

Rebuilding Communities Job Subsidies

David Neumark



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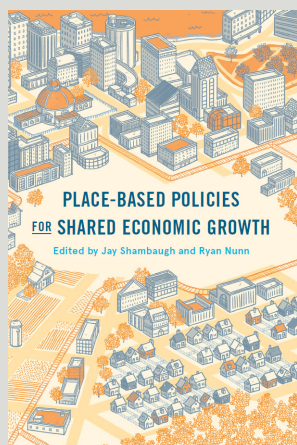


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A CHAPTER IN THE HAMILTON PROJECT BOOK



Place-Based Policies for Shared Economic Growth

For a century, the progress our nation made toward realizing broadly shared economic growth gave our economy much of its unparalleled strength. However, for the last several decades, that progress has seemed to stall. On critical measures such as household income, poverty, employment rates, and life expectancy, there exist yawning, persistent gaps between the best- and worst-performing communities. These conditions demand a reconsideration of place-based policies. The evidence-based proposals contained in this volume can help restore the conditions of inclusive growth that make it possible for individuals from any part of the country to benefit from economic opportunity.

BROOKINGS

Abstract

Poverty remains a persistent problem in many areas in the United States. Existing place-based policies—especially enterprise zones—have generally failed to provide benefits to the least advantaged. Drawing on lessons from the often-negative findings on effects of past place-based policies, but preserving the potential advantage of policies that try to improve economic outcomes in specific areas, I propose a new place-based policy—Rebuilding Communities Job Subsidies, or RCJS—to encourage job and income growth in areas of economic disadvantage. RCJS targets neighborhoods classified as extremely poor, and low-income workers in those neighborhoods, with a period of fully subsidized jobs to build skills and improve and revitalize areas of extreme poverty, to be followed by partially subsidized private sector jobs.

Table of Contents

ABSTRACT	2
INTRODUCTION	4
THE CHALLENGE	5
THE PROPOSAL	15
QUESTIONS AND CONCERNS	23
CONCLUSION	24
APPENDIX	25
AUTHOR AND ACKNOWLEDGMENTS	30
ENDNOTES	31
REFERENCES	34

Introduction

Poverty remains a persistent problem in many areas in the United States. There are numerous challenges to job creation in disadvantaged areas—urban or otherwise—that can include low skills, inadequate and decaying infrastructure, crime, and other ills. Even jobs that might be created are likely to be low-wage, low-skill jobs, and hence may do relatively little to attract workers or raise incomes. Policymakers have tried repeatedly to encourage job creation, especially in urban areas—relying first and foremost on enterprise zones. But these policies to create jobs, raise incomes, and reduce poverty in disadvantaged urban areas have generally failed, especially in providing benefits to the least advantaged.

Consequently, I am proposing a new place-based policy that I call Rebuilding Communities Job Subsidies, or RCJS, to encourage job and income growth in disadvantaged areas. RCJS retains the goal of trying to incentivize the creation of jobs in disadvantaged areas for residents of those disadvantaged areas. However, RCJS takes a significantly different approach from past place-based policies, and focuses on creating high-wage jobs and improving disadvantaged areas to lay the groundwork for future economic development. It includes the following core elements:

- RCJS will offer jobs lasting up to 18 months, fully subsidized by the federal government, with the possibility of cost sharing with state or local governments.
- The jobs must have the potential to quickly build skills that lead to good jobs in the private sector. Financial support will be provided for training to support building these skills.
- RCJS jobs will contribute to revitalizing and improving the disadvantaged areas where the jobs are subsidized.
- RCJS jobs will be administered by local nonprofits, in partnership with local employers and community groups, and perhaps larger nonprofits based elsewhere. Together these groups can identify local needs that the subsidized jobs will help address and skill-building jobs that are more likely to lead to higher-wage private sector jobs.
- After the initial 18-month phase, local nonprofits will help workers transition to private sector jobs, which RCJS will subsidize at a 50 percent rate for another 18 months. Continued employer eligibility for subsidies for new employees will depend on retention of workers placed earlier. Continued nonprofit eligibility for RCJS funds will depend on successful placements of workers in private sector jobs, and on private sector job retention.
- Eligibility for RCJS subsidies will be restricted to economically disadvantaged areas, defined as areas encompassing four to six U.S. Census Bureau (Census) tracts in which, on average, 40 percent or more of individuals are below the poverty line (i.e., the definition of extreme poverty). Within the targeted areas RCJS job subsidies will be limited to workers in families below 150 percent of the poverty line if the hired individual is already employed and 100 percent of the poverty line if the individual is not employed. Workers eligible for the subsidies must initially reside in the targeted areas.
- RCJS will not be restricted to urban areas. However, the structure of RCJS makes it more likely that it will be applied to urban areas.

The Challenge

CONCENTRATED POVERTY AND JOBLESSNESS

Significant areas of the United States, including many U.S. cities, have persistently high poverty rates, high unemployment rates, and low employment rates. Focusing first on cities, poverty rates are somewhat higher in the nation’s smaller cities: in 2016 the poverty rate was 16.1 percent in cities of less than 200,000 versus 13.1 percent in cities of more than 1 million. Moreover, in recent data poverty has fallen somewhat more in cities than it has in suburbs (Berube and Murray 2017).

Many U.S. cities continue to have large concentrations of poor people in extremely poor areas; on this metric, trends in many areas are in the opposite direction from that for urban poverty overall. For example, Kneebone, Nadeau, and Berube (2011) define Census tracts as being in extreme poverty if the poverty rate is 40 percent or higher, and define the

concentrated poverty rate as the share of poor people living in Census tracts that meet the extreme poverty definition. They find that concentrated poverty rose sharply in metropolitan areas in the Midwest over the 2000s, as well as in metropolitan areas in the South. (Note that a metropolitan area can include both the primary city and its suburbs.) While extreme poverty and concentrated poverty rose more in suburban areas in this period, concentrated poverty remains much higher in primary city areas than in the suburbs.

Based on the most recent data from the American Community Survey (ACS) for 2012–16, the concentrated poverty rate for the nation as a whole is 13.3 percent, with 6.2 million people, out of the nation’s more than 46 million people below the poverty line, living in the more than 4,000 extremely poor Census tracts. Table 1 shows that the concentrated poverty

TABLE 1.

Concentrated Poverty in the United States and in Metro Areas, by Region

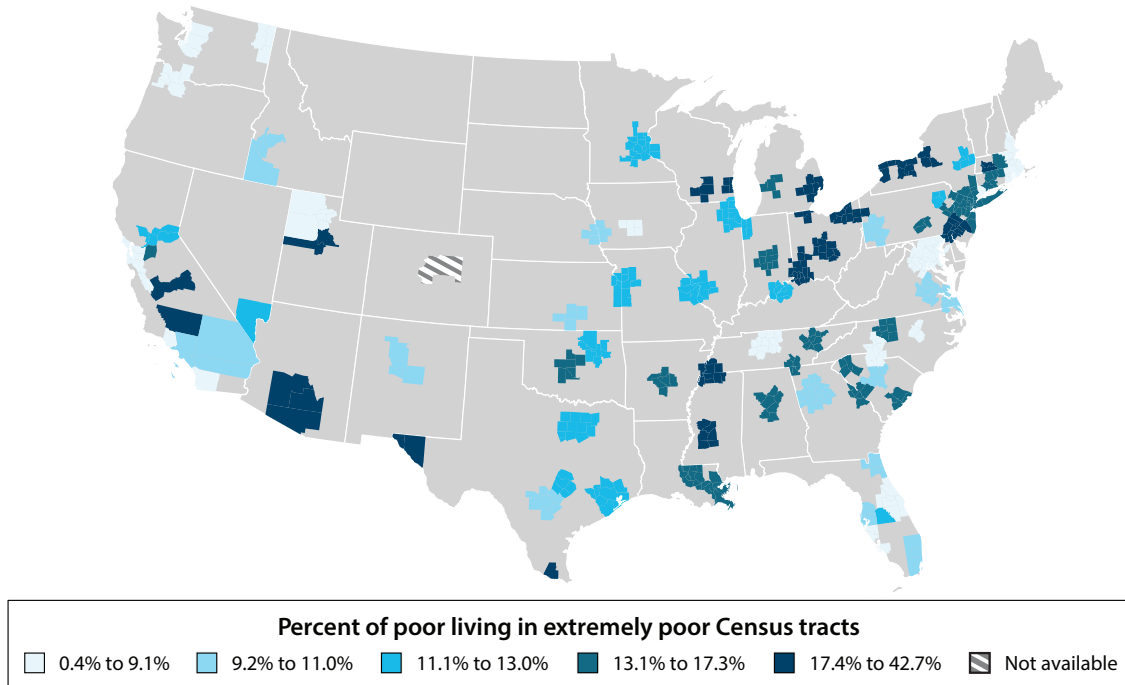
	Number of extreme poverty tracts	Poor population in extreme poverty tracts (thousands)	Concentrated poverty rate
United States	4,084	6,222	13.3%
Northeast	685	1,083	15.1%
Midwest	1,187	1,459	15.6%
South	1,562	2,431	12.6%
West	650	1,249	11.2%
Top 100 Metro Areas	3,096	4,115	14.0%
Northeast	574	939	16.5%
Midwest	830	979	19.1%
South	813	1,300	12.1%
West	470	904	11.3%
Top 101–200 Metro Areas	622	924	17.0%
Northeast	64	81	16.1%
Midwest	151	189	18.9%
South	335	499	17.9%
West	72	156	13.6%

Source: ACS (Census 2012–16); author’s calculations.

Notes: Metro area ranking based on 2012 population. Extreme poverty Census tracts are defined as tracts with a poverty rate of at least 40 percent. Concentrated poverty rate is the share of poor population within a region living in extremely poor Census tracts. Definitions are based on Kneebone, Nadeau, and Berube (2011).

FIGURE 1.

