PLACE-BASED POLICIES

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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 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. But to better guide policy we need to know more about what policies create self-sustaining longer-run gains.

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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) distinguishes a subset of place-based policies or strategies that she labels "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, there are some place-based policies that 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 as well as 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 and Moving to Opportunity programs 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, to reduce spatial mismatch – are examples of indirect policies. This chapter focuses on direct policies.[1]

Place-based policies targeting the disadvantaged, including indirect policies, are often rationalized in part by behavioral hypotheses that seek to explain the overlap between areas with poor

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[1] There are many excellent summaries of the details of both the Gautreaux and MTO program designs, and 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).

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.

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, according to which 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 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.

In the remainder of this introduction, we provide more detail on place-based policies, providing an overview of the types of policies that exist and specific examples. We emphasize the intended recipients and the stated goals of these policies. Then, in the rest of the chapter, we often refer back to the effects of these policies on the intended beneficiaries, and whether the policies achieve their goals.[2] 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 more general conclusions. In turn, this means that we necessarily exclude interventions in developing countries, such as Special Economic Zones in China (see Wang, 2013 and Alder et al., 2013) and India's National Manufacturing and Investment Zones.

We also exclude policies that result from political or fiscal decentralization and which 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) program, 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 large literature on tax

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[2] Kline and Moretti (2013) provide a very useful complementary review article on place-based policies, which focuses largely on a theoretical discussion of the welfare economics of local economic development programs. It has a very limited discussion of the evidence. In contrast, our goal is to provide a comprehensive overview and evaluation of the evidence.

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. in this volume. And states and cities are often viewed as competing on a number of dimensions including taxes, regulations, quality of life, etc., which are often captured and summarized in business climate indexes (Kolko et al., 2013). Bartik (2003) also discusses 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 try to reallocate economic activity across areas, 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 level.[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 (U.S. Government Accountability Office, 2006) – as well as much less generous hiring credits.[5] Spending in the first round of the federal enterprise zone program, through 2000, totaled nearly $400 million in block grants and $200 million in employment credits. Federal expenditures via hiring credits and block grants for the first six years of the program are estimated at about $850 per zone resident. The six original urban Empowerment Zones have been the focus of much research.

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 percent of

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[3] Bartik (2003) notes that earlier related programs focusing on distressed communities include "Urban Renewal" in the 1940s and 50s, "Model Cities" during the War on Poverty, and Community Block Development Grants.

[4] All tracts in the zone had to have poverty rates above 20 percent, 90 percent had to have poverty rates of at least 25 percent, and 50 percent had to have poverty rates of at least 50 percent. In addition, unemployment rates in each tract had to exceed the 1990 national average of 6.3 percent (Busso et al., 2013; U.S. General Accounting Office, 2006).

[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 July 11, 2013).

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 the worker's characteristics. Clearly both federal enterprise zone programs as well as this state program (and the same is true of many others) target areas based on the characteristics of people who live in them.

Enterprise zone policies are also used in some European countries. France introduced an enterprise zone program in 1997 (Zones Franches Urbaines, or 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 at least 20 percent of employees being hired 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 which 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 as well as 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, as well as other spending on, for example, schools and flood controls (Kline and Moretti, 2014). And Glaeser and Gottlieb (2008) discuss the Appalachian Regional Commission, which 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

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[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).

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

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 EU member states (NUTS level 2 areas) with GDP per capita less than 75 percent 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, as well as 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) which 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 (formerly "Development" and "Tier 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 U.K. 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 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. Clusters policies are motivated by evidence on productivity benefits arising from industry localization, or in some cases more specifically by the observed co-location

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

[9] Eligible areas are revised every seven years. The precise set of economic indicators used to define eligible areas has varied over time, as have the geographic units used to define the Assisted Areas, moving from using "Travel to Work Areas," which approximate local labor markets, to smaller "Electoral Wards."

of some high-tech clusters with higher-education institutions. For example, 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 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.

2. THEORETICAL BASIS FOR PLACE-BASED POLICIES

There are a number of possible arguments as to why place-based policies may help overcome important market imperfections or take advantage of externalities. It is also possible to argue that there may be some benefits from redistribution policies that target disadvantaged areas, and not just disadvantaged people.

2.1. Agglomeration economies

The efficiency-related argument for place-based policies that is most central to urban economics is that there are agglomeration economies, in 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) categorize as "sharing, matching and learning." Moretti (2010) emphasizes 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 emphasizes thick markets for intermediate inputs, especially those that are specialized and non-tradable. Examples are professional services, such as computer programming, legal support, or 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 the gains from the latter effect are not captured by those deciding whether or not 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. However, place-based policies that simply aim to address a co-ordination failure and target the size of a city or cluster may still only be a second-best response, even in the best-case scenario in which policy makers know the optimal size to capture, for example, the externalities between firms from

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

co-location in the same area. It is much more difficult to imagine a policy that fully addresses inefficiencies in investment levels conditional on location, for example due to un-internalised increasing returns such as through spillovers. For one thing, the level of these returns can depend on the scale of firms' investment, and it is difficult to conceive of a place-based policy that responds endogenously to this investment in an efficient way.

Moretti (2010) argues that the rationale for place-based policies to exploit agglomeration economies may be quite strong, especially in a dynamic setting with multiple equilibria that can arise from the non-linearities inherent when there are externalities. 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). As Glaeser and Gottlieb (2008) emphasize, in choosing *between* locations in which to encourage growth, policymakers should do so in areas that are more productive and where the elasticity of productivity with respect to agglomeration is higher, exploiting any non-linear relationship between productivity and size or density. In practice, they argue – given the challenges in even estimating the overall magnitudes of agglomeration economies, let alone the non-linearities – policymakers may have little or no knowledge of how this elasticity varies across regions, and hence little basis for preferring one place over another. Moretti, however, suggests that competition among local governments with regard to place-based policies can be an efficient source of place-based policies. He argues that when local governments know the value of the local externality and set locally-financed incentives based on it, and when there is heterogeneity among locations in the value of this externality, then competition among localities 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 for firms. Of course there may be reasons for scepticism 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.[11]

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

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[11] For example, Neumark et al. (2005a) discuss the policy debate in California, in the early 2000s, on businesses coming to or leaving the state, when in fact job creation or destruction attributable to business relocation is trivial compared to job creation or destruction from growth or decline of existing businesses, and births or deaths of businesses.

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]

The existence of 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 it is the potential for knowledge spillovers to be localized that is relevant for place-based policy. If spillovers increase with geographic proximity, and if firms are aware of this, then investment in universities may serve to attract innovative firms to the locality. In addition, local governments may take further steps to increase knowledge spillovers from publicly-financed research, such the creation of business incubators and science parks near research universities.

Echoing the discussion of agglomeration economies generally, Glaeser and Gottlieb (2008) raise questions about national policies (and by extension policies set above the local level) to try to exploit knowledge spillovers by encouraging moves of educated workers from one place to another to potentially overcome a co-ordination failure, or the creation of educated workers in one place rather than another. The argument is similar; there have to be non-linear relationships so that moving skilled people from one location to another location increases productivity more in the target area than it decreases productivity in the origin area, and policymakers must know the relationships.[13]

However, there may be some – perhaps more limited – policy responses to knowledge spillovers that do not appear to be subject to this critique because they do not necessarily imply moving resources (or people) from one area to another. For example, encouraging interactions between universities and businesses or innovators may exploit knowledge spillovers without necessarily taking resources from another area. In such cases, it would seem that policy can potentially successfully exploit knowledge spillovers even if we do not know the form of the non-linearities.

In addition to the potential value of generating knowledge spillovers from attracting high-skilled workers, Moretti (2012) argues that attracting skilled workers in the knowledge-intensive high-tech sector has large local multipliers relative to other industries, in part because of high pay in these jobs, in part

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

[13] In addition, some kinds of policies to exploit knowledge spillovers, such as subsidizing education, can be dissipated because of worker mobility. This may be particularly problematic for higher education as more-educated workers are more mobile. In this chapter we do not review the evidence on general education subsidies (see, e.g., Bound et al., 2004, and 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.

because of demand for business services from this industry, and in part 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.

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, in particular 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.

But the observation that such industrial clusters exist, and the potential presence of externalities, does not in itself justify intervention. For example, for some sectors the location of clusters may be driven by access to natural resources rather than the presence of agglomeration economies. Duranton (2011) analyzes the theoretical basis for industrial cluster policies, and questions 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, as well as 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 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).

Bartik (2003) and Matouschek and Robert-Nicoud (2005) discuss other market imperfections, in investment in training, that might occur when firms are clustered. When this is the case workers can move from firm to firm within the same industry and not lose human capital that is industry-specific rather than firm-specific (in the sense of Becker, 1993). In terms of investment incentives, what matters is both who pays for the training and whether the training is industry or firm-specific. While industrial clustering can mitigate hold-up problems (of workers, by firms) when workers pay for training in industry-specific skills, since workers can more easily move between employers, Matouschek and Robert-Nicoud emphasize that under-investment can still occur, for example, as workers only consider their private benefits. If, instead, firms are paying for industry-specific investments, co-location will lead to under-

investment by firms because of the problem of hold-up by workers. In this case training incentives are greater if firms are more dispersed and are able to recoup the returns on their investment. Finally, if workers can invest in both firm and industry-specific skills, investment in firm-specific skills may also be too low if firms are clustered, with workers potentially overinvesting in less valuable industry-specific skills in order to improve their wage bargaining position. Hence in some cases, when industries are clustered, government involvement in the provision of (industry-specific) training may be justified, more so if there are constraints on workers financing training (such as liquidity constraints, and credit constraints because of a lack of collateral and moral hazard; e.g., Acemoglu and Pischke, 1999). As this discussion indicates, place-based policies can be broader than Moretti's definition of them as "government interventions aimed at reallocating resources from one location to another location" (2010, p. 1296).

