity, since different industries will exhibit highly different propensities to patent and not all patents are of equal value; hence, it remains possible that the policy did result in additional innovation in the aggregate.

18.5.4.3 Summary of evidence on clusters and universities

The evidence on higher education institutions suggests that areas do benefit from productivity spillovers but that these may be highly localized and also industry-specific and, in particular, arise in industries with closer technological links to university research and in industries that employ a higher proportion of university graduates. Knowledge spillovers might benefit incumbent firms, but the evidence is also at least suggestive of university research facilities acting to attract high-tech innovative firms to these areas and hence acting as a basis for cluster formation with potential long-term benefits accruing—at least to certain industries—through these agglomerative forces. Many of these studies are based, however, on long-established universities, very likely in relatively affluent regions. But from the point of view of using universities as a policy tool for economic development, the evidence from Sweden points towards beneficial effects on local labor productivity—effects that do not appear to net out on aggregate across regions and that are potentially driven by increases in local human capital endowments. Hence, the evidence implies that investment in higher education and research, which have public-good elements to them, could generate long-term local effects. But questions remain about the optimal location of higher education investment, and to inform this, more evidence is needed, in particular on the precise channels through which higher education institutions affect local economic activity and how effects vary with local characteristics.

To some degree, high-tech firms do appear to internalize externalities from public-sector research in their location decisions, although science parks and other incentives may also be influential. The results on the impact of the Bavarian cluster initiative also suggest that, aside from seeking to influence firm location decisions, government intervention can also potentially overcome coordination failures and increase the returns to innovative activity, by seeking to bolster interaction between private-sector firms and improve private-sector access to public research facilities. However, this appears to have been both a highly targeted program, with funds flowing to both private firms and public-sector research, and an expensive program, certainly in comparison to the LPS policy adopted in France, which did not result in any evidence of beneficial effects on firm performance.

18.5.5 Infrastructure investment and other regional policies

The European Union has long embraced policies aimed at reducing disparities between regions across all member states, with funds being distributed to lagging regions even within nations with on average relatively high per capita income. The primary policy instrument is EU Structural Funds, comprising the European Regional Development Fund (ERDF) and European Social Fund (ESF), which aim to increase economic growth and create jobs in eligible areas. ERDF expenditure is typically on infrastructure, for example investment in energy, telecommunications, or linked to R&D, but can also include subsidies for investment by firms. Expenditure under the ESF is on initiatives to boost employment, such as training programs or projects to increase labor-market attachment. A third pot of funding, the Cohesion Fund, is available to whole countries with gross national income per capita less than 90% of the EU average. This program funds investment in cross-national transport infrastructure (see Redding and Turner, 2015, for a discussion of the evidence on transport infrastructure and growth) and investment with environmental benefits. During the period 2007–2013, expenditure under the three programs accounted for around 35% of the total European Community budget (Becker et al., 2012), with ERDF expenditure of 201 billion euros accounting for over half of this.[52] The largest amount of funding goes to Objective 1 regions with GDP per capita below 75% of the EU average. Within these lagging areas, European national governments are also permitted under EU regulations to offer discretionary subsidies of the type discussed in Section 18.5.3.

The United States also provides an example of a very large-scale regional development program, the Tennessee Valley Authority (TVA), the long-run impact of which is analyzed by Kline and Moretti (2014b). The authors not only evaluated effects on the targeted region but also estimated aggregate effects for the United States as a whole and examined outcomes after the funding is withdrawn. We first discuss this and related US policy and then turn to EU policy.

The TVA development and modernization policy involved substantial investment in public infrastructure including energy (electricity-generating dams), transport (road networks and canals), and new schools. The investment in electricity-generating capacity was part of a deliberate strategy to attract manufacturing activity to the TVA area. In geographic scope, it spanned four US states, covering nearly all of Tennessee and areas of Kentucky, Alabama, and Mississippi. The program began in 1933, with the highest expenditure occurring during the 1940s and 1950s. Kline and Moretti reported that federal expenditure totaled around \$20 billion (in 2000\$) from 1934 to 2000, with transfers per household during the early 1950s peaking at around 10% of average household income in the region.