Concentrated Poverty Rates, Top 100 Metro Areas



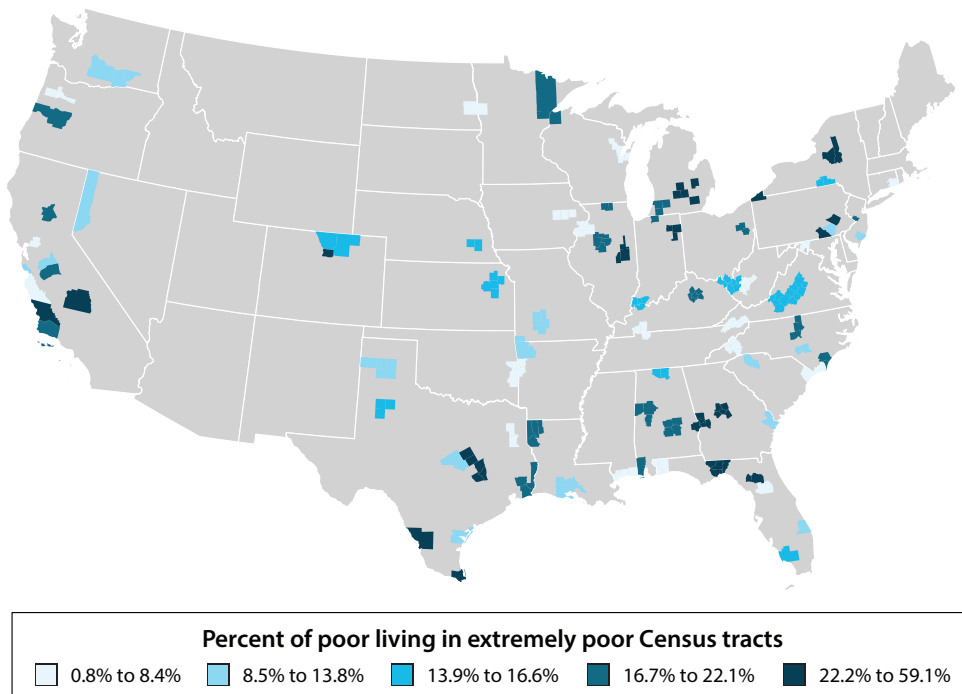
Source: ACS (Census 2012–16); author’s calculations.

Note: Metro area ranking based on 2012 population. Light gray areas are not in the top 100 metro areas.



FIGURE 2.

Concentrated Poverty Rates, Top 101–200 Metro Areas



Source: ACS (Census 2012–16); author’s calculations.

Note: Metro area ranking based on 2012 population. Light gray areas are not in the top 101–200 metro areas.



rate is high throughout the country and at different city sizes. The concentrated poverty rate is higher for the top 100 largest metropolitan areas than it is for the country as a whole, and higher yet for the next 100 metropolitan areas. In general, concentrated poverty is particularly high in the Northeast and Midwest and somewhat lower in the South and West. However, for the top 101–200 cities concentrated poverty in the South is also high (17.9 percent).

Figures 1 and 2 provide more detail, mapping the top 100 and next 100 most populous metro areas, respectively, and shading them by quintile of the concentrated poverty rate: the darker the shading, the higher the concentrated poverty rate. In figure 1, for example, we see top 100 metro areas in the highest quintile of concentrated poverty in upstate New York, Wisconsin, central California, and Mississippi, among others. And in figure 2, for the top 101–200 metro areas, the top quintile includes cities in Michigan, Pennsylvania, Georgia, Florida, and Texas, among others.

Poverty in the United States is both an urban and a rural phenomenon. Indeed, poverty is slightly higher in rural areas (16.5 percent) than in metropolitan areas (14.8 percent).¹ However, nearly 90 percent of extreme poverty Census tracts are in metropolitan areas, and concentrated poverty (i.e., the clustering of the poor in extremely poor neighborhoods) is much higher in urban areas—14 percent in metropolitan Census tracts compared to 10 percent in micropolitan tracts, and only 5 percent in small-town and rural tracts.²

Residents of extremely poor neighborhoods differ from residents of other neighborhoods in terms of a number of characteristics, including minority status, as shown in table 2. In the nation as a whole, 61 percent of residents of extremely poor neighborhoods have a high school education or less, versus 41 percent for the total population. The share of households headed by women with children is more than twice as high as for the total population. And—most importantly with respect to the proposal developed in this chapter—joblessness is high. Among prime-age (25–54 years old) men, the nonemployment rate (including unemployed and those not in the labor force) was 37 percent for residents of extremely poor neighborhoods compared to 19 percent in the nation overall. Table 3 shows that these figures are similar for the nation’s top 100 most populous metropolitan areas. These facts about neighborhoods with concentrated poverty and characteristics of their residents are related, of course. For example, Abraham and Kearney (2018) have documented the prevalence of low employment and low wages and wage growth among less-skilled men, who—as just noted—are strongly overrepresented in areas of concentrated poverty.

UNDERSTANDING JOBLESSNESS IN NEIGHBORHOODS WITH CONCENTRATED POVERTY

The focus of my proposal is on reducing overall poverty, extreme poverty, and concentrated poverty through increased employment. Nevertheless, part of the motivation for the proposal is also to address the underlying problems that

TABLE 2. Neighborhood Characteristics of Extreme Poverty Census Tracts in the United States

	Extreme poverty tracts	All tracts
Unemployment rate	15.3%	7.4%
Prime-age nonemployment rate	41%	24%
Prime-age male nonemployment rate	37%	19%
Single mother households	16%	7%
Race and ethnicity		
White	47%	73%
Black	35%	13%
Asian	4%	5%
Hispanic	29%	17%
High school education or less		
Male	63%	42%
Female	59%	39%

Source: ACS (Census 2012–16); author’s calculations.

Note: Unemployment and labor force variables are based on a weighted average by Census tract labor force. Other estimates are based on a weighted average by Census tract population. Extreme poverty Census tracts are defined as tracts with a poverty rate of at least 40 percent, based on definitions from Kneebone, Nadeau, and Berube (2011). White, black, and Asian refer to reported single race. Hispanic includes all races.



TABLE 3.

Neighborhood Characteristics of Extreme Poverty Census Tracts in the Top 100 Metropolitan Areas

	Extreme poverty tracts	All tracts
Unemployment rate	15.9%	7.4%
Prime-age nonemployment rate	41%	23%
Prime-age male nonemployment rate	36%	17%
Single mother households	17%	7%
Race and ethnicity		
White	41%	69%
Black	38%	14%
Asian	4%	7%
Hispanic	34%	20%
High school education or less		
Male	65%	39%
Female	61%	37%

Source: ACS (Census 2012–16); author's calculations.

Note: Metro area ranking based on 2012 population. Unemployment and labor force variables are based on a weighted average by Census tract labor force. Other estimates are based on a weighted average by Census tract population. Extreme poverty Census tracts are defined as tracts with a poverty rate of at least 40 percent, based on definitions from Kneebone, Nadeau, and Berube (2011). White, black, and Asian refer to reported single race. Hispanic includes all races.



contribute to low employment and continuing neighborhood poverty and disadvantage.

In the context of neighborhoods with many poor and minority residents, a long-standing theory of low employment is spatial mismatch. The spatial mismatch hypothesis—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 available jobs per worker in these areas (Ihlanfeldt and Sjoquist 1998, 851). Spatial mismatch can emerge because of the exit of jobs from these areas with the changing industrial structure (Wilson 1987), and can persist because of residential segregation attributable at least in part to discrimination in housing markets.³ Researchers have hypothesized that spatial mismatch can be exacerbated by inadequate transportation from urban cores to suburban jobs (Gobillon, Selod, and Zenou 2007; Kain 1968).

The segregation of disadvantaged groups into areas with fewer jobs, in addition to inadequate transportation to jobs in other places, implies that wages, minus any commuting costs, are more likely to be below the wages at which individuals would be willing to work. This means that fewer residents of such areas would choose to work, especially among the less-skilled for whom commuting costs represent a larger share of earnings. Spatial mismatch can be reinforced by discrimination against minorities—both by customers and employers—and inadequate information about jobs in other areas (Ihlanfeldt and Sjoquist 1998). Together, these factors

suggest that increasing employment in neighborhoods with many disadvantaged residents will probably require creating jobs in those same neighborhoods.

But research by Hellerstein, Neumark, and McInerney (2008) suggests that this may not be enough. The authors note that racial and ethnic discrimination in hiring can generate evidence that appears consistent with spatial mismatch but that is not fundamentally due to scarcity of jobs where minorities live. Instead, they find support for an explanation they call racial mismatch. Specifically, black job density (the ratio of local jobs held by black workers to black residents) is strongly positively related to black employment, whereas white job density (the ratio of local jobs held by white workers to black residents) is not. This evidence indicates that the spatial distribution of jobs alone is not a critical determinant of black urban employment; the racial dimension in hiring must also be taken into account.⁴ In other words, even if there is a high black population in areas that are dense in jobs, they might not be able to access these jobs due to discrimination and/or racially segregated labor market networks. One implication is that hiring incentives intended to reduce poverty by spurring job creation in disadvantaged minority areas should incentivize hiring of *local* residents. Simply bringing jobs to these areas might not be enough.

Longer-term changes in labor supply and labor demand as well as in institutions that support wages at the low end of the distribution also play an important role in joblessness in areas

with many poor residents. Lower demand for less-skilled workers has been attributed to skill-biased technological change and trade (see citations to the evidence in Abraham and Kearney 2018). And a good deal of research points to the role of weakened unions and lower minimum wages in contributing to lower wages for less-skilled workers (e.g., DiNardo, Fortin, and Lemieux 1995), which can in turn lead to negative labor supply responses (see Juhn 1992 for earlier evidence, and Moffitt 2012 for more-recent evidence). Of course, had unions or minimum wages done more to prop up wages of less-skilled workers, labor demand for these workers would presumably have been lower.

Finally, other factors may discourage job creation in disadvantaged areas. The exit of more-affluent customers, perhaps in part due to crime, blight, and decaying infrastructure, can reduce demand for the products or services of some kinds of businesses (Alwitt and Donley 1997), and make it hard to attract higher-skilled workers. Higher costs for labor, insurance, rents, and loss/theft can deter business and job creation (Hammel 1991; Porter 1995). In my view, these factors have been underemphasized in discussions of job creation policies for neighborhoods with high concentrations of poverty.

EVIDENCE FROM PLACE-BASED POLICIES

A review of the evidence on impacts of key place-based policies—including enterprise zones, empowerment zones, and enterprise communities, among others—suggests a few

core lessons that inform the proposal in this chapter.⁵ This section reviews the evidence and highlights certain lessons:

- Labor market networks are often segregated by location and race (see box 1), increasing the importance of hiring *residents* of low-income areas.
- Hiring credits alone would tend to reallocate jobs across places rather than create new jobs—in other words, they generate negative spillovers for other places.
- Migration into enterprise zones, among other factors, can redirect benefits away from the low-income residents who are the intended beneficiaries of the policy.

A critical factor in explaining urban concentrations of poverty and disadvantage is that labor demand is likely to be low in poor, urban areas. Consequently, policymakers have adopted policies to boost labor demand in these areas.