2.4. Spatial mismatch

Other types of market imperfections aside from positive externalities 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), because of exogenous residential segregation attributable at least in part to discrimination in housing markets, often coupled with the exit of jobs from these areas with the changing industrial structure (Wilson, 1987).[14]

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., 2010), and the United Kingdom (Fieldhouse, 1999), although the geographic patterns can be quite different – with, for example, concentrations of disadvantage in Paris more likely to be on the urban periphery (e.g., Briant et al., 2012).

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.

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[14] For a recent review emphasizing theoretical models and hypotheses regarding spatial mismatch, see Gobillon et al. (2007).

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.

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).[15] Networks are likely to have a spatial dimension – for example, connecting neighbors. Hellerstein et al. (2011, 2014) and Bayer et al. (2008) report evidence suggesting that network connections between co-residents (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 on the basis of 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 argument (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. But even absent the constraints on mobility assumed by the spatial mismatch hypothesis,

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[15] A different hypothesis that can also imply externalities between individuals is peer or neighbourhood effects, in which, for example, the presence of non-employed residents leads other residents to remain non-employed by changing norms of behaviour (Wilson, 1987), and, conversely, creating some employment can have virtuous effects on others. Although the underlying mechanism differs from that of labor market networks, the empirical implications can be similar, implying multiplier effects from increases in employment. However, because the peer-effects literature has focused much more on education and behaviors other than employment, it is not covered separately here; nonetheless, some of our discussion of the implications of network effects would carry over to peer effects, if they are important. Topa and Zenou in this Handbook address neighbourhood and network effects. A recent paper by Angrist (2013) suggests that there is relatively little compelling evidence of peer effects from studies that should identify a causal effect; some of his criticisms likely carry over to studies claiming evidence of network effects. See Dietz (2002) for a similar contention that neighborhood effects are likely small, and also Manski (1993) for general issues in estimation of peer or neighborhood effects.

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 prudent policy, and more cost-effective in areas with low employment.

This strikes us as a common-sense rationale for place-based policies. Because of crime spillovers between neighborhoods and the location of consumption of urban amenities, 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. And if network (or peer or neighborhood) effects are important, it may be efficient to target such policies to areas with large concentrations of non-employed 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 as a result of some employment opportunities being created. This dovetails with other arguments that place-based policies can in part be justified by the need to correct under-provision of public goods in poor areas, often because the tax base is insufficient to provide these goods (Crane and Manville, 2008).

Conversely, however, network effects could diminish the effects of place-based policies. For example, if an enterprise zone policy leads employers to relocate to an area (as some evidence from France indicates), this may do little to boost employment opportunities of local residents if the employees of the relocating companies are not networked to local residents.

It is still, of course, an open question whether place-based policies increase job opportunities. But if we could, for example, ascribe a policy role to instances where disadvantaged urban areas have been turned around, it seems very likely that these policies would be viewed as generating gains to many city residents and others.

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 are 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. But mobility of people and capital can complicate the effects and potentially undo most or all of the gains from such redistributive policies.

Moretti (2010) develops this argument in some detail.[16] 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.

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[16] See also Kline and Moretti (2013).

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 will probably 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 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. There may, for example, be no good reason to believe that the inframarginal workers in the targeted area – who are the ones who gain – are the lowest-income individuals in those areas.

This echoes a broader concern about the targeting of benefits to the disadvantaged via place-based policies. As Crane and Manville (2008) emphasize, given mobility and land-price responses, the jobs created (if they are created) may go to non-poor 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 Based 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) raise 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 say, "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,

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[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.

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.

2.7. Summary and trade-offs

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. And we have suggested some additional efficiency-based arguments that may rationalize place-based policies. But at the end of the day, 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" (2010, p. 1242). 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) present some evidence suggesting that the non-linearities 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.

As the preceding discussion indicates, theory cannot fully pin down the rationale for place-based policies, whether for efficiency or equity reasons, in large part because the reality is so complex, and the potential motivations of policymakers manifold. With strong theoretical assumptions, it is sometimes possible to test for welfare gains, and indeed pin down where any gains are likely to accrue (for example, to landowners). But, reflecting these same complexities, such exercises are highly dependent on the model. Moreover, 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 distributional consequences that policymakers do not intend. Thus, our view is that empirical research that tests for these effects of place-based policies is very valuable even if it does not attempt to address the full welfare implications; and such research represents enough of a challenge in its own right.

As a consequence, 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.

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

Glaeser and Gottlieb (2008) and Moretti (2010) provide recent reviews of evidence regarding the conventional urban economics arguments such as agglomeration economies and knowledge spillovers. We outline some recent evidence briefly, but focus more 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.

3.1. Evidence on agglomeration economies

There is now a considerable body of empirical evidence in support of the idea that increased density of economic activity both across and within industries generates positive externalities. A broad summary of the evidence on the underlying sources of agglomeration economies is provided in Rosenthal and Strange (2004). A range of papers have now sought to estimate parameters such as 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), Combes et al. (2012) and Greenstone et al. (2010).

Combes et al. (2010) carry out a careful analysis to try to deal with identification problems in estimating the relationship between the density of economic activity and productivity. In particular, they address 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 that choose 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 employ 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 which might be expected to have determined where population settlements occurred and how successful they were. To deal with the fact that the quality of labour may be endogenous they use individual panel data on wages that allows them to separate location effects from both observed and unobserved worker characteristics. In terms of the effects of these two exercises on the magnitude of the estimated elasticities, starting from a benchmark elasticity of around 0.05 between

wages and density, they find that controlling for the endogeneity of the quantity of labor reduces this to around 0.04, controlling for the endogeneity of the quality of labor reduces this to a greater extent to around 0.033, and controlling for both leads 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 a similar elasticity of total factor productivity (TFP) with respect to density of around 0.035.

Combes et al. (2012) examine 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 more productive firms do 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 on the order of 14.4 percent, whereas the gains to firms in the lower quartile from being in denser areas are only 4.8 percent, 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) provide 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 percent higher than in plants in control areas after five years Of course, as the authors acknowledge, 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 find 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.

3.2. Is there spatial mismatch?

Research testing spatial mismatch in the U.S. context tries to incorporate direct information on access to jobs that is related to either travel time or the extent of jobs nearby (e.g., Ellwood, 1986; Ihlanfeldt and Sjoquist, 1990; Weinberg, 2000). These studies tend to show that blacks live in places with fewer jobs per person, and that this lower job access implies that blacks face longer commute times to jobs – although the differences may not be large (Ellwood, 1986). 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) provide a more negative assessment of the hypothesis.

Recent work, however, 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. But 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 which 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. term "racial mismatch."

The authors estimate models for employment including measures of job density by location and skill, but also by race, using confidential Census information on place of residence and place of work.[18] 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. And the own-race relationship is stronger at low skill levels. In a number of specifications the estimated

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[18] 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 non-residents (who work but do not live in the area).

coefficient on the black job density measure is larger than that of the non-black 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 one 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 these jobs tend to be held by whites, the employment of black residents can be quite low. Reflecting 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 shows 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 whites lived – is so small.[19] More recent research establishes that the results are very similar for Hispanics in the U.S. labor market (Hellerstein et al., 2010).[20]

There is evidence for European countries, in the studies cited in Section 2.4, which is consistent with spatial mismatch. One of the more compelling studies is Åslund et al. (2010), who study 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 by and large 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

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[19] In a different type of recent exercise developing a structural model of labor and housing markets to consider black-white unemployment rate differences in the United States (and African-French differences in France), Gobillon et al. (2013) suggest that spatial factors explain only 10 to 17.5 percent of the unemployment rate gap between blacks and whites.

[20] Andersson et al. (2014) present a sophisticated analysis of spatial mismatch, focusing on the relationship between unemployment duration of workers who experienced mass layoffs and measures of job accessibility. The focus on mass layoffs is intended to reduce the correlation between unobserved characteristics of individuals and the accessibility to jobs where they live, because search is more exogenous, although among laid-off workers there can still be differences associated with place of residence. They find evidence that greater job accessibility is associated with shorter durations. The study does not have a sharp focus on spatial versus racial mismatch. However, it presents a comparison of 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, the study does not present estimates from 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.

therefore be informative to have evidence on spatial versus racial (or ethnic) mismatch for other countries.

3.3. Are there important network effects in urban labor markets?

Bayer et al. (2008) present evidence of labor market network connections among nearby residents in urban areas. Using data on Boston, they find that individuals living on the same Census block are more likely to work on the same Census block than are individuals living who do not live on the same block but live in the same block group (a somewhat but still narrow geographic designation). Because people within block groups are quite homogeneous, their interpretation is that the higher likelihood or working on the same block for those who live in 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) look instead at whether neighbors work in the same establishment; this is intended to test the conjecture that neighborhood labor market networks operate in part via people getting referred to the employers of those in their network. The method compares the share of an individual's co-workers 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 employer-employee data at the establishment level for the United States (Hellerstein and Neumark, 2003). Labor networks based on place of residence would imply a higher share of neighbors among a worker's co-workers 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 percent 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 percent, and around 40 percent 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.[21]

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[21] Place of residence is treated as predetermined, potentially influencing place of work, rather than determined by people who work together choosing to co-locate residentially. This appears to be a reasonable assumption, because the results were similar when the sample was restricted to people who have lived at the same location for five or more years but have worked at their current employer for fewer than five years; for this subsample, it is highly unlikely the results are driven by co-workers who subsequently move to the same neighborhood.