Using preprogram data from 1930, the authors demonstrated that the set of counties covered by the program performed worse on a set of economic indicators compared to US counties in general and compared with other southern counties. The fraction of employment in agriculture was higher, and the fraction in manufacturing lower, manufacturing wages were lower, and literacy rates were also lower, indicating lower productivity and human capital. Given these systematic differences in the characteristics of the TVA counties, Kline and Moretti exploited the fact that regional authorities were proposed but due to political reasons never established in other parts of the country, to construct an additional set of control areas. To do this, they approximate the geographic boundaries of six potential regional authorities and verify that many of their pre-TVA economic characteristics, including prior trends in the share of employment in manufacturing, are much closer to those in the TVA counties.

[52] See http://ec.europa.eu/regional_policy/thefunds/funding/index_en.cfm (viewed 7 January 2014).

The authors first looked at impacts on the TVA counties for a range of outcomes over different periods: 1940–2000, and two subperiods 1940–1960 and 1960–2000. The subperiod split is chosen because after 1960, federal transfers to the program were negligible. For the full time period, their most consistent findings are that the growth rate of manufacturing employment was higher (at around 5–6% per decade) and the growth rate of agricultural employment lower (around 5–7% lower per decade) relative to control areas. In addition, median family income growth increased around 2.5% per decade because of the policy. The results by subperiod indicate that the faster growth in manufacturing employment occurred during both periods, although the effect was around 3 times higher during the initial two decades (around 10–12% per decade during 1940–1960, as opposed to 3–3.5% from 1960 to 2000). In stark contrast, agricultural employment is found to have experienced substantially faster growth up to 1960 (around 11–12% per decade) but dramatically slower growth thereafter (around 13–17% lower per decade).

The results over the full period 1940–2000 paint a picture of public infrastructure investment in the TVA counties increasing the pace of industrialization, shifting employment out of agriculture towards manufacturing. That the authors found little evidence of wage increases in the manufacturing sector implies that labor supply was elastic, with new workers either moving to the area or switching from agricultural to manufacturing employment. The increase in family income growth therefore is driven by changes in the composition of employment, since wages in manufacturing exceeded those in agriculture. The authors attributed the differential response of manufacturing and agricultural employment growth post-1960 to the presence of agglomeration externalities in manufacturing, discussed next. These externalities continued to make the TVA counties an attractive location for new manufacturing activity even in the face of depreciation of the initial infrastructure investments following the withdrawal of federal funding, whereas the authors argued that faster growth in agricultural employment did not persist because agriculture may not exhibit the same external economies (Hornbeck and Naidu, 2014).

The paper then analyzes the aggregate effects of the TVA program—whether it generated benefits at the national level or whether the gains in the TVA counties came at the expense of other areas. The authors' theoretical framework for analyzing national effects allows for two channels through which the policy can affect aggregate labor productivity: a direct effect whereby investment in public infrastructure acts to raise private-sector productivity and an indirect effect arising from the presence of agglomeration economies in manufacturing. As outlined in Section 18.2, this latter effect cannot have a positive impact on the aggregate unless different areas exhibit heterogeneity in how responsive local productivity is to a change in agglomeration, i.e., the local agglomeration elasticity. If this elasticity is constant across regions, then a spatial redistribution of workers will generate no aggregate benefits. Their empirical evidence is supportive of the latter case.

Rather than estimate effects on labor productivity, the authors assumed perfect labor mobility and estimate effects on manufacturing employment, which will increase in the face of labor productivity-enhancing investment.[53] The paper estimates both the direct effect of the TVA investment on manufacturing employment and the indirect effect on manufacturing employment of agglomeration, as measured by the lagged density of local manufacturing employment. One issue is how to separately identify the direct and indirect effects, given that a direct effect that increases manufacturing productivity will increase manufacturing employment, which in turn will affect the density of manufacturing employment. The model is estimated using a county-level panel in first differences, with the dependent variable the change in log county-level manufacturing employment over a decade. The direct effect is identified from the inclusion of a dummy variable for TVA counties (not differenced). The indirect effect is identified from lagged changes in the density of manufacturing employment, which are instrumented using longer (two-decade) lags.