Policies that target geographic areas are termed place-based policies because they create criteria for policy eligibility based on location characteristics, such as the poverty rate in a Census tract. In contrast, people-based policies try to help the disadvantaged without regard to where they live or how concentrated they are residentially; examples include welfare and working tax credits (such as the Earned Income Tax Credit [EITC] in the United States).

Place-based policies do not have to target disadvantaged areas. One example would be government assistance to

BOX 1.

The Role of Labor Market Networks

Researchers studying enterprise zones have been particularly interested in understanding the factors that may amplify the effects of boosting labor demand in an area. One such factor may be labor market networks. In network models, employment of some residents increases the flow of information about job opportunities to other residents. Networks can also increase the flow of information about workers to employers. Both of these effects reduce the cost of worker-firm matching and increase employment (e.g., Montgomery 1991). Networks often have a spatial dimension, such as connecting neighbors and employers in the same Census tract or neighborhood (Bayer, Ross, and Topa 2008; Hellerstein, Kutzbach, and Neumark 2014; Hellerstein, McInerney, and Neumark 2011).

The potential amplification of local hiring impacts can be particularly important given that residential segregation by race or ethnicity can interact with racially or ethnically stratified labor market networks, making it particularly hard to, for example, boost hiring of black workers in disadvantaged areas.⁶ Hellerstein, McInerney, and Neumark (2011) present evidence of racial stratification of labor market networks that suggests white residents are more likely to share information about jobs with white neighbors than with black neighbors (or more likely to refer a white neighbor than a black neighbor for a job). There is other evidence consistent with ethnically stratified networks that further emphasizes the importance of local hiring.⁷ As a striking example, Kasinitz and Rosenberg (1996) study the Red Hook section of Brooklyn, an area that at the time was characterized by high unemployment and a large population of low-income black residents, and to some extent Hispanic residents, but with a large number of local jobs in the shipping industry. They found that many local employers hired workers almost exclusively from candidates outside Red Hook, recruiting employees via social networks within specific (non-black) ethnic groups.⁸

subsidize the creation of an industry cluster in some area of natural advantage (e.g., natural resources or the presence of a university). A second example would be efforts to revitalize a downtown area through real estate development incentives. The standard arguments considered in the urban economics literature to justify this type of place-based policy typically have the following form: government policy can encourage development in a particular place, thereby increasing efficiency through so-called agglomeration externalities that arise when economic activities occur in close proximity.

However, place-based policies, at least in the United States, usually refer to policies targeting disadvantaged areas, and that is the definition used in this chapter.⁹ The most prominent place-based policy in the United States is enterprise zones, which seek to create jobs in or near areas where poor people live and job prospects are weak. Given that I am focusing (although not exclusively) on urban poverty and disadvantage, and that my RCJS proposal is based on both criticisms of and lessons learned from enterprise zone programs, the discussion in this section focuses on enterprise zones.

Neumark and Simpson (2015) further distinguish between direct and indirect place-based policies. Direct forms of place-based policies seek to increase economic activity and strengthen labor markets where disadvantaged people currently live, whereas indirect policies instead seek to increase the access that disadvantaged people have to locations where labor markets are stronger. Enterprise zones can be viewed as direct place-based policies since they typically create

incentives for hiring and other economic activity in or near areas where disadvantaged people live.

The Gautreaux and Moving to Opportunity (MTO) programs in the United States (as well as transportation-based policies intended to increase access to jobs outside the areas where the disadvantaged tend to reside) are examples of indirect policies.¹⁰ The evidence from Gautreaux and MTO allows us to test behavioral hypotheses about the effects of growing up in different kinds of neighborhoods. However, I do not view Gautreaux and MTO as feasible urban policies for the simple reason that they cannot be implemented on a large scale. It is probably infeasible to successfully encourage massive numbers of poor people to move out of urban areas with high poverty. Even in the event that this were accomplished, the effects of the policy when implemented broadly could be far different from what the experimental evidence has found.

In contrast, other policies motivated by spatial mismatch concerns could in principle be taken to larger scale. For example, if there is a dearth of jobs for low-skilled workers in urban areas with concentrated poverty but strong demand for such workers in outlying suburban areas, then investments in transportation infrastructure that make urban-to-suburban commuting more feasible could substantially raise employment in urban areas with high poverty rates.¹¹ However, the evidence regarding labor market networks and racial mismatch described above suggests that the effectiveness of such policies could be substantially limited by racial and ethnic discrimination in hiring. Moreover, commuting

BOX 2.

Enterprise Zones and Related Policies in the United States

Enterprise zones and related policies are intended to increase employment and generate local economic growth and development in disadvantaged areas. The general term “enterprise zone” is often used to refer to a number of different state and federal policies, but the more-specific designations are defined below.

- **State enterprise zones** existed in 40 separate state programs as of 2008 (Ham et al. 2011). These programs vary in terms of budgets, the number of zones in each state, targeting, and the benefits available, but business hiring credits typically featured prominently.¹²
- **Federal empowerment zones and enterprise communities** were authorized in 1993, allowing local governments to submit proposals for zones made up of relatively poor, high-unemployment Census tracts. Far more enterprise communities—which have much less generous benefits—were created than were empowerment zones.¹³
- **New Markets Tax Credit (NMT) program** provides individuals and corporations with federal tax credits for investments in economically distressed communities. The program was established as part of the Community Renewal Tax Relief Act of 2000, and finally expired in January 2014 when Congress did not renew the program.
- **Opportunity zones** were created by the Tax Cut and Jobs Act of 2017, allowing investors to reduce their capital gains tax burden by investing in designated opportunity zones. Zones were selected by the Treasury Secretary in April 2018 from a pool of localities nominated by the states.

costs—including time—could still pose substantial barriers for low-wage workers.¹⁴

Policies such as enterprise zone programs—which create incentives for the creation of jobs (and other investments) in neighborhoods with concentrated disadvantage—have the potential to create jobs where disadvantaged people live and can plausibly be taken to very large scale. Perhaps reflecting this potential, enterprise zone programs have been used extensively in the United States, at both the federal and state levels (see box 2 for more details).¹⁵

Weak Evidence for Positive Impacts of Enterprise Zones

The problem that prompts my RCJS proposal is that most enterprise zone programs have failed to deliver the goods—creating jobs and raising incomes for the least-advantaged people in neighborhoods with high concentrations of low-income residents. In part to make the case that we need to consider different place-based policies, but also to draw lessons from the existing research, in this subsection I provide a fairly comprehensive review of this evidence, with a focus on the U.S. experience (see box 3 for a summary).¹⁶

Although most of the existing research focuses on estimating the effects of place-based policies on residents and/or areas, some also tries to assess effects on welfare. One key question regarding welfare effects is whether the intervention largely reallocates economic activity from one place to another, or instead generates gains in output (e.g., because of agglomeration effects). Of course, even pure reallocation can

be a legitimate policy goal. A second issue is whether policy that targets specific places creates distortions to capital and labor mobility, lowering efficiency by reducing incentives of firms or individuals to move to more-productive locations (Glaeser and Gottlieb 2008). Economic theory suggests that the consequences of place-based policies for economic welfare depend on whether the policy generates benefits for the targeted area—including newcomers—and more specifically for those originally resident in the targeted areas. As Crane and Manville (2008) emphasize, jobs that are created may go to nonpoor residents or migrants, and gains from land prices seem unlikely to accrue to the poor.¹⁷ These considerations imply that evaluations should look beyond evidence of effects on local employment to effects on local unemployment, transitions of local residents into jobs, and changes in commuting patterns, as well as effects on rents and house prices, to help assess who gains from these policies.¹⁸

A key challenge in estimating effects of enterprise zone programs is that selection of geographic areas for the programs occurred on the basis of unobserved area characteristics, which may differ from those of comparison places. In other words, policymakers do not choose areas at random, but rather do so on the basis of characteristics such as past or expected job growth. This non-random selection requires that researchers choose control areas carefully to make statistical comparisons.¹⁹

A second problem in studying the effects of place-based policies is spillover effects between areas. Evidence that enterprise

BOX 3.

Summary: Weak Evidence for Positive Impacts of Enterprise Zones

On net, the weight of evidence is that the hiring credits in enterprise zones have had limited if any positive impact, especially on poor households. Research on three specific state programs (California, Florida, and Texas) concludes that two of them generate no employment effects, and the third (on Texas) finds positive effects concentrated on lower-paying 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, although 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. There is little reliable evidence that state enterprise zones reduced poverty or helped low-income families generally.

Evidence from analyses of the U.S. federal Empowerment Zones Program is also mixed. One study finds strong effects on job growth, whereas another suggests that if we fully account for differences between zones and other places 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 would be challenging, given the small number of affected zones; these grants may have done more to increase the attractiveness of zones to higher-income people.

The evidence on spillovers is also mixed, with some studies suggesting negative spillovers that offset program benefits. There might 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.

zone designation led to job growth might be regarded quite differently depending on whether the zone created new jobs on net, or whether employers moved from one area to another to take advantage of enterprise zone credits. The latter would imply negative spillovers on areas outside the enterprise zones.²⁰ Although negative spillovers do not necessarily imply that a program has failed, they do reduce the social benefits of the policy. There can also be positive spillovers. For example, an enterprise zone might increase traffic or income in a geographic area, spurring demand and hence job growth in nearby areas; these positive spillovers could bias the estimated effect of enterprise zones on employment toward zero when comparing enterprise zones to neighboring areas that were also positively affected.²¹

With these issues and challenges in mind, I now turn to an overview of empirical results from the enterprise zone literature. The results from early generation 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; research reviewed in Wilder and Rubin 1996) found positive employment effects, at least in the short run. More-recent overviews of the literature conclude that it is difficult to find evidence of positive employment effects of enterprise zones (Elvery 2009; Ham et al. 2011; Lynch and Zax 2011). However, in the past decade or so many studies of enterprise zones have made creative use of both data and econometric methods to try to provide more-rigorous evidence on the effects of enterprise zones. This literature is summarized in appendix table 1.

Studying the California enterprise zone program, Neumark and Kolko (2010) find estimates that are small, statistically insignificant, and negative as often as they are positive, even when correcting for the causal issues outlined above and accounting for overlap among other redevelopment programs. The null effects do not appear to be driven by positive spillovers mentioned above, since the evidence is similar using larger control rings.