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) test for this stratification by constructing the network measure two different ways: first, treating the relevant set of a black worker's neighbors and co-workers 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 neighbour – exactly what the evidence suggests. Specifically, the network measure is 40 percent lower when disregarding the race of neighbors and co-workers, suggesting that labor market information is less likely to flow between, e.g., black and white co-residents than between co-residents of the same race.

Hellerstein et al. (2010) present 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 – 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 poor English speakers. This study provides further evidence of stratified networks, and illustrates how evidence of racial or ethnic mismatch can be generated by stratified networks.

There is other evidence consistent with ethnically-stratified networks. Kasinitz and Rosenberg (1996) study 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 find that many local employers hire workers almost exclusively from outside of Red Hook, recruiting employees via social networks within specific (non-black) ethnic groups. Turning to other countries, Patacchini and Zenou (2012) find 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 couple of ways). Damm (2012), taking advantage of a quasi-experiment involving the settlement of refugee immigrants in Denmark, finds that those who were settled in areas with higher overall employment rates of non-Western immigrants and co-nationals had a greater probability of finding employment, and had higher annual earnings if employed.[22]

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

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[22] 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).

areas. Effective place-based policies may need to do more to exploit linkages between residents and workers in targeted areas.

4. IDENTIFYING 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 get adopted.[23] But 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.

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., Bondonio and Greenbaum, 2007; O'Keefe, 2004). 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) notes 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 six 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 California's enterprise zones, develop a method of precisely identifying enterprise zones 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 requires identifying the location of business establishments as inside or outside enterprise zones, because enterprise zone benefits for businesses are based on this location. Neumark and Kolko use a new data source – the National Establishment Time-Series (NETS) (see, e.g., Neumark et al., 2005a) – which provides exact street addresses for

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[23] See Baum-Snow and Ferreira, in this Handbook, for an overview of identification strategies used to uncover causal effects in urban economics research.

establishments in every year. These addresses are then geocoded to obtain precise longitude and latitude, which permits the placement of these establishment in quite exact locations within their enterprise zone (and control area) maps.

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 were either 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. For instance, Peters and Fisher (2002) estimate the effects of enterprise zones in a number of states relative to the areas of states outside the enterprise zone; and Lynch and Zax (2011) use all regions of Colorado that are not in enterprise zones. 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 characteristics of the zones or simply nearness to the zone. O'Keefe (2004) and Elvery (2009) match 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.[24] None of these studies makes use of before and after comparisons of areas observed both before and after enterprise zones were established, while other studies use these matching strategies with before and after comparisons.[25]

More recent research tries to construct more reliable control groups by using more detailed geographic information on narrow areas. Billings (2009) uses 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.[26] Neumark and Kolko (2010) use their detailed GIS maps of California's enterprise zones to pick out a very narrow control ring (1,000 feet 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

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[24] Moreover, for many of the zones the O'Keefe's matching is on post-treatment observations, which implies that the matching may mask the effects of enterprise zones by conditioning on them. Also, she matches on employment levels, whereas we would like to hold employment growth rates (in the pre-treatment period) constant between treatment and control groups.

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

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

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 4.4.

Geographic proximity of control areas does not preclude unobserved differences relative to treatment areas that 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 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) exploit the expansion of zones in their data to compare changes in employment when an area of a zone is designated relative to contemporaneous changes in areas that were designated earlier or will be designated later. To illustrate, Figure 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 1,000-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 areas in the control groups that were included in the zone at some point during the sample period were appropriate for enterprise zone designation. Busso et al. (2013) use 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 – doing comparisons with areas that become parts of zones in the future.

Hanson (2009) also compares employment outcomes in federal Empowerment Zones with unsuccessful applicant areas. However, he also instruments 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.

4.3. Identifying effects of specific policies when areas are subject to multiple interventions simultaneously

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 U.S. 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.

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 U.K. enterprise zones found that between 50 and 80 percent 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 compare results using a 2,500-foot control ring instead of a 1,000-foot control ring, to see if the estimates of employment effects are stronger using the larger ring in which positive spillovers should be less apparent. Similarly, they revert to the 1,000-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.

4.5. Studying effects of discretionary policies targeting specific firms

Some place-based policies have a discretionary nature – for example, governments 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. But comparisons to non-recipient 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 non-recipient firms may be judged by government officials to offer less scope for generating additional investment or employment. Hence non-applicant and non-recipient 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 – more closely paralleling the analysis of enterprise zones. 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 on the basis of specific economic characteristics. And there may be spillovers (positive or negative) from the policy to non-subsidized firms both within and outside the eligible areas.

Criscuolo et al. (2012) analyze the effects of a discretionary subsidy policy – Regional Selective Assistance (RSA) in Great Britain. The authors exploit the fact that the set of areas eligible for discretionary subsidies are revised every seven years according to European Union (EU) state aid rules. Under these rules subsidies could only be provided in designated Tier 1 and Tier 2 areas, and then only up to an area-specific maximum subsidy rate determined by the European Union. 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 the weighting applied to each), and changes in economic conditions in other EU member states which 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 use specific features of the eligibility rules as instruments for receipt of an RSA grant. In the estimation, they instrument a post-treatment 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 and deal with the endogeneity of participation, although this will not deal with problems of time-varying unobservables. Using plant-level or firm-level data, 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 non-zero being in principle eligible to apply).

Finally, the authors use 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, provide evidence on the magnitude of agglomeration externalities generated by the opening of a new manufacturing plant in a location. 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, although it does not examine effects on employment. As a novel identification strategy to deal with the endogeneity of the location decision, they exploit information on runner-up localities that narrowly lost out on each plant opening, and use 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 argue 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 non-treated locations. For example, if location choices are made on the basis of 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 or one-to-many matching, but with the matches determined directly from information on firms' decision-making processes. The authors estimate 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 identification of spillovers in the period after plant opening is within each matched pair.

A number of papers evaluate the effects of Law 488/1992 in Italy by exploiting a specific feature of the grant allocation process. This program offered capital investment subsidies to firms in Objective 1

regions predominantly within the South of Italy to stimulate growth and employment in lagging areas. 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 pre-allocated amount of expenditure under the program each year, and hence projects are funded in rank order until the funding pot is exhausted, with applicants with lower rankings not receiving a subsidy. 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) exploit detailed data on the actual variables used to construct the project-ranking scores. They use 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 argue 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 cut off 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 employ a difference-in-differences approach using data on firms in their pre-application year compared to the year after the subsidized project is completed. Clearly for the control group who do 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 argue that spillover effects from subsidized to non-subsidized firms are unlikely to confound the estimates since subsidized firms make up a very small fraction (around 3 percent) of manufacturing firms in the eligible regions.

Bronzini and de Blasio (2006) also look at the effects of Law 488 on investment using a difference-in-differences estimator and using applicants who did not receive a subsidy as controls. Since they find that those firms that score highly and receive a subsidy are a non-random sample of all applicants, they try to address this problem by also adopting an approach akin to a regression discontinuity (RD) design (see Baum-Snow and Ferreira, this Handbook, and Lemieux and Lee, 2010).

To do this they use narrower groups of treated and control firms that are close to the funding cut-off 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 cut-off threshold. Pellegrini and Muccigrosso (2013) also aim to identify the impact of Law 488 on the survival of recipient firms using an RD approach. They argue that receipt or non-receipt of a subsidy close to the budget cut-off point, as in Bernini and Pellegrini (2011), can essentially be considered random.

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 multi-faceted set of incentives that place-based policies provide in practice.

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 (Lynch and Zax, 2011; Elvery, 2009; Ham et al. 2011).[27]

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 the same research challenges and how other studies attempt to resolve them, but also trying to reach a resolution as to what the fairly extensive new literature says and the important questions for further research.

As the earlier discussion indicated, the multiple challenges that arise in studying enterprise zones (and other place-based policies) imply that the details of the analysis can be quite important. Hence in

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[27] An earlier review by Wilder and Rubin (1996) is somewhat more positive in concluding that enterprise zones increased economic activity. However, much of the evidence in this earlier review is hardly definitive – being based, for example, on perceptions of program participants (e.g., U.S. Department of Housing and Urban Development, 1986). See related criticisms in Boarnet (2001).

some cases we delve into these details – not with the goal of being comprehensive, but with the goal of illustrating 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.

5.1.1. The California Enterprise Zone Program

California's enterprise zone program had multiple goals – 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 percent of qualified wages (up to 150 percent of the minimum wage) in the first year, falling by 10 percentage points each year until reaching zero after five years. The main criterion 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. But 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 as well as 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).[28] 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 exploit the expansion of original zones to construct control areas, while alternative control areas come from very narrow geographic rings around the zone. They define the original zone and each expansion areas, as well as the control rings (when used) as unique

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[28] The California enterprise zone program was substantially changed in 2013, including eliminating the hiring credit, based in part on the research described here (http://www.hcd.ca.gov/ezregs/docs/EZ_Reform-InitialStatement_of_Reasons.pdf, viewed September 6, 2013).