A second aim is to allow the agglomeration elasticity to vary flexibly across the distribution of the density of manufacturing employment. This is done using piecewise splines in manufacturing employment density, estimating spline functions for low, medium, and high sections of the distribution. The estimates using a spline in the log of manufacturing employment density measure the agglomeration elasticity with respect to manufacturing employment, and there appear to be no significant differences in the estimated elasticities across the three sections of the density distribution, with a 1% increase in density resulting in a 0.4–0.47% increase in manufacturing employment.[54]

The estimated direct effect over the entire sample period 1960–2000 is positive but statistically insignificantly different from zero in the IV specifications, suggesting no evidence of differential manufacturing employment growth in TVA counties over this period. However, this conceals considerable variation in the estimated direct effect over three subperiods 1940–1960, 1960–1980, and 1980–2000. When the sample is extended further back in time, the results indicate that for the early years—1940–1960—the TVA policy resulted in a significant direct boost to manufacturing employment, but for the final two periods, the direct effect is estimated to be negative but insignificant. Hence, while the direct effect of the policy is felt in the period when substantial federal transfers were being made, the indirect effects on manufacturing employment and productivity

[53] Their data are decadal; hence, the assumption needs to hold at this frequency. In their model, the TVA investment increases firm productivity, which increases the wage, leading to an inflow of workers until in the longer run, the wage returns to its equilibrium level but at a higher level of manufacturing employment. This higher level of employment will only be permanent if either the productivity increase from the investment is permanent or the elasticity of productivity with respect to employment density is nonlinear.

[54] Their central estimate of the elasticity of productivity with respect to density is around 0.2, which is somewhat higher than the majority of elasticity estimates reported in Melo et al. (2009) across a range of studies, but not outside the overall range of estimates.

within the TVA counties, generated by agglomeration externalities, continued after the funding had been withdrawn, as evidenced by the continuing faster growth in manufacturing employment in the affected counties noted earlier.

As a final exercise, they use their estimates in a cost–benefit analysis from the point of view of the aggregate effect on the United States. Since the constant agglomeration elasticity implies that agglomeration benefits of the policy cancel out across US counties, the only benefit is the direct benefit to the TVA counties, by raising the productivity of the manufacturing labor force in that location during 1940–1960. They estimate the net present value of the benefits delivered by the program to be $23.8 billion, which exceed the net present value of the federal transfers of $17.3 billion.

In summary, for the TVA counties, once the federal subsidy program ended, the gains to agricultural employment that arose during the earlier period were eventually eroded. But increases in manufacturing employment brought about by investment in public infrastructure continued well beyond the policy end date, due to agglomeration externalities. The estimated direct effects of this infrastructure investment on aggregate manufacturing productivity are positive, with estimated benefits exceeding the program cost. However, based on the near-constant estimated elasticities of manufacturing employment with respect to manufacturing density, the estimates of indirect effects on manufacturing arising from agglomeration externalities are around zero in aggregate, with the positive agglomeration benefits that accrued to the TVA region offset by negative effects elsewhere.

Glaeser and Gottlieb (2008) discussed the effects of the Appalachian Regional Commission (ARC) that, beginning in 1963, disbursed considerable federal funding, primarily for transportation infrastructure but also for expenditure on health and education, to counties spanning a large geographic area from Mississippi to New York. They estimated effects on ARC counties using other nontreated counties within the same states as controls (excluding those within 56 miles of the coast). They found some evidence of a positive effect of the infrastructure expenditure on population growth over 1970 to 1980, but no statistically significant evidence of an effect over a longer period 1970 to 2000, and importantly, no statistically significant evidence of an effect on growth in per capita income. Their results are quite different to the findings of a previous study by Isserman and Rephann (1995), who primarily used comparisons of mean growth rates across matched treatment–control pairs (this time excluding as potential controls counties within 60 miles of ARC counties) and found large effects of the program on per capita income. While the stark differences in findings may be due to the alternative ways in which the authors addressed the problem of constructing a proxy for unobservable counterfactual outcomes, Glaeser and Gottlieb did acknowledge that the standard errors around some of their estimates are large enough that they cannot rule out substantial positive effects. In fact, their conclusion is that evaluation of this type of wide-ranging expenditure policy can prove very difficult, due to the funding being spread thinly across a very large geographic area and over a long time period, and the difficulty of controlling for many other confounding factors that might affect economic growth over the longer term across such a wide region.

Becker et al. (2010, 2012) provided recent evaluations of the impact of major infrastructure investments under EU regional policy. They focused on the impact of Structural Funds on growth in employment and GDP per capita in Objective 1 regions. Becker et al. (2010) exploited data on NUTS2 and NUTS3 regions for three rounds of the Structural Funds program between 1989 and 2006. The authors used the fact that the NUTS2 area eligibility threshold is in principle a strict cutoff at GDP per capita below 75% of the EU average, in an RD evaluation strategy, arguing that those regions with GDP per capita close to the threshold will have ex ante similar characteristics, but only those below the threshold will qualify for funding as Objective 1 regions. Since in practice there are a few exceptions to defining eligibility at the NUTS2 level—a small number of NUTS3 regions received funding and a small fraction of NUTS2 regions have a treatment status that does not adhere to the strict eligibility rule—the paper implements a fuzzy RD approach and instruments regions' treatment status using the eligibility rule, and also conducts a robustness check whereby treatment status is defined at the NUTS3 level.