Elvery (2009) focuses on the effects of enterprise zones in California and Florida, designated in the mid-1980s, on employment of zone residents in the 1986–90 period. He finds no evidence of positive effects of enterprise zones on employment; indeed, his point estimates are always negative, ranging from about –0.4 to –2.6 percentage points (though not statistically significant). Freedman’s (2013) analysis of the Texas enterprise zone program exploits the fact that Census block groups were automatically designated as enterprise zones based on whether the poverty rate in the 2000 Census was equal to 20 percent or greater. Comparing locations near the 20 percent cutoff, he estimates effects on annual resident employment growth of 1 to 2 percent, which are fairly large when accumulated over a number of years. These employment

effects are concentrated in jobs paying less than \$40,000. However, when he accounts for the possibility of negative spillovers, very few of the employment estimates are significant, and in some cases the positive effects on resident employment become smaller or even negative. This is consistent with the positive findings being driven by relocation of jobs between nearby areas.

Freedman (2013) also finds a statistically significant 11 percent increase in median housing values in block groups near the 20 percent poverty threshold, as well as a 4 percent decline in the share of housing units that are vacant. At the same time, the data indicate no change in median household income. One interpretation is that the main effect of enterprise zone designation seems to have been an increase in land value—a finding that arises in other studies, some of which suggest that this might be a principal effect of enterprise zones (e.g., Hanson 2009).²²

Ham et al. (2011) study both state and federal programs. 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. Enterprise zone benefits vary widely across these states; for example, there are very large hiring credits in California and Florida, and negligible or no hiring credits in Ohio and Oregon. The results Ham et al. report for the combined (average) effect of state enterprise zones and for the two types of federal zones are almost always strong and positive. Particularly striking are their estimated effects on poverty reduction—a key goal of these programs. Some of their estimates are extraordinarily 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. Large positive effects of enterprise communities are especially surprising given that other researchers regard enterprise community benefits as inconsequential relative to empowerment zone benefits (Busso, Gregory, and Kline 2013; Hanson and Rohlin 2013).²³

The large benefits of enterprise zones that Ham et al. (2011) estimate are outliers in the enterprise zone literature, as appendix table 1 makes clear. Furthermore, a reexamination of this evidence casts serious doubt on the findings (Neumark and Young 2017). The large poverty reductions that they attribute to state enterprise zones are driven almost entirely by a data error. And their estimated effects of federal empowerment zones and enterprise communities appear to be overstated because treated zones are not comparable with comparison locations; accounting for this reduces the estimated impacts of empowerment zones and suggests that their estimated positive effects of enterprise communities are spurious.

Busso, Gregory, and Kline (2013) study the effect of federal empowerment zones, comparing 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 to areas that would eventually be designated as empowerment zones.²⁴ 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 no dedicated hiring credits. The authors focus on the estimated impact of empowerment zones designated in 1993 on changes over the 1990s, finding that empowerment zone designation appears to generate substantial job growth—between 12 and 21 percent. Moreover, the authors find that there were increases in jobs in the zone held by residents (17.6 percent), but less evidence of such effects for nonresidents (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). The fairly large estimated employment effects for zone residents working outside the zone suggest that the effects on zone employment are not fully attributable to the hiring credit. The block grants were substantial, and there is some evidence—although Busso, Gregory, and Kline 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.²⁵ This could have boosted employment of nonresidents in the zone, and perhaps, through spillovers, employment of zone residents outside the zone. If in fact the block grants played a major role, this might help square the results of Busso, Gregory, and Kline with other weak evidence of effects of enterprise zones on job growth.²⁶

Other studies of the impact of federal empowerment zones lead to less-favorable conclusions. Hanson (2009) finds that selection into the program is based on unobserved improvements in economic conditions, biasing other studies toward estimating positive effects.²⁷ After taking this into account, he finds no significant effects on employment.

Careful accounting for negative spillovers is also important for understanding the impacts of place-based policies. 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 similar to the empowerment zones—in terms of either geography or economic characteristics—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 on establishment counts and employment. Hanson and Rohlin (2013) suggest that empowerment zones are, to a first-order approximation, simply reallocating economic activity among similar areas.

Yet another concern is that positive average effects of empowerment zones may mask distributional effects that are much less favorable to the disadvantaged. Reynolds and Rohlin (2015) conclude that the zones were advantageous to high-skilled, high-income people who, to some extent, likely moved into empowerment zones because the program made these areas more attractive. In contrast, the zones were neutral or even harmful to the impoverished residents of these zones. They find that the effects on median household income and poverty were small and statistically insignificant.²⁸ Thus, these results present a more-negative portrait of federal empowerment zones as failing to deliver on the goal of helping low-income families than the evidence presented in Busso, Gregory, and Kline (2013) and Ham et al. (2011).²⁹

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. 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. For example, Wilder and Rubin (1996) concluded 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)” (478). And more-recent evidence in Kolko and Neumark (2010), which supplements their analysis of California enterprise zones with a survey of enterprise zone administrators, finds, among other results, that enterprise zones have a more-favorable effect on employment in zones where managers report doing more marketing and outreach activities. One implication of these findings is that the overall evidence from the research literature on enterprise zones may be somewhat too pessimistic, and that it might be possible to find ways to make enterprise zones more effective at creating jobs.

However, it is very hard to make the case that the research establishes the effectiveness of enterprise zones in terms of job creation, poverty reduction, or welfare gains. At the same time, there is an obvious appeal in using place-based policies to try to improve socioeconomic conditions in neighborhoods that have concentrated disadvantage. These lessons inform my proposal, which tries to combine some of the potential benefits of encouraging job creation in neighborhoods with many disadvantaged residents, but differs from enterprise zones in ways that are likely to prove more beneficial to these neighborhoods.

Opportunity Zones Unlikely to Ameliorate Concentrated Poverty

Despite the unconvincing track record on enterprise zones, the Trump administration recently revealed a new version

of place-based policies, designating opportunity zones in 18 states, as part of the Tax Cuts and Jobs Act of 2017. While intended to spur job creation, and targeted at disadvantaged Census tracts, opportunity zone incentives are directed at investors in property, allowing deferral or avoidance of federal taxes on capital gains in investments in these zones (U.S. Department of the Treasury 2018).

Past research may provide some insight into the potential effects of such a policy. Freedman (2012) examined the federal New Markets Tax Credit program, which has some parallels to the opportunity zone program in its focus on real estate development, although it also subsidizes capital investments for businesses through loans or preferential interest rates. For the period studied (2002–09), the NMTC provided \$26 billion in tax credits to investors making capital investments mainly in businesses located in moderately low-income neighborhoods, defined as tracts that had median family incomes below 80 percent of the state’s median income, based on metro status, in the 2000 census. Freedman reports that around 70 percent of the funds went to commercial real estate development, and most of the rest went to business development, mainly as loans to firms.³⁰

The evidence suggests that there is a jump in NMTC investment just below the tract-level median family income eligibility threshold—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 (2012) finds flow more from the real estate development side of the NMTC. Freedman finds a modest poverty-reduction effect, which he characterizes as limited and costly, with a cost of about \$23,500 to lift one person out of poverty. At the same time, he also finds some evidence consistent with compositional changes, with a few of the estimates indicating increases in household turnover of about 0.75 percentage points. 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 estimates very close to zero, which is less consistent with a compositional change toward higher-income, higher-skilled people.³¹ Given 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. This likely does not bode well for the success of the new Opportunity Zones program.

The Proposal

There are many reasons for policymakers to be interested in improving employment opportunities in urban areas that have concentrated disadvantage. As discussed in the introduction to this volume, these include reasons of efficiency and equity. Successful policies could increase the tax base, reduce crime, improve outcomes for children, spur human capital investment, and generate positive externalities for other city residents.

Although the benefits of improving socioeconomic circumstances in neighborhoods with high concentrations of poverty will accrue to many city residents, such benefits will likely accrue in particular to nonpoor members of minority groups, who tend to cluster residentially in poor areas.³² Research suggests that living in poverty areas creates extra hardships for the poor and nonpoor alike, owing to less private sector investment, higher crime, weaker labor market networks, and worse health, as well as decaying infrastructure and lack of quality physical public goods (Dempsey 2008; Schilling and Logan 2008; Wiewel and Persky 1994; Wilson 2008).³³

Finally, recent research indicates that policies that deliver gains in employment and income in disadvantaged areas may have important short- and long-term positive spillovers for places. Shorter-term spillovers can arise from network effects that generate positive multipliers from local hiring (e.g., Hellerstein, McInerney, and Neumark 2011; Piil Damm 2014). Chetty et al. (2014) show that the disadvantage of neighborhoods can have lasting impacts on the next generation. I am therefore proposing a new approach to creating job growth in areas with high concentrations of disadvantage, which I call Rebuilding Communities Job Subsidies, or RCJS. RCJS retains the goal of trying to incentivize the creation of jobs in urban and other areas of concentrated disadvantage for residents of those disadvantaged areas (defined as sets of Census tracts). But it represents a significantly different approach from enterprise zone programs. RCJS is characterized by the following core elements.

First, RCJS will subsidize jobs in two phases. In the first phase, lasting up to 18 months, RCJS jobs will be fully subsidized by the federal government, with the possibility of cost sharing with state or local governments; workers in these jobs generally

will be deployed by local nonprofits. In the second phase workers will transition to private sector jobs. These jobs will be subsidized at a 50 percent rate by the federal government for the first \$30,000 of annual earnings—again, with the possibility of cost sharing with state or local governments—for an additional 18 months, on the condition that these jobs require some of the skills that workers develop in the initial period. Private sector employers claiming RCJS do not need to be located in the target areas, although workers must initially reside in those areas. Employer eligibility for subsidies for new employees will be terminated if the retention rate of prior employees hired under RCJS falls below 50 percent within one year of the end of the subsidies.³⁴ Continued eligibility of nonprofits for RCJS funds will depend on successful placements of workers in private sector jobs, and the same retention criterion.

Second, RCJS jobs in the first phase must contribute to revitalizing and improving the areas of concentrated disadvantage where the jobs are subsidized.

Third, RCJS jobs in the first phase must be administered by local nonprofits, in partnerships with local employer and community groups, with the optional support of larger nonprofits based elsewhere. Together, these groups will identify local needs that the subsidized jobs help address and structure subsidized jobs such that they quickly build skills and effectively lead to successful private sector job placements. Financial support will be provided for training to support building these skills.

Fourth, RCJS job subsidies will be limited to workers in families below 150 percent of the poverty line if the hired individual is already employed, and 100 percent of the poverty line if the individual is not employed. And eligibility for RCJS subsidies will be restricted to areas that are substantially disadvantaged, defined as areas encompassing four to six Census tracts in which, on average, 40 percent or more of individuals are below the poverty line (the definition of extreme poverty). RCJS is not restricted to urban areas. However, the structure of RCJS makes it more likely that it will be applied to urban areas.