"subzones," constructing an observation on each subzone-year pair. They specify 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 include 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 j, the effect of enterprise zone designation is identified. They also estimate models including subzone-specific linear time trends, and models that allow enterprise zone designation to shift the growth rate of employment.[29]

Other geographically-targeted policies are accounted for in two steps. First, subzone-year pairs are redefined to represent status with regard not only to 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 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 tell us 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

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[29] Because there are separate dummy variables for each subzone, an area that is in an enterprise zone for the entire sample period contributes nothing to the identification of the effects of enterprise zones. Given that the data begin in 1992, whereas most zones were originally designated prior to that year, much of the identifying information comes from expansions.

within zone boundaries but just outside zone boundaries as well, then there might be no evidence on an effect of enterprise zones on employment because enterprise zones are compared to immediately neighboring areas. But the evidence is similar using the larger (2,500-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 not 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 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 (Dowall, 1996; Elling and Sheldon, 1991; Erickson and Friedman, 1990), 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) explore 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, as well as 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) reaches similar conclusions for California. He focuses on the effects of enterprise zones designated in the mid-1980s on employment of zone residents in the 1986-1990 period. His method matches tracts that are in zones or substantially overlap with them to non-zone tracts using propensity score methods. Elvery also estimates 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 estimates the effect of enterprise zone designation on this neighborhood component, which leads to more precision.

Elvery finds 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). Indeed his point estimates are always negative, ranging from about −0.4 to −2.6 percentage points (not statistically significant).[31] Elvery does impose some sample restrictions based on age (18-55) and, in some analyses looks 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 reports. Elvery does not pay attention to overlapping programs. He pays limited attention to spillovers (without directly estimating any effects of spillovers) by excluding tracts within five miles of enterprise zones from the controls, although such large distances could imply that the controls are not as similar on unobservables.

5.1.2. Other recent evidence for U.S. 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) study of the Texas enterprise zone program is a good example of a study that addresses many of the key challenges in studies of enterprise zones. First, the study exploits an unusual

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[30] Moreover, until reforms in 2007 "cross-vouchering" was allowed, whereby one zone helps businesses from other zones get vouchers for the hiring credit, for which they could collect fees. This cross-vouchering activity likely detracted from other efforts to boost zone employment, especially if it focused on retroactive credits.

[31] As noted earlier, in California the enterprise zones are not necessarily where the targeted residents live, so looking at employment of zone residents can be the wrong metric.

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 percent or greater. Freedman therefore uses a regression discontinuity around the 20 percent cutoff to estimate effects on job growth (his main focus), as well as other outcomes.

Freedman also pays careful attention to estimating 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 in return for committing to hire a certain share of employees from enterprise zones or who are economically disadvantaged.[32] Freedman suggests 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 argues, the hiring effects should be more pronounced for zone residents than for establishments in the zone, and Freedman examines 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 accounts for overlapping state and federal enterprise zones, and, as discussed below, presents some analyses meant to avoid the effects of spillovers.

Freedman focuses most of his analysis on block groups in a narrow band around the 20 percent 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 to 2 percent, 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 clear 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 reports 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

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[32] The designated firms can get sales and uses tax refunds of up to $1.25 million over five years, with the size of the refund tied to capital investment and jobs created or retained. There is no explicit hiring credit.

more by relocation between nearby areas.

Freedman concludes 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 present results using the same research design to look at other outcomes, using American Community Survey (ACS) data. Regression estimates indicate a statistically significant 11 percent increase in median housing values at the 20 percent poverty threshold, as well as a 4 percent 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.[33]

Ham et al. (2011) study state and federal programs, and conclude 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 note, 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 five 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 $2,400 for 18-24 year-olds for the first year of employment, and Empowerment Zones also offering a credit of up to $3,000 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 focus 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

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[33] For example, Hanson (2009), discussed below, suggests that evidence of higher property values coupled with no evidence of wage and employment effects implies that employers capture the entire subsidy (vertical labor supply). Evidence of effects of enterprise zones on *commercial* property would be more compelling on this score; Burnes (2012) provides some evidence of capitalization of enterprise zone benefits in California into commercial real estate prices.

1990 in the tracts where zones were established. They then subtract 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 subtract 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 non-enterprise 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. restrict attention to tracts affected by only one of the three programs during the 1990s. And to address spillovers they estimate treatment effects for the nearest non-enterprise zone tract to each enterprise zone tract, using the second-nearest non-enterprise 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 is not the same – in contrast to comparing both to a more-distant tract that clearly was not affected by enterprise zone designation.[34]

The results Ham et al. report for the combined (average) effect state enterprise zones and for the two types of federal zones are almost always strong and positive. As summarized in Table 2, across the three types of zones, the authors generally find positive and significant effects on the outcomes they consider. Moreover, they conclude 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 consider (which is what Table 2 reports), the estimates are often implausibly large, such as an increase in employment of around 34 percent 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

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[34] Ham et al. also implement an IV estimator to address the possibility that enterprise zone designation is a response to adverse shocks, so that positive effects can appear simply from regression to the mean from those shocks. This approach requires an unusual instrument (the 1980 value of a second labor market outcome as an instrument for enterprise zone designation between 1990 and 2000). The IV estimation leads to implausible effects of enterprise zone designation (e.g., a 26.1 percentage point reduction in poverty from state enterprise zone programs).

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.[35] To increase precision, Ham et al. present 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 as well as 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) study the effect of federal Empowerment Zones. They compare 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 as well as 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.[36] In addition to substantial hiring credits, Empowerment Zones 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.[37] Busso et al. use 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 focus on the estimated impact of Empowerment Zones designated in 1993 on changes over the 1990s.

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[35] Billings (2009) reports 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 $1,000 under some circumstances, plus a job training credit).

[36] Busso et al. do not address overlap between federal and state enterprise zone programs. They also argue 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 relocation of businesses into a zone from nearby.

[37] 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. study these as a separate group of treated cities, and sometimes find 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.

Busso et al. find, in both the Census and LBD data, that Empowerment Zone designation appears to generate substantial job growth – around 21.3 percent in the LBD, and 12.2 percent (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 percent), but less evidence of such effects for non-residents (6.4 percent, not significant). The Census data also point to large increases in non-zone employment of zone residents (12.3 percent, not significant), which suggests that there may have been effects due to factors other than zone incentives.

They also find evidence of positive wage effects on zone residents working in the zone (12 percent), but no effect on wages of non-residents working in the zone. The positive point estimate for employment of non-residents in the zone is consistent with zone designation increasing 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 non-zone 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. estimate are not fully attributable to the hiring credit. And some of the other estimates they report (in their Table 5) of employment effects on non-zone residents – although not their preferred estimate – are quite large. The block grants are large, and there is some evidence – although Busso et al. appropriately note 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 non-residents, 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 find no effect of the California enterprise zone program that very much concentrated its incentives on hiring credits.

The authors also develop a stylized general equilibrium model that captures the welfare implications discussed in Section 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. find large effects on house prices. They suspect these are inflated, in part because they do not find an effect on vacancies or on out-migration, but other estimates still point to positive effects. And they suggest that rents may be sticky in the short-run because of rent control. However, other evaluations of the Empowerment Zones discussed below report evidence of house price increases as well, as well as some evidence of compositional shifts

towards the more-skilled and a decline in vacancies, suggesting smaller welfare gains.

The authors estimate relatively little deadweight loss from the program, although this is based on what is likely an overly rosy estimate that one-third of the credits claimed resulted in jobs that would not have occurred otherwise. For example, in the context of a very positive assessment of a hiring credit program in Michigan, Bartik and Erickcek (2010) estimate that the credit is decisive in 8 percent of cases, or that 92 percent of credits are paid for jobs that would have been created anyway.[38]

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 percent) the benefits would outweigh the costs, but we do know the actual impact of these block grants. Another question is whether the evidence of positive employment effects is attributable to effects of these block grants. It would therefore be useful to know how these block grants are 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) approach 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 find 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 find 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). Moreover, Reynolds and Rohlin present 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) extends the analysis of Empowerment Zones by considering the 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

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[38] In their welfare analysis, however, job creation in the zone is associated with *higher* deadweight loss because quantity adjustments imply that labor markets were distorted more.

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 U.S. House Ways and Means Committee. Hanson considers possible reasons the instrument may be invalid, such as if Ways and Means representation yields other economic benefits (or costs) to the same districts. He presents 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 member 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.[39] Hanson concludes that "OLS specifications over-estimate the effect of the EZ program on increasing resident employment and decreasing poverty" (p. 728).

Hanson and Rohlin (2013) attempt to directly estimate the spillover effects of federal Empowerment Zones on nearby or similar areas – effects that could be negative or positive. They identify tracts that are close to the Empowerment Zones, based on either geography or economic similarity, and compare 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.[40] Moreover, when they estimate 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 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 mask distributional effects that are much less favorable to the disadvantaged. Using similar but not identical methods and data, their results indicate that the zones that were advantageous to high-skilled, high-income people who to some extent likely moved into

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[39] Hanson also finds that OLS estimates of the effects on median residential property values indicate insignificant increases of around $6,600, 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.