Their results imply a robust positive effect of Structural Funds expenditure on growth in GDP per capita, with a preferred estimate of around 1.6 percentage points per annum within a funding period.[55] However, in the vast majority of specifications, they found no effect on employment growth. The authors conducted a back-of-the-envelope cost–benefit analysis, which suggests that the program of transfers is cost-effective, generating a return of 20%, or a multiplier of 1.2. However, the confidence intervals around their estimates mean that they cannot reject a multiplier of only 1. They speculated that the findings of effects on per capita GDP, combined with no effects on employment, are driven by productivity gains from infrastructure investment but that any new job creation may only occur with a longer time lag. Overall, though, their results imply that the policy is cost-effective in increasing income in targeted regions.

Becker et al. (2012) extended this analysis to look at the relationship between the generosity of financial assistance under the program, i.e., the treatment intensity, and growth in per capita income. Their approach allows them to analyze whether the Structural Funds budget could have been redistributed across eligible EU regions to achieve higher growth in the aggregate and faster convergence. The underlying idea is that if the investment funded by the transfer payments exhibits decreasing returns, it is possible that

[55] Their evidence of a positive effect on per capita GDP growth in Objective 1 regions is also supported by Mohl and Hagen (2010). Table 1 in that paper provides an excellent summary of the findings from evaluations of the effects of EU Structural Funds, together with information on the data and econometric approaches. The majority of studies suggest positive effects on regional growth or on regional convergence, with a few exceptions.

funding beyond a certain level becomes inefficient. It is also possible that very low levels of transfers are simply ineffective in stimulating growth, with a minimum funding generosity required to generate a big push.

The authors used data on the intensity of transfers under the Structural Funds and Cohesion budgets over two EU funding rounds (1994–1999 and 2000–2006) at the NUTS3 level. They identified substantial variation in the intensity of transfers across regions, as measured by annual transfers as a fraction of a region's GDP in the year before the funding began. This ranged from 0.00009% of GDP for a region within Sweden to 29% of GDP for a region in Greece, with an average intensity of 0.756%. The paper estimates the effects of different degrees of treatment intensity using generalized propensity score estimation. This nonparametric method is an extension to continuous treatments of the propensity score matching approach to analyzing the impact of a binary treatment. The method allows the authors to estimate whether the treatment effect varies at different funding intensities, conditional on observable determinants of the treatment intensity itself (Hirano and Imbens, 2004).

The results confirm the findings of Becker et al. (2010) that on average the program generated positive effects on regional growth, but they also imply a nonlinear relationship between treatment intensity and growth in per capita income. From this, the authors can back out various thresholds: first, the "maximum desirable treatment intensity," defined as the intensity beyond which they cannot reject a null hypothesis of zero effects on growth, and the second, the "optimal transfer intensity," the level at which one additional euro of funding generates exactly one euro of additional GDP in the average region.

They found that around 18% of NUTS3 regions received funding beyond the estimated maximum desirable treatment intensity threshold of approximately 1.3%. For these regions, growth would not have been substantially lessened by a reduction in the generosity of funding to this level. In addition, they estimated that a redistribution of funds away from these regions and towards regions with lower funding intensities would be more efficient and could have increased average regional growth in per capita income by around 1.12 and 0.76 percentage points in the first and second funding rounds, respectively. They also found that around 36% of regions received transfers beyond the optimal transfer intensity of 0.4% of regional GDP. Redistribution across regions based on this lower threshold could have raised aggregate GDP growth but would have come at the cost of working against the regional convergence objective, since the redistribution would have been towards relatively prosperous areas. This suggests a trade-off between maximizing the aggregate efficiency of the program and potentially taking advantage of greater agglomeration externalities in relatively well-off regions, and the specified redistributive aim of the scheme. The authors also found no evidence to suggest that a minimum level of transfers is necessary to generate increases in per capita income growth, estimating positive effects of even small transfers.