Fifth, RCJS should be administered by the U.S. Department of Housing and Urban Development (HUD), which has administered federal enterprise zone programs. HUD

would establish a competitive application process, soliciting proposals from the types of nonprofits outlined in the proposal, and choosing potential treatment areas based on two criteria: expected success at leading participants to higher-paying private sector jobs, and improvement of the targeted area via the jobs subsidized in the first phase of the subsidies. Program administrators might also want to give preference to applications that include cost sharing by state or local governments, although this should receive little weight to maintain the broadest availability of RCJS in places with little fiscal capacity.

RATIONALES FOR CORE ELEMENTS OF RCJS

The two-phase structure of RCJS—initial, fully-subsidized jobs with local nonprofits, followed by partial subsidies of private sector jobs—is intended to accomplish two goals: first, to create a strong incentive for a fast ramp-up in job creation; and, second, to induce the transition of workers in these subsidized jobs into higher-paid jobs in the private sector. With regard to job creation, there is evidence from past guaranteed/subsidized jobs programs (e.g., the TANF Emergency Fund during the Great Recession) that take-up of generous subsidies could be large and fast, and could lead to some positive post-program effects (see the discussion below). The subsidies then phase out over time, contributing to the goal of increased economic self-sufficiency. RCJS is intended to be complementary with other policies, such as the EITC, which provide ongoing work subsidies (to workers, rather than to employers) for workers in lower-income families.

Improving Neighborhoods

The focus on improving neighborhoods is intended to encourage the creation of jobs that, in addition to helping workers and their families, also increase the productive potential and quality of life in the targeted areas. We know from existing research that there are deeper problems in urban areas that simple hiring credits, even if effective, are unlikely to address. These problems are often related to poor infrastructure such as parks and schools, safety, side effects of depopulation such as vacant lots and abandoned homes, and a need for tutors or health-care providers. (See, e.g., Dempsey 2008; Schilling and Logan 2008; Wiewel and Persky 1994; Wilson 2008). The focus on neighborhood improvement also reflects the concern that place-based policies lead largely to reallocation, rather than creation, of economic activity.

There is no way to guarantee that a neighborhood that improves because of a policy intervention such as RCJS (or any other place-based policy) will not generate some relocation of economic activity or displacement of the most-disadvantaged residents. However, by generating actual physical (or human capital) improvements, RCJS can do more than simply reallocate jobs and people, making it more

likely that the program will, on net, result in improvements to disadvantaged areas and skill increases among the residents of those areas. Unfortunately, we as yet know very little about whether place-based policies lead to sustained growth in jobs and income after subsidies end. There is evidence of a long-term benefit from the Tennessee Valley Authority (Kline and Moretti 2014a), although the applicability of this evidence to much smaller-scale place-based policies is questionable. Nonetheless, it seems reasonable to presume that focusing job creation under RCJS on jobs that improve neighborhoods is more likely than simple hiring credits to generate positive spillovers that can help spur job creation and business investment over the longer term.

The focus on work that will improve neighborhoods can also lead to the creation of jobs that can prepare workers for higher-skilled jobs, such as construction and skilled trades, education, and health care. For example, subsidized nonprofit and subsequent private sector jobs could entail improvement of schools, parks, or other public infrastructure, or working in community health centers or larger medical enterprises. We know that there are some higher-paying jobs in these sectors, and there is ample anecdotal and survey evidence of strong demand for these middle-skill jobs that do not require a college education, but do require skills that take time to acquire.³⁵ Indeed, a past Hamilton Project proposal (Holzer 2011) called for enhanced workforce development systems to help move workers into these and other middle-skill jobs, relying on both community colleges and private employers to help create career pathways. In neighborhoods where RCJS is implemented, the two ideas can be highly complementary.

Skill Building: The Roles of Nonprofit and For-profit Employers

The requirement that jobs subsidized under RCJS must have the potential to quickly build skills that lead to good jobs in the private sector is intended to better support individual and family economic self-sufficiency. This strategy contrasts with the bias toward the creation of low-wage, higher-turnover jobs in current and past enterprise zone programs.³⁶ For example, federal empowerment zones offered a credit of 20 percent of a worker's wages, up to a maximum \$3,000 (at \$15,000 in wages paid), thereby providing the largest relative subsidy to the lowest-wage and hence lowest-skilled workers. Similarly, California's program paid up to 50 percent of wages up to 150 percent of the minimum wage. In both cases the credit declined over the employee's tenure, potentially leading to excessive job churn. Moreover, the hiring incentives offered under these and other programs are in no way tied to the creation of skills that can lead to higher-paying jobs.

The requirement that RCJS jobs must be administered by local nonprofits, in partnerships with local employer and community groups, is intended to reinforce the revitalization/

improvement goals of the policy. In addition, this requirement should lead to the development of subsidized jobs that are more likely to yield successful private sector job placements using the skills acquired. Local nonprofits, local businesses, and community members are more likely than outsiders to know the unique challenges and needs in the areas in which they work. And local businesses, perhaps in conjunction with community colleges, can help to identify the most promising local jobs for which to train participants. There is also scope to partner with larger nonprofits based elsewhere, which may be able to provide infrastructure and expertise drawn from their prior experiences.

The RCJS proposal also takes account of racial mismatch and local—possibly racially stratified—labor market networks. RCJS focuses on helping disadvantaged residents in targeted areas; it imposes income targeting and residence requirements for eligibility, as well as the explicit goal of improving the targeted areas. Moreover, the involvement of local nonprofits and community organizations should channel efforts in ways that most help local residents. Furthermore, the involvement of local nonprofits might make it more likely that local programs are structured to deliver more benefits to low-income residents of the community rather than landowners and higher-income newcomers.

The suggested rules regarding job retention criteria for private sector employers to remain eligible for partial RCJS subsidies are intended to incentivize the creation of longer-term jobs, and reduce the incentive of employers to churn employees—replacing unsubsidized employees with subsidized employees to extract greater benefits from the subsidy program. The possibility of churning workers to take advantage of hiring credits is a long-standing concern (see, e.g., Bishop and Haveman 1978), and Neumark and Grijalva (2017) find evidence of job churning under countercyclical hiring credits. Similarly, making continued eligibility of nonprofits for RCJS funds dependent on successful placements of workers in private sector jobs is intended to incentivize effective training and good job placements. In addition to providing incentives, these requirements will make RCJS a more attractive program to nonprofit and for-profit employers that can achieve strong labor market outcomes for many participants, and a less attractive program for those that cannot.

Focusing on Concentrated Poverty

The targeting of RCJS subsidies aims to achieve two objectives: supporting low-income families and leading to jobs that pay well above the minimum wage. The requirement that workers eligible for RCJS subsidies initially live in the targeted areas is meant to maximize impact on disadvantaged areas. However, so as not to impede mobility—especially when taking private sector jobs—moving out of the targeted area will not end eligibility for the subsidy. Moreover, employed workers are

eligible, albeit at a higher family income threshold, so that individuals already employed in low-wage jobs can still be eligible for RCJS.

The criterion for eligibility of geographic areas—four to six Census tracts in which, on average, 40 percent or more of individuals are below the poverty line—is intended to achieve two goals. First, it targets RCJS to the neediest areas; second, it focuses RCJS incentives on areas that are relatively compact, but not so small as to limit opportunities for training and job opportunities or to unduly constrain the efforts of local nonprofits.

Finally, the emphasis in much of my discussion on urban areas is not intended to ignore or deny the importance of rural poverty. However, the focus of RCJS on not only creating jobs, but also improving infrastructure and other elements of neighborhoods with concentrated disadvantage, makes it likely that the program will yield the most benefits in urban areas.

WHY A PLACE-BASED POLICY?

Given that past enterprise zone programs have generally been unsuccessful, why propose a new place-based policy? Concentrated poverty and disadvantage remain serious concerns that may be amenable to policy solutions. There are a number of potential rationales for place-based policies, as discussed in Neumark and Simpson (2015) and more recently in Austin, Glaeser, and Summers (forthcoming), with reference to much broader geographic areas of the United States. We do not have solid evidence on all of these, but I believe there is a strong case for continuing to try to develop effective place-based policies, targeting areas of concentrated disadvantage, to use as part of our policy approach to reducing poverty—and urban poverty in particular.

Market imperfections that have been highlighted in the labor economics literature help to justify the kind of antipoverty, place-based policy proposed here.³⁷ One type, discussed earlier, is spatial and racial mismatch. These hypotheses imply that place-based incentives need to focus on jobs for local residents, which is the case with RCJS. A second rationale for place-based policies is positive externalities stemming from network effects, whereby employment of residents can help other residents find jobs. Again, RCJS incorporates this perspective, incentivizing jobs for local residents, which existing research (e.g., Hellerstein, McInerney, and Neumark 2011) suggests is necessary to reach the disadvantaged residents of some areas—especially minorities. Finally, consistent with evidence on the spatial mismatch hypothesis, Bound and Holzer (2000) show that less-skilled workers are less likely than high-skilled workers to move in response to local labor demand shocks. This provides another reason for

policyholders to focus on spurring job creation in areas where low-income workers live.

EVIDENCE ON THE POTENTIAL EFFECTIVENESS OF RCJS

I am not aware of a policy closely similar to RCJS that has been tried in the past. However, there is evidence from research on existing or past programs that provide empirical support for some elements of the proposal, or that can address potential criticisms.

Hiring Credits

Evaluations of the effectiveness of enterprise zones have been disappointing, as are the generally negative findings from past research about the effects of general hiring credits used to boost labor demand, especially hiring credits targeting the disadvantaged.³⁸

The poor track record of these kinds of hiring credits is often attributed to stigmatization of those eligible for the credits. Eligibility of workers for targeted hiring credits can provide information to employers that they have been unsuccessful in the labor market, leading employers to regard eligible workers as risky or as less productive, offsetting the potential impact of the hiring credit (Dickert-Conlin and Holtz-Eakin 2000; Katz 1998). The problem may be particularly severe for narrowly targeted hiring credits (e.g., Burtless 1985).

RCJS has the potential for similar risks, but stigmatization is less likely to be a problem. RCJS does not target workers based on factors that necessarily indicate past employment difficulties, such as long-duration unemployment, welfare receipt, a criminal record, and so on. Rather, its targeting is based on residence in a neighborhood with concentrated disadvantage, as well as low family income. These criteria can be correlated with individual characteristics that might be negative signals to employers. However, potential employers will likely understand that low income, and interest in RCJS subsidies, is in part a reflection of place, not people. That is, an employer looking at a worker's eligibility for RCJS would rationally attribute at least part of the worker's eligibility to factors beyond the worker's control, making it less likely that eligibility would stigmatize the worker.