[40] The authors do not report the means needed to calculate percentage effects, but these are large numbers relative to tract population, which average about 4,000

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 percent. 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 percent decline.[41]

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 in income, 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), as well as an increase in the share of households with income less than $10,000.[42] They also present evidence of increases in the share of people with higher 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.[43]

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 on the importance of trying to assess the

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[41] 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 an insignificant 2 percentage point decline. An earlier version of the Busso et al. study (Busso and Kline, 2006) used Decennial Census data by tract and reported evidence of reductions in poverty. Again, there are some differences that would benefit from a serious reconciliation effort.

[42] 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.

[43] 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 analysis using ACS data.

distributional effects of public policies – especially those intended to influence the distribution of income (e.g., Neumark et al., 2005b; 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 (as Reynolds and Rohlin also find) 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 (U.S. General Accounting 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 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 were 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 38 cities 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 cities (Mulock, 2002; U.S. General Accounting 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 California's 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 do specifically require job or payroll growth, Neumark and Grijalva (2013) find 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 claw-back feature, with payment linked to specified job or investment targets, is used in some of the discretionary subsidy policies discussed later.

5.1.3. Evidence from other countries

The French Zones Franches Urbaines (ZFU's) are enterprise zones modeled similarly to U.S. programs. Firms with fewer than 50 employees located within the zones' boundaries are exempt, for five 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 five 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 the ZFU's, Givord et al. (2013) address 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.[44] The authors have 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 that second group of 416. The authors' identification strategy compares changes in these 41 new ZFUs with changes in some of the remaining group that were initially identified as distressed, but not designated as ZFUs in the second round.[45] 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

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[44] Givord et al. focus on single-establishment firms because the tax information is at the firm level, and other data supposed to be at the establishment level is 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.

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

the same index, they are able to compare the ZFUs designated in the second round with zones having he same index value that were not designated, and show that the main selection criterion was that new ZFUs be 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.[46]

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

Givord et al. first show evidence not usually available – that the ZFU program resulted in substantially lower business taxes and social security contributions relative to the control areas. (One potential explanation for an absence of employment effects many U.S. studies is that most companies do not take advantage of the tax credits. We have evidence that take-up of other hiring credits can be quite low; Hamersma, 2003.) 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 percent. 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 percent compared to 25 percent.

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 percent, 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, report 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. explore 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.

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[46] In a closely-related paper on the same program, using the same data source, Mayer et al. (2012) compares ZFU and non-ZFU areas in the same city. As they argue, 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).

Using a procedure very similar to Neumark and Kolko (2010), the authors construct 300-meter 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.[47] 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.

A recent paper by Briant et al. (2012) extends the analysis of ZFUs to consider heterogeneity in 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. use 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. suggesting 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 U.S. literature as well that we need to learn more about when enterprise zones are more effective or less effective.

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 to 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 that 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

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[47] Mayer et al. (2012) also conclude 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 designation of a ZFU within the municipality.

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 U.S. 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 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 zones 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).[48]

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 not compositional shifts, leading to a rather strong positive evaluation, while other research emphasizes housing price increases (which may 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

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[48] See http://development.ohio.gov/bs/bs_oezp.htm (viewed September 6, 2013).

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 has 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 findings from a research literature in which the findings remain rather disparate.

5.2. Place-based policies that account for network effects[49]

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) echoes this point, suggesting that place-based policies should recognize "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 recent study of Jobs-Plus – a program aimed at increasing labor supply incentives for public housing residents by reducing the rent increases that accompany increases in earnings – provides some of the first evidence of which we are aware (Riccio, 1999). In particular, aside from changing labor supply incentives and 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.

Job-Plus had a clear place-based flavor, attempting to transform the community through a saturation strategy that targeted all non-disabled 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 and "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, ultimately, contribute to still more residents getting and keeping jobs" (Riccio, 1999, p. 13).

There is fairly clear 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,

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[49] The discussion in this subsection draws heavily on Hellerstein and Neumark (2012).

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) note 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) note, 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 et al., 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.[50] 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. To be fair, this is not a criticism of the evaluations of Jobs-Plus, which focused more on evaluating the overall success of the program, and did not attempt to parse out the effects of the separate components. Rather, this assessment is based on our own reading of the evidence that tried to ascertain what we could conclude about the network-related components of the Jobs-Plus program. Thus, at the end of the day, it is difficult to point to the evidence from Jobs-Plus as confirming that place-based policy efforts focusing on strengthening labor market network ties are productive. 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 earnings increases on rent, and other components of the program that assisted with job readiness and challenges such as child care and transportation.

The discussion in this section thus far has focused on how network effects might be leveraged to

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[50] Kato et al. quote an operations site representative at the Baltimore site: "A lot of times you don't talk to your neighbor, because your neighbor may be a crack addict. So you don't want to tell your neighbor about the job … you wouldn't want them working or coming to your job" (p. 30). 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).

help blacks (and others) in 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) echoes 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, 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, when 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 is a high priority for research and policy alike.

5.3. Discretionary grant-based policies

As discussed in Section 4.5, 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 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 (including inferences for welfare), for example looking at effects on productivity of participant firms and of other firms whose performance might be affected by agglomeration externalities, both within those locations targeted by the policy and outside, as well as looking at evidence of between-area displacement of employment, an issue highlighted in the discussion of enterprise zones. Much of this research also faces a similar problem 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, for example in the form of EU Structural Funds. It is therefore crucial to control for these other sources of support in order to fully 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 micro-data and find some evidence of statistically significant but very small effects of discretionary grants on firm location decisions. Crozet et al. (2004) estimate conditional logit and nested logit models to examine the effect of the Prime d'Aménagement du Territoire (PAT) on foreign multinational firm location decisions across NUTS3 areas (départements) in France. PAT grants aim to create or safeguard employment in lagging regions, and the authors report that around half of annual expenditure under this

scheme went to foreign-owned firms. The authors measure 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 robust across all specifications. Even where a positive effect is found, 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 which we cover in Section 5.5, and which may affect the attractiveness of locations for foreign investment. They consider three types of funding, under Objective 2 (funding for areas facing industrial decline), under Objective 5b (funding for rural areas), and "community initiative funds" (including funding for locally-driven projects, for example involving investment aimed at boosting declining urban areas). They find no evidence that EU expenditure in any of these three categories acted to boost the appeal of the qualifying locations for foreign investors.

Devereux et al. (2007) analyze the effect of the similar U.K. 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 only subsidize additional investment that would not have been undertaken in the absence of 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 with area characteristics. While Crozet et al. use area-level data on grant expenditure, Devereux et al. use micro data 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.[51] 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 U.K. manufacturing firms). But the magnitude of the effect was extremely small. Grants were found to be more effective in locations that had greater pre-existing employment in the relevant industry – i.e., their impact on location choices was magnified in areas which 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

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[51] 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.

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) evaluate the same policy as Devereux et al. (2007), but analyze its effect on employment, investment, and productivity, including displacement effects across areas. Their IV evaluation strategy was outlined in Section 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 European Union, which varies over time and by area and is zero in non-eligible areas. The downward bias in the OLS estimates is consistent with the idea that there is selection into both 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 percent, 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 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 those 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 argue 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 analyze impacts at the area level and find that the policy acted to increase employment both within existing firms and via new firm entry, and to reduce unemployment in these

lagging areas. They also found little evidence of displacement effects on non-participant firms or for non-eligible but neighboring areas. The only evidence they find that is suggestive of displacement is for plants that are part of larger firms, which is in line with larger, multi-plant 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 sources of regional support did not affect the estimates of the impact of the RSA scheme.

Although they do not conduct a full cost-benefit analysis of the policy, the authors perform some additional calculations on the magnitudes of their estimated effects. Their results suggest that a 10 percent subsidy rate increases area employment by nearly 2.9 percent. At the average subsidy rate of 24 percent, 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 £4,900 in 2010 prices. They report that this is lower than previous estimates of the cost-per-job under the policy, and argue that this is due to the fact that their IV strategy overcomes the downward bias affecting OLS results, which arises from the fact that participant firms and eligible areas are relative underperformers. Further calculations for the estimated number of individuals drawn out of unemployment by the policy 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 the 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) evaluate the impact of this policy on the performance of subsidized firms using a difference-in-differences estimator with unsuccessful applicants as controls. The authors consider a very 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 find 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 percent higher in subsidized firms, while employment growth was around 16-17 percent higher. Growth in physical capital was even higher at around 40 percent. Growth in labor productivity as measured by output per worker was found to be 7 percent lower, and TFP growth 8 percent lower, as a result of the subsidies. Similar to Criscuolo et al., they also find 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 be 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 tradeoff 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 non-recipient firms may have had much stronger incentives to increase their own efficiency, potentially due to increased competition from the subsidized firms.

Bronzini and de Blasio (2006) investigate whether Law 488 generates additional investment, focusing on the time profile of investment in subsidized firms relative to unsuccessful applicants. They find evidence of an increase in investment in subsidized firms two years after the subsidy was awarded. This is exactly the point at which firms undertaking a two-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 (with the final third being paid in the third year). 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 find that recipient firms show a decrease in investment relative to the control group at five years, i.e., two to three years after the final subsidy payment is made.