In summary, the evidence from the analysis of both the TVA investment program and the EU Structural Funds implies that infrastructure investment can be cost-effective in delivering productivity growth in targeted regions and can act as a redistributive tool across locations. Questions remain about how long-lasting these effects are, with the direct benefits of the TVA program appearing to erode over time, and also about the precise mechanisms underlying the effects on growth. For example, this type of funding typically covers a wide range of public infrastructure investments, and it would be instructive to know the relative benefits of each in terms of their effects and whether these investments are complementary to each other in terms of increasing local growth. There is also evidence that infrastructure investment can result in agglomeration benefits for the targeted areas, although this evidence also seems to suggest that these may come at the expense of efficiency gains in other regions.

18.5.6 Community development and locally led initiatives

Finally, we turn to a small number of place-based policies that do not fit so neatly into the previous categories. First, in the United States, there are a number of programs that focus on real-estate development, but sometimes also have other components. Moreover, these often have some discretionary flavor. The discussion of enterprise zones above already referred to redevelopment areas. A common tool in these areas is to allow tax increment financing (TIF) whereby increases in property taxes owed as a result of appreciation (presumably stemming from redevelopment) are used to finance the debt incurred to engage in the redevelopment. TIFs and redevelopment are somewhat controversial, and we do not discuss them in any detail in this chapter as the research on them focuses nearly exclusively on implications for real-estate prices (e.g., Weber et al., 2007). Low-income housing programs similarly have a place-based, discretionary flavor. Among studies examining their effects—again, mainly on housing markets—are Sinai and Waldfogel (2005) and Eriksen and Rosenthal (2010).

A recent study (Freedman, 2012) examines the federal New Markets Tax Credit (NMTC) program, which not only concentrates on real-estate development but also devotes resources to economic development mainly through subsidizing capital for businesses through loans or preferential interest rates. The study examines the same kinds of labor-market outcomes examined in the literature on enterprise zones. For the period of this study (2002–2009), the NMTC provided $26 billion in tax credits to investors making capital investments mainly in businesses located in moderately low-income neighborhoods. Freedman reported that around 70% of the funds go to commercial real-estate development and most of the rest goes to business development—mainly loans to firms. NMTC funds are channeled through Community Development Entities (CDEs), often banks or financial institutions, which have to meet several criteria including serving or providing capital to low-income communities and people. The channeling through the CDEs is what gives the NMTC a discretionary flavor, especially given that only a very small fraction received the right to allocate tax credits in the years Freedman studied, via a competitive process. The tax credits flow to investors that make equity investments in the CDEs.

The study has many parallels to Freedman's (2013) study of the Texas enterprise zone program discussed earlier, although the focus is national. He used the same data and a similar research design, exploiting a discontinuity in eligibility for NMTC funds based on the main criterion that makes tracts eligible—having median family income in the 2000 Census below 80% of the state's median for nonmetro areas and below the greater of the MSA or state median for metro areas. This is not the only rule determining eligibility, so Freedman used a fuzzy design that instruments for actual NMTC credits with whether or not a Census tract is eligible based on this rule. He carried out many of the same kinds of analyses to validate the RD design as in the enterprise zone paper. He did not, however, consider overlap with enterprise zones, which could be important.

The evidence suggests that there is a discontinuous increase in NMTC investment at the threshold for eligibility based on median family income in the tract—about $1 million more in NMTC investment than similar tracts that do not qualify and about 0.05 additional businesses receiving investment. Given that these amounts seem fairly small, it may be more plausible to believe that the effects Freedman found—discussed next—flow more from the real-estate development side of the NMTC. Using Census data, the main statistically significant effect Freedman found is for reducing the poverty rate, with estimates centered on a reduction of about 0.8 percentage point. He characterized this as a limited and costly effect, so that despite the small investment effects, the evidence implies that it costs about $23,500 to lift one person out of poverty. At the same time, Freedman also found some evidence consistent with compositional changes, with a few of the estimates indicating increases in household turnover of about 0.75 percentage point. Such displacement effects could imply even higher costs to reduce poverty. However, unlike some of the work on enterprise zones, Freedman did not find evidence of an effect on median housing values, with the estimates very close to zero, which is less consistent with a compositional change towards higher income, higher skilled people. Inferring a direct impact on poverty of residents is also challenging because there is no statistical evidence of employment effects from the LEHD data. While the point estimates hover around 1.5%, the standard errors are 3 times as large. Freedman generously concluded that there is a "modest positive effect on private-sector employment" (p. 1012), and while a positive but insignificant effect does not imply no effect, this still seems too strong a conclusion. And compositional shifts, in and of themselves, could also lead to higher employment. Between the potential compositional shifts, the difficulty of understanding how such small amounts could have much impact, and the small impacts that occurred even if we rule out compositional changes, it is hard to attribute much success to the NMTC program.