The argument has some parallels to arguments I made in Neumark (2013) about the likelihood of more-positive effects of hiring credits enacted in response to severe economic downturns. I argued that a hiring credit focused on nonemployment related to the business cycle is less likely to result in eligible workers being stigmatized, because eligibility for such a hiring credit based on current unemployment or labor force nonparticipation might not send employers much of a bad signal. Earlier evidence consistent with this argument comes from Katz's (1998) analysis of the federal New Jobs Tax

Credit (NJTC), which was intended to help spur recovery after the recession in the 1970s. The NJTC was noncategorical rather than targeting specific groups. Katz finds that a "temporary, noncategorical, incremental employment subsidy" (31) such as the NJTC has some potential for creating job growth. Neumark and Grijalva (2017) present more-recent evidence based on state-level hiring credits, many of which were enacted as countercyclical tools during and after the Great Recession. They find that some specific types of hiring credits enacted during the Great Recession succeeded in boosting employment, including credits targeting the unemployed. Heaton (2012) provides additional evidence of positive employment effects for hiring credits adopted during (or just before) the Great Recession, examining the 2007 expansion of the Work Opportunities Tax Credits (WOTC) for veterans entitled to compensation for a service-connected disability.³⁹

Kroft, Lange, and Notowidigdo (2013) provide evidence that is particularly relevant to geographic variation in labor market strength. In a hiring field experiment, they find that, although callback rates are lower for long-term unemployed workers, the stigmatizing effect of a long unemployment spell is less strong when the labor market is weak. Of course, RCJS is not a countercyclical hiring credit. But as with countercyclical hiring credits, the greater role of circumstances as opposed to individual characteristics in determining eligibility for RCJS could reduce stigma and hence boost the benefits of the program.

Subsidized Wages

Wage subsidies have also been the subject of useful recent research.⁴⁰ The American Recovery and Reinvestment Act (ARRA) of 2009 included a \$5 billion Temporary Assistance for Needy Families (TANF) Emergency Fund, under which states could get substantial reimbursement for subsidizing jobs. States were not limited to creating subsidized jobs programs for families receiving TANF, and many chose a broader target population, using a higher income threshold, extending the program to the long-term unemployed, and so on (Farrell et al. 2011).⁴¹

The evidence shows that the program overall resulted in a large number of job placements: there were approximately 260,000 placements of low-income parents and youth in subsidized jobs during 2009 and 2010 (Warland, Young, and Lower-Basch n.d.), half of these representing summer jobs for youths (Farrell et al. 2011).⁴² In addition, evidence from surveys of participating employers points to strong support for these programs (Roder and Elliott 2013), and Lower-Basch (2011) reports that states found more employers willing to hire the target population than they could accommodate. The large level of placements, if nothing else, suggests that RCJS could expect a strong response to its heavily subsidized jobs, in contrast to the experience of low take-up for other hiring

credit programs (Hamersma 2003), often attributed to both administrative costs and stigma.

The stigma associated with these wage subsidies might have been lower because of their adoption following a severe recession, when many people were unemployed because of negative demand shocks (paralleling the argument for hiring credits targeting the unemployed), and because eligibility for Emergency Fund subsidies was broad compared to earlier credits narrowly targeting the disadvantaged. For example, some states set eligibility based on family income at or even above 200 percent of the poverty line (Pavetti, Schott, and Lower-Basch 2011). In addition, subsidies of 100 percent might have allayed employer concerns about worker quality since they could terminate the worker without having incurred any direct wage costs. Also, in some cases the employer of record was a nonprofit intermediary or workforce agency, protecting firms from adverse impacts on their unemployment insurance (UI) tax rating and other legal liability when workers exit (Lower-Basch 2011). This provides further support for the RCJS model of relying on nonprofits in the first phase of job subsidies.

Did these wage subsidies lead to job creation, or did they just create windfalls? One type of evidence, which should be taken with a grain of salt, comes from surveys of employers or program administrators. Pavetti, Schott, and Lower-Basch (2011) report that administrators of subsidized employment programs surveyed by telephone claimed that the subsidies helped some small businesses expand. Roder and Elliott (2013) conducted a telephone survey of employers who took part in job subsidy programs in three states, and report that 63 percent said they created new positions to hire the subsidized workers.

Turning to the question of post-program effects, many descriptions of TANF Emergency Fund job subsidy programs note a high degree of placement in unsubsidized jobs after program completion. Lower-Basch (2011) notes that several states and counties reported “retention rates ranging from 10 to 50 percent” (10), and describes a Boston program in which 46 percent of graduates obtained unsubsidized employment after the program ended. However, this evidence does not compare experiences of participants and nonparticipants, and is unlikely to reveal a causal impact of the program.

More compelling evidence comes from studies of two TANF Emergency Fund programs. A study of the Florida Back to Work Program, using state UI records, finds higher earnings and employment for participants, including the long-term unemployed, than for eligible nonparticipants in the four quarters after the program ended (Roder and Elliott 2013).⁴³ Similar results were obtained for the program in Los Angeles County that included paid work experience that subsidized nonprofit or public sector jobs, as well as an on-the-job training program that subsidized jobs with private

employers who agreed to hire participants after an initial two-month trial period (see Glosser, Barden, and Williams 2016). A randomized evaluation study found that, one year after assignment to the program, employment was substantially higher in the two treated groups than in a control group, and was highest for those in the paid work experience.

Lower-Basch (2011) suggests that these more-positive conclusions compared to the research on effects of past hiring credits targeting the disadvantaged may be attributable to the discretionary nature of the TANF Emergency Fund job subsidy programs, in which administering agencies were “able to select both employers and workers to participate” (2). In contrast, programs such as WOTC were available to any employer who hires from the targeted population and files the required paperwork. Some of these potential advantages of the TANF Emergency Fund job subsidies might also apply to RCJS.⁴⁴

There are a few past programs with some features that are similar to RCJS.⁴⁵ Box 4 describes these programs and appendix table 2 provides a summary.

INCORPORATING AN EVALUATION COMPONENT

It is always useful to evaluate program effects, but even more so in the context of RCJS, given the unimpressive track record of past place-based policies. Thus, funding for RCJS should include support for evaluation, and initial implementation should be limited to what is needed to learn about program effects, with a more scaled-up approach dependent on evaluation outcomes.

To obtain the most rigorous evidence, two levels of randomization are needed. First, given that one of the key goals of RCJS is to impact neighborhoods, there has to be randomization across sites. Local organizations should develop proposals for RCJS funding. Program administrators should select twice as many sites for implementation as can be funded under the allocated budget, and then randomly select half of these sites for implementation. It might also be ideal to do the randomization within states and (if possible) within metropolitan areas, so that a within-state or within-city design can be used to control for other unobservables that could be correlated with selection into the program.

Some neighborhood-level data can be obtained from public sources, such as tract-level poverty rates, although ACS data regarding this and other variables are only publicly available in five-year roll-ups. Ideally, a research partner would be identified that can access confidential ACS data at a Census Research Data Center, enabling that partner to study data at the tract level at an annual frequency. Other tract-level measures, such as crime, business openings, and so on, are also important, and the research partner might need to

develop or use other data sources on some of these (such as the National Establishment Time Series, and crime reports) to fully evaluate the RCJS program.

It is also important to assess the effects of RCJS on individuals, in addition to communities. This level of the analysis requires

randomization of access to subsidized RCJS jobs across individuals and within the selected sites. Local program administrators should select a number of applicants that is double the allocated number of subsidized jobs, and then randomize those selected to treatment and control groups. For analysing labor market outcomes for these two groups,

BOX 4.

Past Programs That Share Features with RCJS

The Neighborhood Jobs Initiative (NJI) was targeted at neighborhoods in four cities from 1998 to 2001. It included three types of employment services: employment-related services such as training and counseling; increasing knowledge about programs that create work incentives, such as the EITC, TANF earnings disregards, and child-care subsidies; and community support for work, such as working with community-based organizations to create employment programs. The NJI was funded by nonprofits, with technical assistance from MDRC and the Urban Institute. Although there was no formal quantitative evaluation of this program, NJI sites set out to bring neighborhood employment up to employment levels of the surrounding area, with a focus on both job quality and retention, typically on a five-year timeline. By the time of the final MDRC report, data show that the Fort Worth and Chicago sites were on track to meet their goals. MDRC found that programs were more appropriate for neighborhoods with a more-stable population—those without too much movement in and out of the community—where residents are in the neighborhood long enough to benefit from the programs (Molina and Howard 2003).

Phase I of the Earn + Learn program ran from 2011 to 2013; it was a subsidized jobs and training program, targeting minority males aged 18–24, formerly incarcerated individuals, and chronically unemployed adults in Detroit, Flint, and Saginaw.⁴⁶ Paralleling RCJS, Earn + Learn had some training and employment opportunities focused on removing urban blight, with about 10 percent of placements in construction but also a good number—25 percent—in manufacturing (see Schultz Patel 2015). One focus of the program, partnering with the Detroit Training Center, trained students in both traditional demolition as well as deconstruction.⁴⁷ Earn + Learn was funded by foundations, and state and local governments. There was some evaluation based on observational data and interviews, with some indication of participants moving into unsubsidized jobs, but no evidence based on comparison groups.⁴⁸

The New York City Parks Opportunity Program (POP), which has run from 1994 to the present, is a transitional jobs program focused on cleaning and maintaining city parks, funded by the city government. Participants receive six-month placements in parks maintenance and operations, where they receive training in basic skills such as forestry, security, or horticulture, as well as training in soft skills and general skills such as computer literacy and English as a second language. The focus on improving urban infrastructure has parallels to RCJS. However, POP has quite different targeting, focusing on welfare recipients who have reached their five-year benefit limit. In addition, training in fields such as forestry or horticulture might be less productive if there are few private sector jobs requiring these skills. The program has placed over 11,000 trainees into full-time positions since 1994, and ratings of park sites are reported to have improved significantly, although the report notes recent increases in crime (see Council of the City of New York 2017).

New Hope for Families and Children was run in two inner-city neighborhoods in Milwaukee. For adult residents of eligible neighborhoods, New Hope offered community service–based full-time jobs at local nonprofits, personalized job search and employment assistance, and monthly earnings supplements, along with subsidized health insurance and child care. Within the targeted neighborhoods, participants had to have household incomes below 150 percent of the poverty line and be willing and able to work at least 30 hours per week. Funding came from foundations, companies, and state and federal sources. Similar to RCJS, local nonprofits played an important role, and some community-service jobs were in construction and property maintenance.