The authors interpret this as evidence that the policy may not in fact have generated additional

investment, but merely brought forward investment that would have happened eventually, in a similar way to which firms might respond to a temporary tax reduction. Potentially in line with this argument, they find that at the five-year point subsidized firms exhibit lower debt, which is suggestive of firms bringing forward investment and substituting public funds for private borrowing which would otherwise finance investment at a later date. One issue is how to square the findings with those of Bernini and Pellegrini (2011), who find a larger effect on capital investment. One explanation is that the latter use information on the exact completion date of the project and do 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 multi-million dollar investment grants to attract internationally mobile FDI. Greenstone et al. (2010) evaluate the effects of subsidies to attract large plants to specific U.S. counties. Rather than focusing on the effects on the recipient plant, they look 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 4.5, the study uses information on counties that narrowly missed out on winning the plant as counterfactual locations. The authors find 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-difference 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 do 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 find that five 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 percent 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 translate this average effect into an increase of around $430 million in incumbent plant output after five years. The

average effect on wages is an estimated to be an increase of around 2.7 percent. Using an equivalent econometric specification for productivity spillovers that implies an effect on incumbent plant TFP of 4.8 percent, the estimated effect on wage costs represents around 13 percent of the TFP increase (a share of expenditure on labor in total costs of 23 percent implies a total cost increase of 0.62 percent). 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 do 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 than 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 make 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 asymmetric. The existence of such asymmetric 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 just 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 as well as 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.

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 selection of businesses into the schemes (which applies both at the application and the subsidy-offer stage), as well as dealing with issues of location-based selection common to place-based policies that almost by definition target a highly non-random set of areas (see Section 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 3 provides an overview of the findings. The facts 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 recipient plants, and if, as in the case of the U.K. 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 U.S. are quite different to those typically targeted in a European context. Greenstone et al. report that U.S. counties that win a new plant have superior economic characteristics, for example in terms of incomes, population growth and labor force participation rates, compared to the rest of the country, which could reflect Glaeser and Gottleib's (2008) suggestion, discussed in Section 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 which might provide a theoretical justification for the use of such subsidies, may be highly contingent on local economic characteristics.

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.[52] 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. The answer depends on whether clusters are to some degree being formed on a sub-optimal scale, whether the optimal scale is itself identifiable, whether the precise market failures underlying any inefficiency can be pinpointed and, if so, whether cost-effective policy levers exist to overcome them. 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.

5.4.1. Clusters policies

This section discusses the rather scant evidence base on the effectiveness of cluster policy initiatives that aim to improve cluster performance, and on policies that may, as a side-effect, have influenced cluster development.

Martin et al. (2011) evaluate 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, of which there are 94) or employment area (local labor market areas, of which there are 341). 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

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[52] See Carlino et al. (2012) for recent descriptive evidence on the location and spatial scale of clusters of R&D labs within the Northeast corridor of the United States.

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 use an administrative dataset to identify a set of firms that participated in the LPS scheme. They use 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 provide 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 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 post-participation relative to the controls. In a separate examination of pre-program trends, the authors find that TFP in participating firms appeared to be declining relative to non-participants 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 look at other adjustment margins, but find no effect on employment growth or on firm-level exports.

The authors acknowledge that there may be spillover effects from participant to non-participant firms at least within region, which may affect the firm-level estimates. They therefore carry out a second estimation exercise at the industry-département level. An area-industry 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 under the policy.

The results paint a rather negative picture of the impact of the scheme. Potential explanations include that the program was too focused on lagging industry-regions, the development of which was an objective of the government agency which administered the policy, possible rent-seeking activity by firms, and insufficient funding per project to generate discernible effects.

Falck et al. (2010) analyze the effects of a cluster policy in the state of Bavaria in Germany, which is more directly focused on innovation in high-tech sectors and that appears to have been more successful, although at a substantial cost. 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 (p. 575).[53] Over this period 1.35 billion Euros were spent on initiatives aimed at improving firm performance and also at improving public-sector research in these fields, with around 50 percent 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 from before to after the program in affected industries in Bavaria to the before-after change in the same industries outside Bavaria (to control for common shocks at the industry-level), relative to the corresponding comparison of changes in non-affected industries. Using data up to 2001 (when other states within Germany started to introduce cluster policies), they estimate effects on three outcomes: an indicator of whether or not the 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 percent, 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 percent. Although at a first glance this appears a somewhat contradictory set of findings, Falck et al. suggest 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 know-how, 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) provide 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

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[53] Prior to this, from 1994 to 1999, Bavaria implemented a more general program, not targeted at specific sectors, which aimed to increase innovation and growth.

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 non-compete contracts to try and 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 develop a model under which, if the nature of the innovation process is one in which success is highly uncertain but 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 argue 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.

5.4.2. Universities

While Carlino and Kerr in this Handbook 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 highly non-random 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 (forthcoming) attempt to overcome these identification issues using an IV strategy to analyze the effect of knowledge spillovers from universities on non-education-sector labor income in U.S. urban counties. Their empirical analysis examines the relationship between non-education-sector wages and university expenditure. As an instrument for the latter they exploit exogenous variation in county-level university expenditure brought about by national stock market shocks, which affect the market value of universities' endowments, and through this, university expenditure.

The authors use 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. Time-invariant industry- and county-level characteristics are therefore differenced out. 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 to 5 percent 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 non-education labor income.[54]

The IV estimates imply that an increase in university expenditure of 10 percent increases wages for workers in the non-education sector by 0.8 percent. However, this relatively small average effect masks some heterogeneity. The authors distinguish 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 find that industries that are technologically closer to university research (measured as 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 analyze whether the presence of universities, and in particular highly-rated university research departments within universities, are associated with the location of firms' R&D facilities. They exploit data from the U.K. Research Assessment Exercise, which is used to allocate the main publicly-funded grant for research and provides ratings of individual university research departments, with the highest-rated departments deemed to be carrying out internationally-leading, frontier research. The authors link this information at the area level

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[54]The latter assumption may be problematic if unobserved trends or changes in the local economy are associated with past contributions to the endowment, potentially generating spurious evidence that university expenditure has positive economic effects. To counter this, the authors claim that endowment size is primarily determined by the date the university was established and how long the endowment has been able to grow. They 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. They also address the possibility that universities will be differentially affected by stock market shocks due to the composition of their endowment assets, by altering the first stage of the IV estimation to allow for differential responses to shocks according to measures of university quality. All these checks are supportive of the main findings.

to data on private-sector R&D labs. They find 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) find that pharmaceutical firms tend to locate their R&D labs disproportionately within 10 kilometers of world-class chemistry research departments. Similar evidence is found for R&D services firms – many of which serve the pharmaceuticals industry. However, their results cannot be interpreted as causal; in fact, they find that many of these positive relationships can be explained by the presence of science parks. And while it is quite likely that the existence of science parks is itself a result of universities' research standing, and also due to private-sector demand for premises in proximity to these universities, it remains difficult to untangle the direction of causality.

Other recent evidence in a similar vein includes Woodward et al. (2006), who find 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 find considerable heterogeneity across industries, the Woodward et al. findings are consistent with knowledge spillovers from university research across a number of high-tech sectors.

In the same spirit as Greenstone et al. (2010), Helmers and Overman (2013) exploit the existence of a runner-up site to estimate the effects of a substantial investment in scientific research infrastructure. They find that compared to the runner-up site near Manchester, the location near Oxford of a third-generation synchrotron light source (known as the Diamond Light Source) – an investment relevant to research across a number of academic fields – led to the creation of a highly-localized academic research cluster. They find an increase in related academic publications, generated both directly from research that uses the facility for experimentation and through research that does not, where the latter effect can be interpreted as an externality leading to increased academic productivity, as opposed to a direct spillover to the private sector. However the spatial scale of the externalities generated, as measured by the indirect effect on publications, is found to be very limited, within around a 25-kilometer radius. Whether the research infrastructure and subsequent increase in academic productivity lead to spillover effects to business remains unknown, with effects potentially only occurring in the longer-run. The authors could not detect any statistically significant effect on patent applications in the year after the facility opened, which, although restricted to a very short-time frame, would have been a step closer towards identifying effects on the private sector.

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 which involved an increase in student enrollment

at smaller and newer colleges, 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. But the policy could also have had effects on the demand for and supply of 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. estimate 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 differentiate between newer and older established institutions (the original six universities in operation in 1977), and consider whether effects vary with geographic distance. Overall, they find 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 effects are greater with respect to newer institutions for both measures of university presence, and finally that effects are strongly spatially concentrated. The latter results imply that over half of the estimated productivity gains accrue within 20 kilometers 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 non-education-sector TFP.

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 also 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, where university presence and scale is instrumented using measures of pre-existing 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 to be even more localized. In addition, they estimate 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. For both specifications with area fixed effects and IV specifications the results suggest a positive relationship between the measures of investment in higher education and innovative outcomes, where the estimated marginal effect of additional research staff is far larger at new institutions. 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 asymmetric across areas according to their human capital endowments, although the latter may also have evolved endogenously as a result of the policy.

As a final exercise, the paper estimates the net innovation and productivity gains at the aggregate country level by constructing a no-decentralization counterfactual whereby researchers based at new institutions created post-1987 are allocated proportionately to pre-existing institutions, and re-calculating levels of innovation and productivity in each region. The results of this exercise imply on 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. But using the labor productivity estimates, the authors back out an estimate of GDP gains from the policy of between 0.01 percent and 0.10 percent. While the authors do not attempt to reconcile these findings, they 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 (at least as measured by patents), since the results for patents seem to imply a constant elasticity of innovative output with respect to the quantity of university researchers across areas. However, the non-citation-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.