Some place-based community development policies involve considerable local autonomy in terms of designing interventions and spending public money. Two examples within Europe are the Patti Territoriali program in Italy, which aimed to stimulate growth and employment, and the New Deal for Communities in England, which focused on community development with a very broad remit. Accetturo and de Blasio (2012) evaluated the effects of the Patti Territoriali program, established in 1997. These territorial pacts aim to boost economic growth and employment in lagging areas eligible for support under the EU criteria. During the period analyzed, in principle, the program covered the whole of southern Italy as an Objective 1 region and some areas of central and northern Italy. Within these areas, local governments, local business groups, and trade unions from proximate municipalities could come together to form an agreement, which set out a development plan for the area. Therefore, not all eligible municipalities participated in the program in practice, although the authors reported that those that did tended to form large groups covering on average 27 municipalities and an average population of 235,000. Public funding was allocated to both public infrastructure investment and financial incentives for private-sector investment in the participating areas. A maximum of 50 million euros was allocated to each area, with public infrastructure investment expenditure limited to 15 million euros.

The paper evaluates effects on employment and on plant numbers. Clearly, both the eligible municipalities and within those the subset that actually choose to participate are nonrandom samples of the full population. Within northern and central Italy, not all areas are eligible to form Patti Territoriali. The authors used propensity score matching to identify similar eligible and noneligible municipalities prior to the implementation of the program and used difference-in-differences to estimate an intention-to-treat effect, which can be a valuable parameter for policymakers wanting to know the effect of the program on the target areas. They also estimated the effect of treatment on the treated by comparing a set of participating areas with a set of comparable areas that were ineligible to participate.

These evaluation approaches are not available in assessing the impact of the program in southern Italy since the entire geographic area is eligible as an Objective 1 area. Instead, difference-in-differences estimates compare changes in outcomes in participating municipalities to changes in nonparticipating municipalities in the south. Equivalent difference-in-differences estimates, using eligible but nonparticipating municipalities as controls, are calculated for northern and central Italy to try to assess the degree to which selection into participation on unobservables might affect the estimated program effects for the south. The authors also addressed the fact that the Law 488 program discussed above was running concurrently.[56]

[56] For northern and central Italy, only areas eligible for Patti Territoriali are eligible for Law 488 financial incentives, potentially biasing estimates of the effects upward. For southern Italy, the nonparticipant areas used as controls are also eligible for Law 488, which could bias estimates of the effects upward or downward depending on how Law 488 affected the different areas.

Using Italian census data from 1996 to 2001, the intention-to-treat estimates for central and northern Italy on employment and the number of plants are negative and not statistically significant. While it is possible that this reflects a combination of positive effects on participating areas and negative spillover or displacement effects on eligible but nonparticipating areas, the estimated effect of the program on participating municipalities is also negative and statistically insignificant. Estimates for the south, which compare outcomes for participating versus nonparticipating but eligible municipalities, imply a positive and significant effect of the program on employment. However, replicating this approach for the northern and central regions suggests that the estimates for the south are very likely upward-biased, and if the sample for the south is reduced to only those municipalities that received no funding under the Law 488 program, the estimated effect on employment decreases in size and becomes statistically insignificant.

These results point towards there being no positive effects of the policy on employment or on the creation of new plants. This raises the question of why it was not successful. One possibility the authors discussed is that the available funding of 50 million euros per area was spread too thinly to generate sufficient additional activity in these lagging regions. However, since this level of funding was found to be equally ineffective in the most deprived southern regions and in the relatively more prosperous regions of northern and central Italy, where perhaps a lower level of expenditure might have been required, the expenditure cap was likely not the only explanation. The second suggestion is that the program fell victim to rent-seeking activities, which could even have been heightened by the bottom-up, locally led approach, and that subsidies were diverted to inferior private-sector projects.