Unlike the other programs discussed in this box, New Hope was evaluated with a rigorous random assignment design. The program shows positive long-term effects on earnings, employment, marriage, mental health, and child achievement and behavior, although there was some fade out. Only about a third of participants in subsidized jobs did not transition to an unsubsidized job (see Center on Poverty and Inequality 2016; Miller et al. 2008).

More recently, Chicago CRED (Creating Real Economic Destiny; 2017) has provided transitional jobs and support services to men in the south and west sides of Chicago who are at high risk of experiencing gun violence. The program uses a street-level recruitment strategy to identify men who are at the highest risk of being shooters or being shot. Chicago CRED provides transitional jobs, training, and support services for participants; after graduating from the program, participants are placed into permanent, full-time jobs with private employers, with whom the program has built relationships. The transitional jobs share the RCJS feature of revitalizing the communities where participants live, and can include interior home demolition, conservation, and city beautification.

state UI records are ideal, so I propose that data cooperation agreements with state agencies be a requirement for program selection.⁴⁹ Absent this requirement, data collection on individuals randomized to treatment and control groups would be much more difficult.⁵⁰

In addition to this experimental analysis, it is possible to gain insights from qualitative research on implementation via interviews with program administrators and other stakeholders, as well as from quantitative research that captures variation in implementation across sites. Such implementation variation is often important in similar programs, and this evidence can help researchers to interpret the experimental results, providing lessons for future implementation should policymakers decide to scale up the program.⁵¹

COST ESTIMATES

A serious implementation and evaluation of the RCJS program could be done at a moderate expense. In this section I provide a rough estimate of costs.

The program is scalable, with the overall cost depending on both the number of implementation sites and the number of participants at each site. I begin with an estimated cost per worker and per site, and then suggest a reasonable scale and the implied overall cost.

Suppose workers are hired at a wage of \$10, which is \$2.75 more than the current federal minimum wage. (Of course, in states or cities with a higher minimum wage, the wage would have to be accordingly higher.) Accounting for other labor-related costs and the likelihood of implementation in some areas with higher minimum wages, assume a cost of \$30,000 per worker per year for labor costs in Phase 1. It is difficult to estimate costs for training and other services, but I assume this is another \$15,000 over the life of the period of full subsidies, implying a labor cost of \$60,000 per worker for the Phase 1 full 18-month subsidy period. The subsidy amount in Phase 2 depends on the average private sector wage paid to participants. Assuming that only the first \$30,000 of wages per year are subsidized, the 50 percent subsidy over an additional 18 months adds \$22,500 in cost per worker, for a total per-worker estimated cost over three years of \$82,500.

The number of sites and the number of jobs per site are the other key factors in calculating the cost. I assume an average of 50 local jobs per site, which seems small enough to be feasible but large enough to have an impact on the local area. Given that recent data show the average number of employed persons per extreme poverty tract is 1,591, these 50 jobs would represent about a 3.1 percent increase in jobs held by residents, although there could be some crowd-out of other employment. With 50 jobs per site, the per-site cost is \$4.125 million. Finally, suppose 100 sites are funded. As shown in table 1, there are

about 4,100 extreme poverty Census tracts in the United States. Thus, 100 sites of around five tracts each would cover about 12 percent of extreme poverty tracts. Furthermore, not all extreme poverty tracts would qualify for RCJS, whether due to absence of participating nonprofits or other considerations. Thus, this experimental phase would cover a sizable share of potentially eligible tracts, while keeping costs at a reasonable level until the evaluation can provide policymakers with more information on effectiveness. This evaluation design would lead to 100 treatment and 100 control areas for the site-level analysis, and approximately 5,000 treated individuals and 5,000 control individuals in the person-level analysis. The total program cost would be \$412.5 million.

Finally, as a rough estimate, a serious evaluation of the program could cost about \$2.5 million, bringing the total cost to \$415 million.

This may seem like a large cost for a program that, at the upper limit, would be expected to create 5,000 jobs—though these jobs would last for three years under the two phases of subsidies and hopefully longer, given the design. However, the experiences of other programs suggest that this cost is not inordinately high. The federal Empowerment Zone Program studied by Busso, Gregory, and Kline (2013) cost approximately \$641 million. Their program estimates (table 10 of their paper) imply 6,928 net jobs created in the treated zones, which is comparable to the calculations in this chapter.⁵²

It is also important to note that the per-job cost is not out of line with other costs associated with hiring credit programs focused on the unemployed. Neumark (2013) reviews a number of other studies and suggests that costs per job created under such programs, for what are generally much shorter durations, range from \$9,100 to \$75,000. Note that the actual credit available for such programs is typically much less. However, windfalls to employers for hiring that would have occurred absent the credit tend to drive up the cost substantially (Neumark and Grijalva 2017), whereas RCJS seems less likely to produce employer windfalls.

On the benefit side, recall that RCJS aims to go beyond simply adding jobs, and will produce a meaningful impact on the community. That is, in the first 18 months the newly employed workers would be engaging in work to improve the community in a number of ways. So, the money is also purchasing improved public goods for the selected sites.

Of course, policymakers could well decide to reduce the wage subsidies—say to 50 percent during Phase 1 and 25 percent during Phase 2—which would cut the wage-subsidy costs of the program in half. This would probably reduce take-up; it is not clear how the jobs would otherwise be financed, particularly during Phase 1. To resolve some of this uncertainty, the evaluation could incorporate different subsidy levels to help

gauge how impacts fall off with the subsidy level and whether lower subsidies have as high a benefit-to-cost ratio.

Note that these cost figures can be used to approximate the cost of extending the RCJS program to encompass all eligible locations. At the upper limit, retaining a figure of 50 jobs per site, the cost would be roughly 8.3 times the previous estimate of \$412.5 million, or \$3.4 billion. Of course, spending is unlikely to reach this high, because not all extreme poverty tracts would meet eligibility criteria, including the presence of nonprofits in a position to effectively use RCJS incentives.

The ongoing costs of RCJS could be incurred annually—if a new cohort of workers were started in each year—or once every three years, if one cohort is funded at a time. Either way, RCJS holds the promise of delivering economic benefits to the nation’s neighborhoods that have the highest concentrations of poor residents. It does so at a cost that is small relative to other social assistance programs, and that has the potential benefit of leading to longer-term gains in earnings by building skills and improving disadvantaged areas, rather than simply providing safety net support.

Questions and Concerns

1. Will RCJS confer benefits on its intended beneficiaries—residents of selected neighborhoods?

It is true that even if we just focus on redistribution, which should be easier to accomplish than net increases in jobs, urban economics highlights the potential complexities arising from mobility of people and capital. As discussed in Moretti (2010), a place-based job subsidy will result in higher wages unless labor supply is infinitely elastic. If labor is mobile, some workers will move to the subsidized area, and as long as housing supply is not infinitely elastic, housing prices and rents will increase, offsetting at least some of the gains to the original residents. 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, the only effect of the policy is to increase land prices. This is a concern given that landowners are not the target population for place-based policies.

However, other than unlikely knife-edge cases—such as infinitely elastic labor supply that implies no wage increases, or perfect mobility that undoes all gains from place-based policies—mobility probably will only partly undermine the effects of redistributive place-based policies, and, conversely, these policies will provide some benefits to the disadvantaged residents of the targeted areas.⁵³ The potential mobility and land-price effects, as noted earlier, underlie Crane and Manville’s (2008) idea of trying to create institutional arrangements that make it more likely that the intended beneficiaries benefit. As noted above, this idea is built into the RCJS proposal.

2. Is it necessary to offer 100 percent job subsidies in the first phase?

One can clearly question whether it is necessary to offer 100 percent job subsidies in the first phase, rather than some smaller subsidy. I embrace this dimension of the proposal for two reasons. First, RCJS is intended to have a strong effect on employment, and to create spillovers that could help improve disadvantaged areas. For that reason, trying to induce as much take-up as possible—which includes local nonprofits and related organizations creating ways to use workers under RCJS—is inherently valuable.

Second, this element of RCJS dovetails at least partially with calls for guaranteed jobs in the United States, as a response to low wages and low employment among the least-skilled workers. This is an idea that has been embraced by prominent Democrats, including Cory Booker, Kirsten Gillibrand, and Bernie Sanders—all three of whom might run for president in 2020.⁵⁴ Two of the key rationales for guaranteed jobs programs are, first, to create jobs, and, second, to provide a wage floor with which other employers will have to compete. This is not the place to delve into a full discussion of the merits of a guaranteed jobs program, but there are clear limitations of a national proposal: the prohibitive cost, the potential creation of jobs that do nothing to build skills and prepare people for private sector jobs, and the open-ended nature of the commitment of the government to paying or subsidizing wages.⁵⁵ Although some might argue that these criticisms do not undermine the case for a guaranteed jobs program,⁵⁶ it simply seems infeasible that proposals with such pitfalls will attract sufficient political support. In contrast, RCJS has elements of a guaranteed jobs program that could make it more palatable to policymakers who are likely to be more skeptical of government job creation efforts: it targets a limited number of disadvantaged areas, based in part on a competitive process that chooses promising deployment of RCJS support; it seeks to improve these areas, and it aims at transitions of participants into higher-wage, private sector employment.⁵⁷

3. Will hiring credits create windfalls for employers?

Another potential problem with hiring credits is that they could create windfalls for employers, leading to credit payments for jobs that would have been created regardless, whereas an effective program should provide incentives for employers to create jobs they would not otherwise have created. Such problems may be particularly urgent for low-skilled or disadvantaged workers, who have high turnover. However, this seems less likely to be a concern in the kinds of areas the RCJS targets, which tend to have low labor demand. Moreover, the particular structure of RCJS—using nonprofits to engage workers in jobs that improve areas of extreme and concentrated poverty—makes it even more likely that the program would create jobs that would not otherwise have been created.

Conclusion

The RCJS proposal is intended as a proactive policy to address poverty in areas of extreme poverty. RCJS retains the geographic targeting of prior place-based policies such as enterprise zones, but with a very different structure, and different incentives, that are intended to increase positive impacts on residents of the targeted areas. Specifically,

RCJS emphasizes building skills that can lead to higher-paying private sector jobs, and improving the disadvantaged areas to which program benefits are targeted. I believe that RCJS offers the potential for substantial improvements in economic conditions in areas where our nation's poorest residents live.

Appendix

APPENDIX TABLE 1.