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 those 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 some degree 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, however, based 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 which 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. But 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.

5.5. Infrastructure investment and other regional and local 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 income per capita. 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 percent of the EU average. This program funds investment in cross-national transport infrastructure (see Redding and Turner in this Handbook, 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 percent of the total European Community budget (Becker et al., 2012), with ERDF expenditure of 201 billion Euros accounting for over half of this.[55] The largest amount of funding goes to Objective 1 regions with per capita GDP below 75 percent 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 5.3.

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

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 (2014). The authors not only evaluate effects on the targeted region, but also estimate aggregate effects for the United States as a whole, and examine outcomes after the funding is withdrawn. We first discuss this and related U.S. 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 U.S. states, covering nearly all of Tennessee plus areas of Kentucky, Alabama, and Mississippi. The program began in 1933, with the highest expenditure occurring during the 1940s and 1950s. Kline and Moretti report that federal expenditure totalled around $20 billion (in 2000$) from 1934 to 2000, with transfers per household during the early 1950s peaking at around 10 percent of average household income in the region.

Using pre-program data from 1930, the authors demonstrate that the set of counties covered by the program performed worse on a set of economic indicators compared to U.S. counties in general and compared to other Southern counties (the fraction of employment in agriculture was higher and that in manufacturing lower, manufacturing wages were lower, and literacy rates also lower, indicating lower productivity and human capital). Given these systematic differences in the characteristics of the TVA counties, Kline and Moretti exploit the fact that regional authorities were also 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.

The authors first look at impacts on the TVA counties for a range of outcomes over different periods: 1940-2000; and two sub-periods 1940-1960 and 1960-2000. The sub-period 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 percent per decade) and the growth rate of agricultural employment lower (around 5-7 percent lower per decade) relative to control areas. In addition, median family income growth increased around 2.5 percent per decade as a result of the policy. The results by sub-period indicate that the faster growth in manufacturing employment occurred during both periods, although the effect was around three times higher during the initial two decades (around 10-12 percent per decade during 1940-1960, as opposed to 3-3.5 percent from 1960-2000). In stark contrast, agricultural employment is found to have experienced substantially faster growth up to 1960 (around 11-12 percent per decade), but dramatically slower growth

thereafter (around 13-17 percent 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 either new workers 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 attribute the differential response of manufacturing and agricultural employment growth post-1960 to the presence of agglomeration externalities in manufacturing, discussed next, which 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 they argue that there is evidence to suggest agriculture may not exhibit the same external economies (Hornbeck and Naidu, 2013), and hence this is why the faster growth in agricultural employment did not persist.

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: first, a direct effect whereby investment in public infrastructure acts to raise private-sector productivity; and second, an indirect effect arising from the presence of agglomeration economies in manufacturing. As outlined in Section 2.1, 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 assume perfect labor mobility and estimate effects on manufacturing employment,[56] which will increase in the face of labor-productivity-enhancing investment. 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

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[56] 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 non-linear.

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. To do this they use either piecewise linear or piecewise log linear splines in manufacturing employment density, and estimate spline functions for three different sections of the distribution – low, medium, and high density. The estimates of the indirect effect using a piecewise linear spline indicate that the percentage change in manufacturing employment in response to an increase in manufacturing density of one worker per square mile is decreasing in manufacturing employment density. However, the picture changes for the estimates using a spline in the log of manufacturing employment density, which provide direct measures of the agglomeration elasticity with respect to manufacturing employment. Here there appear to be no significant differences in the estimated elasticities across the three sections of the density distribution, with a 1 percent increase in density resulting in a 0.3-0.35 percent increase in manufacturing employment.[57]

The estimated direct effect over the entire period 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. But this conceals considerable variation in the estimated direct effect over three sub-periods 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 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 U.S. counties, the only benefit is the direct benefit

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[57] Their central estimate of the agglomeration elasticity with respect to productivity is around 0.14, 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.

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 $22-$32.4 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 which arose during the earlier period were eventually eroded. But increases in manufacturing employment brought about by investment in public infrastructure spending continued well beyond the policy end date. 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 which accrued to the TVA region offset by negative effects elsewhere.

Glaeser and Gottlieb (2008) discuss the effects of the Appalachian Regional Commission (ARC) which, 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 estimate effects on ARC counties using other non-treated counties within the same states as controls (excluding those within 56 miles of the coast), and find 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 income per capita. 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 income per capita. While the stark differences in findings may be due to the alternative ways in which the authors address the problem of constructing a proxy for unobservable counterfactual outcomes, Glaeser and Gottlieb do 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) provide recent evaluations of the impact of major infrastructure investments under EU regional policy. They focus on the impact of Structural Funds on growth in employment and per capita GDP in Objective 1 regions. Becker et al. (2010) exploit data on NUTS2 and

NUTS3 regions for three rounds of the Structural Funds program between 1989 and 2006.[58] The authors use the fact that the area eligibility threshold is in principle a strict cut-off at GDP per capita below 75 percent 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 per capita GDP, with a preferred estimate of around 1.6 percentage points per annum within a funding period. However, in the vast majority of specifications they find no effect on employment growth.[59] The authors conduct a back-of-the-envelope cost-benefit analysis, which suggests that the program of transfers is cost-effective, generating a return of 20 percent, or a multiplier of 1.2. However the confidence intervals around their estimates mean that they cannot reject a multiplier of only 1. The finding that the Structural Funds expenditure only generated increased growth in GDP per capita suggests that the infrastructure investment increased productivity in the Objective 1 regions, but not employment growth, at least within the maximum 7-year window of a funding round. Their evidence of a positive effect on per capita GDP growth in Objective 1 regions is also supported by Mohl and Hagen (2010).[60] Becker et al. (2010) speculate that the findings of effects on GDP per capita, combined with no effects on employment, are driven by productivity gains from infrastructure investment, but that new job creation only occurs with a

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[58] The regional unit used to define eligibility is the NUTS2 region. The European Commission defines three levels of regional units, under the Nomenclature des Unités Territoriales Statistiques or 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, with NUTS3 regions containing 150-800 thousand inhabitants, such as départements within France.

[59] The only exception is when they consider the potential for spillovers by estimating effects also spanning neighboring regions within a 150- or 200-kilometer radius of an Objective 1 region. This results in positive and statistically significant estimated effects on employment growth in both the treated and neighboring regions. One explanation might be that the initial estimates on employment growth were biased down by the presence of positive spillovers to regions that were forming part of the control group, but an experiment excluding these neighboring regions from the estimation sample does not result in positive and significant effects for Objective 1 regions. It is also possible that workers were commuting to new jobs in Objective 1 regions from the neighboring areas, hence those individuals gaining employment were resident outside the Objective 1 regions. But given that most of the results point towards no effect on employment growth, the authors do not put much weight on this one specification.

[60] Table 1 in Mohl and Hagen (2010) 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, although there are a few exceptions.

longer time lag. Overall though, their results imply that the policy is cost-effective in increasing income in targeted regions.

Becker et al. (2012) extend this analysis to look at the relationship between the generosity of financial assistance under the program, i.e., the treatment intensity, and growth in income per capita. 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 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 use 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 identify 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 percent of GDP for a region within Sweden to 29.057 percent of GDP for a region in Greece, with an average intensity of 0.756 percent. The paper estimates the effects of different degrees of treatment intensity using generalized propensity score estimation. This non-parametric 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 also imply a non-linear relationship between treatment intensity and growth in income per-capita. 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 second the "optimal transfer intensity," which is the level at which one additional Euro of funding generates exactly one Euro of additional GDP in the average region.

They find that around 18 percent of NUTS3 regions received funding beyond the estimated maximum desirable treatment intensity threshold of approximately 1.3 percent. For these regions growth would not have been substantially lessened by a reduction in the generosity of funding to this level. In addition, they estimate 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 income per-capita by around 1.12 and 0.76 percentage points in the first and second funding rounds, respectively. They also find that around 36 percent of regions received transfers beyond the optimal

transfer intensity of 0.4 percent 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 re-distribution 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 find no evidence to suggest that a minimum level of transfers is necessary to generate increases in per capita income growth.

In summary, the evidence from both the analysis of 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 indeed 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.

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 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 reports that around 70 percent 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 are 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 studies, 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 study of Texas enterprise zones (2013) discussed earlier, although the focus is national. He uses 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 percent of the state's median for non-metro areas, and below the greater of the MSA or state median for metro areas. This is not the only rule determining eligibility, so Freedman uses a fuzzy design that instruments for actual NMTC credits with whether or not a Census tract is eligible based on this rule. He carries out many of the same kinds of analyses to validate the RD design as in the enterprise zone paper. He does 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 finds – discussed next – flow more from the real-estate development side of the NMTC. Using Census data, the main statistically significant effect Freedman finds is for reducing the poverty rate, with estimates centered on a reduction of about 0.8 percentage point. He characterizes 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 finds 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 does 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 percent, the standard errors are three times as large. Freedman generously concludes 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 communitity-development policies involve considerable local autonomy in terms of exactly how public money is spent and how interventions are designed. 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 focuses on community development with a very broad remit.

Accetturo and de Blasio (2012) evaluate the effects of the Patti Territoriali program in Italy, established in 1997. These territorial pacts aim to boost economic growth and employment in lagging areas eligible for support under 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 to together to form an agreement, or Patti Territoriali, which set out a development plan for the area. Therefore not all eligible municipalities participated in the program in practice, although the authors report 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. Private-sector incentives included grants for new investment and plant relocations.