A second example of a policy with local autonomy is the New Deal for Communities, which was operational in England from 2002. The ultimate aim of the policy was to improve living standards in the most deprived neighborhoods in the country. In practice, the program involved local committees devising and implementing a range of policies that aimed to improve employment and educational attainment, reduce crime, improve health, and address local housing and environmental issues. Examples of projects aimed at increasing employment included advice and credit schemes for those wanting to start their own business, become self-employed, or develop an existing business, and support for vocational training. Thirty-nine neighborhoods participated in the scheme and a total budget of £2 billion was allocated to be spent supporting these local initiatives over a 10-year period.

Gutiérrez Romero (2009) used a difference-in-differences approach to analyze the effects of the program on employment outcomes in participating areas. Control group neighborhoods were selected based on being within the same—in principle eligible—local authority as treated neighborhoods, but not directly bordering the treated neighborhoods where the program was in operation. Using a household survey carried out in treatment and control neighborhoods, she found that the program increased the likelihood of entering employment for specific types of individuals, such as those who were in full-time education or undertaking training in the preprogram period and those who were claiming incapacity benefits, but not for those who were claiming unemployment benefits. Partly in line with these results, in a companion paper using administrative data on benefit claimants, Gutiérrez Romero and Noble (2008) found that the same program led to a reduction in individuals in treated neighborhoods claiming unemployment and incapacity benefits.

Like in the analysis of enterprise zones, one point to come out of studies of local initiatives that include a very wide range of policy elements is the need to understand which components of these programs make a difference, in those instances where beneficial effects are detected. In addition, for policies that do not appear to generate effects, it would help to have further corroborative evidence on why they did not work—whether it is simply that the financial scale of the intervention is too low with funds being spread too thinly or whether the policy design was ineffective. In addition, the analyses of the NMTC and New Deal for Communities highlight the value of trying to isolate which sets of individuals are affected by the policy and whether the policy is reaching the target groups.

18.6. UNANSWERED QUESTIONS AND RESEARCH CHALLENGES

In summary, what have we learned from the available evidence? The answer is probably "not enough." To guide policy, we need know more about *what* works, *why* it works, and, crucially for place-based policy, *where* it works and *for whom* it works. We conclude by suggesting five areas where the evidence base could be usefully extended: investigating long-run effects, isolating specific features of policies that make them effective or that create unwelcome distortions, identifying more precisely what the effects are and who it is that gains benefits or incurs costs, learning more about potential strategic interactions between jurisdictions offering place-based policies, and examining whether broader policy levers such as tax policy might be more effective than place-based initiatives.

In our view, a major shortcoming of the research on place-based policies is that even the most positive evidence on their effectiveness does not establish that they create self-sustaining economic gains. That is, at best, the evidence (sometimes) says that when place-based incentives *are in effect*, there are increases in economic activity and perhaps welfare. There may be some gains from benefits even if governments have to continue paying the costs. However, a much stronger case would exist if some kinds of place-based policies helped to jump-start economic development in an area in a way that becomes self-sustaining—in the language of economics, by moving the area to a new equilibrium. Moretti (2012) concurred, arguing "The real test is not whether [place-based policies] . . . create jobs during the push . . . Instead we need to look at whether the publicly financed seed can eventually generate a privately supported cluster that is large enough to become self-sustaining" (pp. 200–201).

Some of the most positive evidence summarized above seems to point towards the benefits of infrastructure expenditure, perhaps within enterprise zone-type programs, and more clearly as part of EU Structural Funds, as well as benefits from expenditure on higher education and university research. This is perhaps not surprising given the public-goods nature of this type of investment. However, more evidence is needed on longer term outcomes. The findings from Kline and Moretti (2014b) on the effects of investment under the TVA program suggest that this very substantial push did generate long-lasting increases in manufacturing employment and income in the targeted region, both through a direct effect during the peak period of infrastructure spending and through subsequent agglomeration externalities. But even this study finds that the direct effect of the public investment on local productivity diminished over time and also that when measured at the national level, the indirect agglomeration benefits to manufacturing appear to net out across regions at around zero. This analysis further highlights the value of assessing the aggregate welfare effects of these investment programs, and not just those on the directly affected areas, in order to fully determine the magnitude of any trade-off between aggregate efficiency and redistribution across regions—although that is very hard to do.

As summarized above, for enterprise zone policies, much of the evidence to date is at odds and presents mixed messages as to the effects of such programs. More could be done to try to reconcile the existing findings and to unpack whether differences are due to data, to econometric methods, or to genuine differences in the effectiveness of different programs operating in areas with different economic characteristics. Moreover, different features of programs may alter their effectiveness, as exemplified by the discussion of the potential role of block grants in the federal Empowerment Zones. This highlights our second point, that even if the current evidence did all point in the same direction—for example, in terms of positive employment effects—what it does not tell us is exactly which features of these programs matter—for example, the use of hiring credits versus infrastructure investment. Knowing more precisely what works in terms of specific elements of the policy, and importantly why these elements work, would be of considerable value to policymakers.