The paper uses a difference-in-differences approach to evaluate effects on employment and on plant numbers. Clearly both the eligible municipalities and within those the subset that actually choose to participate are non-random samples of the full population. Within the North and Center of Italy not all areas are eligible to form Patti Territoriali. The authors use propensity score matching to identify similar eligible and non-eligible municipalities prior to the implementation of the program, and use difference-in-differences to estimate an intention-to-treat effect, which can be a valuable parameter for policy makers wanting to know the effect of the program on the target areas. The set of variables used in estimating the propensity score include those set out in the EU rules for defining eligibility, such as the unemployment rate and share of industrial employment.

They also estimate 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 control areas are excluded from participation due to the fact that eligibility (as an EU Objective 2 or 5b area) is first defined on the basis

of the characteristics of broad NUTS3 areas rather than at the level of individual municipalities, and hence within these wider regions there exist municipalities that would otherwise be eligible on the basis of their own characteristics, but are situated within NUTS3 areas with average characteristics that make the area as a whole ineligible. Moreover, even within potentially-eligible NUTS3 areas only some municipalities will actually be eligible, due to an EU limit on the population that can be covered by Objective 2 and 5b areas in each country, with the eligible set subject to negotiation between the European Union and the national government. This generates a second set of control municipalities within potentially-eligible NUTS3 regions that are deemed ineligible.

These evaluation approaches are not available to Accetturo and de Blasio in assessing the impact of the program in the South of 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 with those in non-participating municipalities in the South. Equivalent difference-in-difference estimates, using eligible but non-participating municipalities as controls, are calculated for the North and Center to try and assess the degree to which selection into participation on unobservables might affect the estimated program effects for the South. The authors also address the fact that the Law 488 program discussed above was running concurrently.[61]

Using Italian Census data from 1996 to 2001, the intention-to-treat estimates for the Center and North of 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 non-participating 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 non-participating but eligible municipalities, imply a positive and significant effect of the program on employment. But 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. A further check uses an alternative data set to extend the analysis period to 2007 and again finds no evidence of positive effects of the policy across all regions.

These results point consistently towards there being no positive effects of the policy on employment or on the creation of new plants. This begs the question of why it was not successful. The

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[61] For the North and Center only areas eligible for Patti Territoriali are eligible for Law 488 financial incentives, potentially biasing estimates of the effects of the Patti Territoriali upwards. For the South the non-participant areas used as controls are eligible for Law 488, which could bias estimates of the effects of Patti Territoriali upwards or downwards depending on how Law 488 affected the different areas. A robustness check is carried out using a subset of treatment and control areas that never received funds under the Law 488 program.

authors discuss two possibilities. The first is that the available funding of 50 million Euros per area was spread too thinly and hence insufficient to generate additional activity in these lagging regions. However, since this level of funding was found to be equally ineffective in both the most deprived Southern regions and in the relatively more prosperous regions of the North and Center of 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, but which focuses on community development more broadly, 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 ten-year period.

Gutiérrez Romero (2009) uses a difference-in-differences approach to analyse the effects of the program on employment outcomes in participating areas. Control group neighborhoods were selected on the basis of 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, which was commissioned for the specific purpose of assessing the impact of the policy, she finds 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 pre-program 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 benefit.

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 it is that 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, perhaps by relying on program components that were ineffective rather than those that would be more effective. In addition, the analyses of the NMTC and New Deal for Communities highlight the value of trying to isolate which sets of individuals are being affected by the policy, and whether it is reaching the target groups.

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 four 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; 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 if one points to the most positive evidence on their effectiveness, the evidence does not establish that place-based policies 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) concurs, 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. 200-1).

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, and also investment in higher education and university research. This is perhaps not surprising given the public-good nature of this type of investment. But more evidence is needed on longer-term outcomes. The findings from Kline and Moretti (2014) on the effects of the investment under the TVA program suggests that this very substantial push did generate long-lasting increases in manufacturing employment and income in the targeted region, through both 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, 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 we summarized above, for enterprise zone policies, much of the evidence to date is somewhat at odds, and presents very mixed messages as to he effects of such programs. More could be done to try and 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. Moretti (2012) comes down strongly supportive of the claim that federal Empowerment Zones were very effective, based on the evidence in Busso et al. (2013).[62] He argues that this happened, where other place-based policies failed, because the zones led to neighborhood revitalization on many dimensions by solving the collective action problem that often undermines such efforts (Moretti, 2012, p. 212). It is not clear what the basis of this claim is, although it may be based on the large block grants that accompanied Enterprise Zone designation – even if we do not know, from the research, exactly what these grants accomplished.

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 just the policy details that vary but the economic environments in which they are implemented. 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. But 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 are given targeted incentives to increase employment, does this create any unwelcome distortions to firm productivity, for example through the type of workers recruited or through

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[62] He cites an earlier version of the paper.

displacing 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 land owners 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 individuals in low-income groups, whom the policies often aim to benefit, who actually realize these gains? 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 setting hiring incentives for specific groups.

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 suggests that efforts to capture marginal tax rates may substantially narrow the range of estimates, and argues that evidence on Michigan's MEGA programs, which provided marginal subsidies to businesses that export from the local economy, shows that it is much more effective (six times more impact per dollar of foregone revenue) than cutting the overall business tax rate (Bartik and Erickcek, 2010). He also notes 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 much of this is captured 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 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 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 far from clear.

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Table 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; U.S. Federal Empowerment Zones; U.S. Federal Enterprise Communities; French enterprise zones	U.K. 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 co-operation 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 and use tax credits; tax-exempt financing; community block grants; property, corporate and wage tax relief	Reduced business rates; relaxed planning regulation; enhanced capital allowances in some cases	France: subsidy for a project, e.g., to boost exports in which firms participate; Bavaria: access to public research facilities, venture capital funding, and science parks	Reduced electricity rates; other infrastructure improvements	ERDF: Transport, telecommunications infrastructure, and investment linked to innovation or energy; ESF: training programs	Subsidy on new investment in physical capital by firms linked to jobs targets	Tax credits to investors or real estate developers, tax-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

Table 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 percent (levels), with large confidence intervals (≈ −8 to +6 percent); 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 percent; for Florida from −1 to −4 percent
Freedman (2013)	United States	Texas enterprise zones	Positive effect on employment growth among zone residents (1-2 percent 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 percent 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 percent) Significant positive effect on median home value (10.7 percent)
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 percent); employment (≈3.7 percent)[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 percent); employment (≈34.2 percent) Enterprise Communities, significant positive impacts on: unemployment rate (−2.6 percentage points); poverty rate (−20.3 percentage points); fraction of households with wage and salary income (4.9 percentage points); average wage and salary income (≈12.7 percent); employment (≈10.7 percent) Positive but insignificant spillovers on neighboring Census tracts

Study	Country	Program	Results
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 percent); magnitudes generally larger for employment in zone of zone residents (15-17 percent) than non-zone residents (6-16 percent) Positive generally insignificant weekly wage effects on zone residents employed in zone (3-5 percent); magnitudes smaller for non-residents working in zone ($\approx$0 percent), but similar for zone residents working outside zone (2-5 percent) No effects on rents, population, or vacancy rates, large significant positive effects on house values (28-37 percent)
Reynolds and Rohlin (2014)	United States	Federal Empowerment Zones	Positive effect (1.1 percent, insignificant) on difference between rent and wage premia ("quality of life") Positive effect (6.4 percent, significant) on sum of rent and wage premia ("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: generally significant effects on number of establishments (-15.2 to -36.5); negative, sometimes significant effects on employment (-52 to $-1,223$, 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 the 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 percent), but not on median household income (one-tenth as large)

Study	Country	Program	Results
			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 share of households with income < $10,000 and above $100,000 Other results point to higher-skilled, higher-income people moving in: 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; increases in housing values for houses valued at $100,000 or higher, extending above $300,000
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 percent); similar estimates for employment, but imprecise; positive effects fully offset by negative effects in 300-meter ring around zone Briant et al. (2012) report 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.

Table 3: Summary of Evidence on Discretionary Grants

Study	Country	Program	Results
Crozet et al. (2004)	France	Prime d'Aménagement du Territoire	Small, non-robust 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 plant firms and domestic multi-plant firms Heterogeneity in the effectiveness of grants in influencing location choice, with grants having a greater effect in areas exhibiting higher existing employment in the firm's industry
Criscuolo et al. (2012)	United Kingdom	Regional Selective Assistance	Positive effects on plant employment (43 percent 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 percent subsidy rate increases area employment by 2.9 percent), and negative effects on unemployment (a 10 percent subsidy rate reduces unemployment by 6.9 percent) No evidence on average of employment or plant displacement from non-eligible 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 percent higher over on average 3.6 years, employment growth 16-17 percent higher, and growth in physical capital around 40 percent higher; labor productivity growth and TFP growth 7 and 8 percent 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 two years following receipt of the subsidy, but at five 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 percent higher after five years Heterogeneity in magnitude of TFP effects across industries and across locations Positive effect on county-level wages (2.7 percent)

Figure 1: Santa Ana Enterprise Zone, Initial 1993 Designation (thick black lines), 1994 Expansion (light grey lines), and Control Ring (dark grey outer envelope)