In addition, we need to know about how generalizable the policy conclusions are across areas. By their very nature, place-based policies are implemented in locations with different characteristics, and it is not only the policy details that vary but also the economic environments in which they are set. A few studies have looked for and found evidence of heterogeneous effects of the same policy across different locations—perhaps most notably and systematically the Briant et al. (2012) study of variation in the effectiveness of the French ZFUs based on transportation accessibility and barriers between targeted areas and main employment centers. Clearly, much more could be done to understand the source of this variation in policy effectiveness.

We have argued that studies that provide evidence on the outcomes specifically targeted by policies are valuable in their own right and are the obvious first step in policy evaluation. However, research could look at a wider range of outcomes relevant to assessing overall welfare effects. For example, are there distortionary effects of enterprise zone policies beyond the displacement of employment from neighboring areas? If firms receive targeted incentives to increase employment, does this create any unwelcome distortions to firm productivity, for example, through the type of workers recruited or through the displacement of firms from otherwise more productive locations? While some of the empirical literature on discretionary grants assesses potential distortions of this type stemming from the policy design, the literature on enterprise zones does not typically look at plant labor productivity or TFP, although it has looked at related outcomes for individuals such as wages.

Research could also aim to shed more light on exactly who gains any benefits from place-based policies. If programs are effective in increasing local productivity, are the ultimate beneficiaries actually landowners if the supply of housing or buildings is inelastic? If policies are found to be effective in raising employment rates or average incomes in targeted areas, is it resident individuals in low-income groups, whom the policies often aim to benefit, who actually realize these gains, or is there significant in-migration? More evidence on the redistributive effects of these policies across individuals within eligible areas would be valuable, in particular linked to features of the programs such as hiring incentives for specific groups.

Some of the empirical literature pays attention to overlapping place-based policies, but that may be just the tip of the iceberg. Given the evidence that place-based policies often encourage simple relocation of economic activity, it seems natural to think that—at least for place-based policies at a local level—jurisdictions may respond to the policies offered nearby. This is a common theme in concerns about a "race to the bottom" in welfare programs, environmental regulation, and tax policy (e.g., Brueckner, 1998). This can have potentially important implications for empirical work: for example, the estimated short-term impact of a place-based policy implemented in a particular jurisdiction may capture the partial equilibrium effect, rather than the general equilibrium effect once other jurisdictions have responded. While this issue has been taken up with respect to other state or local policies, it seems it could be fruitfully considered in the context of the kinds of place-based policies this chapter considers and is also relevant to the issue of whether such policies are in reality a zero-sum game.

Finally, we noted earlier that there are policies intended to boost local labor-market activity—such as lowering business taxes—that are outside the definition of place-based policies, because they do not favorably treat one area *within* a government's jurisdiction. In light of some of the theoretical arguments against place-based policies discussed in this chapter, it may well be that policies that encourage economic activity *without* distinguishing between regions within a jurisdiction are more efficient. Bartik's (2012) review suggests that the evidence clearly indicates that lower local average business taxes are associated with higher local labor demand, although the range of elasticities is so large

(−0.1 to −0.6) as to make cost–benefit calculations meaningless. He also argued that evidence on Michigan's MEGA program, which provided marginal subsidies to businesses that export from the local economy, shows that it is much more effective (6 times more impact per dollar of foregone revenue) than cutting the overall business tax rate (Bartik and Erickcek, 2010). Bartik also noted that we know a lot about policies to increase the quality of labor supply and how this can have positive externalities on others (Moretti, 2004), but not nearly as much about how a regional development policy to increase human capital results in more jobs and higher quality jobs in the local economy.

We think an important research question is whether place-based policies, per se, are more efficient, or less distortionary, than broader local economic development efforts. We are not aware of work that tries to weigh the alternative approaches, but given the fairly weak or highly uncertain evidence regarding the effectiveness of at least some place-based policies, it is likely that broader policies are more efficient. At the same time, it is plausible that the broader policies fail to achieve some of the distributional goals of place-based policies, although as we have noted, the evidence that place-based policies achieve their distributional goals is itself far from clear.

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