Summary of U.S. Evidence on Enterprise Zones

Study	Program	Results
Neumark and Kolko (2010)	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 (\approx -8 to +6 percent); no evidence of spillovers.
Kolko and Neumark (2010)	California enterprise zones	Zones more involved with marketing and outreach exhibited positive employment effects; zones focused on tax credits exhibited negative effects.
Elvery (2009)	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)	Texas enterprise zones	<p>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).</p> <p>Negative and insignificant effects on share black and with income below the poverty line.</p> <p>Significant negative effect on vacancy rate (-4 percent).</p> <p>Significant positive effect on median home value (10.7 percent).</p>
Ham et al. (2011)	State enterprise zones, federal empowerment zones, federal enterprise communities	<p>State programs, significant positive impacts on: unemployment rate (-1.6 percentage points); poverty rate (-6.1 percentage points); average wage and salary income (\approx1.6 percent); employment (\approx3.7 percent).^a</p> <p>Empowerment zones, significant positive impacts on: unemployment rate (-8.7 percentage points); poverty rate (-8.8 percentage points); average wage and salary income (\approx20.6 percent); employment (\approx34.2 percent).</p> <p>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 (\approx12.7 percent); employment (\approx10.7 percent).</p> <p>Positive but insignificant spillovers on neighboring Census tracts.</p>
Neumark and Young (2017)	State enterprise zones, federal empowerment zones, federal enterprise communities	<p>Large poverty reductions from state programs reported in Ham et al. (2011) result from data error.</p> <p>Strong positive effects of federal empowerment zones reported in Ham et al. (2011) overstated because of selection into zones, and beneficial effects of enterprise communities likely spurious.</p>

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

Summary of U.S. Evidence on Enterprise Zones

Study	Program	Results
Busso, Gregory, and Kline (2013)	Federal empowerment zones	<p>Positive and significant effects on job growth in Longitudinal Business Database (12–21 percent), likely concentrated among births, and existing establishments with > 5 employees.</p> <p>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).</p> <p>Positive generally significant weekly wage effects on zone residents employed in zone (8–13 percent); magnitudes smaller for zone residents generally (3–5 percent and usually insignificant) and nonresidents working in zone (\approx0 percent).</p> <p>No effects on rents, population, or vacancy rates, large significant positive effects on house values (28–37 percent).</p>
Hanson (2009)	Federal empowerment zones	<p>OLS estimates: positive significant effect on employment rate (2 percentage points); negative significant effect on poverty rate (–2 percentage points).</p> <p>IV estimates: No effect on employment rate (0 percentage points); insignificant positive effect on poverty rate (2 percentage points).</p>
Hanson and Rohlin (2013)	Federal empowerment zones	<p>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.</p> <p>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.</p>
Reynolds and Rohlin (2015)	Federal empowerment zones	<p>Positive significant effects on mean household income (11 percent), but not on median household income (one-tenth as large).</p> <p>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.</p> <p>Significant increase in share of households with income < \$10,000 and above \$100,000.</p> <p>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.</p>

Note: Most of this table comes from Neumark and Simpson (2015), although it has been updated to include more-recent studies.

APPENDIX TABLE 2.

Summary of Programs Sharing Features with RCJS

Program name Locations (Dates)	Neighborhood Jobs Initiative Neighborhoods in Chicago, IL; Hartford, CT; Fort Worth, TX; New York, NY; and Washington, DC (1998–2001)	Earn + Learn, Phase 1 Detroit Area, MI (2011–13)
Description	<p>The Neighborhood Jobs Initiative (NJI) targeted employment services at an entire neighborhood, rather than at individuals. It included three components:</p> <ul style="list-style-type: none"> • Employment-related services and activities, such as job development, training, and counseling; • Financial incentives to work, including increasing participants' use of the EITC, earnings disregards for TANF recipients, child-care subsidies, Medicaid, food stamps (SNAP), and wage subsidies; and • Community support for work, including increasing the quality and quantity of residents' social networks to facilitate the sharing of information. NJI worked with community-based organizations at each site to deliver employment programs. 	<p>Earn + Learn was a subsidized jobs and training program. Multiple partners worked together to train, place, and assist participants in maintaining employment. The program provided work-readiness training, individualized support services, employment, basic adult education, and vocational training to participants.</p>
Population served	<p>NJI served residents of targeted neighborhoods (15,000 to 17,000 people). The goal was to raise employment levels in the targeted neighborhoods to that of the surrounding area, so the participants were likely unemployed persons living in the neighborhoods.</p>	<p>The program targeted disconnected, at-risk youth (ages 18–24), young minority males including prisoners reentering communities, and chronically unemployed adults in Detroit, Flint, and Saginaw.</p>
Funding mechanism	<p>Funded by nonprofits: each site received funding as well as intensive technical assistance from MDRC, the Urban Institute, and MDRC's consultants to develop and implement employment strategies.</p>	<p>Funded by foundations and state and local funding.</p>
Employer partners	<p>Not applicable</p>	<p>Local for-profit (48 percent), nonprofit (44 percent), and government and public sector (7 percent) partners.</p>
Types of jobs	<p>Not applicable</p>	<p>Employer partners were clustered mostly in the manufacturing, retail, and health-care and social assistance industries. Jobs included deconstruction and blight removal.</p>
Evaluation	<p>While there was no formal quantitative evaluation of the overall NJI initiative, sites aimed to bring neighborhood employment in line with employment levels of surrounding area. At measurement of final report, data show that the Fort Worth and Chicago sites were on track to meet their goals.</p> <p>Implementors found that programs were more appropriate for neighborhoods with a more-stable population, with less movement in and out (Molina and Howard 2003).</p>	<p>There were both nonexperimental evaluations based on observational data and participant/employer interviews, as well as quasi-experimental evaluations based on comparisons to individuals receiving standard Workforce Investment Act of 1998 (WIA) programming in the same counties (Schultz Patel 2015).</p>
Effects	<p>Not applicable</p>	<ul style="list-style-type: none"> • 65 percent of all participants received work readiness training, with a 93 percent completion rate. • 32 percent of all participants received occupational training, with an 83 percent completion rate. • 69 percent of participants were matched with subsidized transitional jobs that lasted on average more than nine weeks. • 77 percent of participants moved into unsubsidized employment. Among participants who completed work readiness training, occupational training and subsidized transitional employment were the most successful, with 83 percent transitioning into unsubsidized employment. • Relative to the WIA comparison group, earnings rose less in treatment group. Employment rose more in treatment group, but level was lower up to eight quarters post treatment.

APPENDIX TABLE 2. (CONTINUED)

Summary of Programs Sharing Features with RCJS

Program name Locations (Dates)	New York City Parks Opportunity Program New York, NY (1994–Present)	New Hope for Families and Children Milwaukee, WI; Two neighborhoods (1994-1998)
Description	<p>The New York City Parks Opportunity Program (POP) is a transitional jobs program that hires applicants referred by the Human Resources Administration/Department of Social Services to clean and green parks, playgrounds, and other facilities citywide.</p> <p>Participants are placed in city jobs, primarily in parks maintenance and operations, for six months, during which they receive training in basic skills in forestry, security, or horticulture through the Job Training Participants program. Training in soft skills such as resume writing and interview skills are also provided.</p> <p>Participants in POP have played an increasingly large role in maintaining the city’s 28,000 acres of parkland as the Parks and Recreation department full-time staff has declined—from 5,400 in 1980 to just under 1,800 in 2017 (New York City Council Finance Division 2017; Schwartz 2004).</p>	<p>The New Hope Project operated from 1994 to 1998 in two inner-city areas of Milwaukee, Wisconsin, offering:</p> <ul style="list-style-type: none"> • Community service–based full-time job opportunities (CSJs) at local nonprofit organizations for participants unable to find full-time work (or part-time job opportunities to supplement an existing part-time job) in the private job market. Participants could use this resource two times for a total of 12 months of employment within the three-year program period. About one third of New Hope participants used a CSJ at some point during the three-year period; • Personalized services assisting participants in job searches, child care, and other employment-related needs; and • For those working full time (30+ hrs/week), the program offered monthly earnings supplements to raise income above the poverty threshold, subsidized health insurance, and subsidized child care.
Population served	<p>To participate in the POP program and get assistance, participants must be on public assistance and must be referred to the POP program by the Human Resources Administration. The POP program primarily targets welfare participants, especially those that have reached their five-year benefit limit.</p>	<p>Individuals were eligible for New Hope if they lived in one of the targeted neighborhoods, were age 18 or older, had earnings of less than 150 percent of the federal poverty level, and were willing and able to work full time.</p> <p>The two targeted neighborhoods were selected to provide racial and ethnic diversity and to concentrate the program on inner-city, high-poverty areas.</p>
Funding mechanism	<p>Funded by the New York City government.</p>	<p>Funded by a large consortium of local, state, and national organizations including State of Wisconsin Departments of Workforce Development and Health and Human Services, the U.S. Department of Health and Human Services, the National Institute for Child Health and Human Development, and many foundations and companies.</p>
Employer partners	<p>New York City Department of Parks and Recreation</p>	<p>Local nonprofit organizations</p>
Types of jobs	<p>Park maintenance</p>	<p>About two thirds of the CSJs were either office support and data entry, or construction and property maintenance.</p>
Evaluation	<p>No known formal evaluations.</p>	<p>Evaluation based on random assignment: half of the individuals who applied for the program were randomly selected to participate in New Hope for three years while the other half formed a comparison group that was ineligible for New Hope benefits (Miller et al. 2008).</p>
Effects	<p>Approximately 2,500 public assistance recipients are hired as seasonal workers and perform a wide variety of functions including maintenance, security, customer service, and clerical duties. Since its inception in 1994, POP’s six-month training program has placed more than 11,000 trainees into full-time positions.</p> <p>The percentage of park sites rated acceptable for cleanliness has increased from 73 percent in fiscal year 1993 to 92 percent in fiscal year 2015 (New York City Council Finance Division 2017).</p>	<p>About a third of New Hope participants used a CSJ at some point during the three-year period. Nearly 40 percent of CSJ users transitioned to unsubsidized jobs, while another 25 percent used a CSJ as a filler between two stints of unsubsidized work. However, a third of CSJ users did not make the transition to unsubsidized work.</p> <p>New Hope increased employment, earnings, and incomes, including the program’s earnings supplement and the EITC. Most of the effects were concentrated in the three years of the program, but participants with moderate barriers to employment saw improved outcomes in a five-year follow-up. Some participants saw increased psychological well-being.</p>

Summary of Programs Sharing Features with RCJS

Program name Locations (Dates)	Los Angeles County TANF Emergency Fund Subsidized Employment LA County, CA (2010–11)	Chicago CRED (Creating Real Economic Destiny) Serves participants in Roseland, North Lawndale, West Garfield Park, and Englewood neighborhoods of Chicago, IL (2016–present)
Description	The program offered two distinct approaches to subsidized employment. The first, Paid Work Experience (PWE), subsidized the wages of individuals placed at nonprofit or public sector employers. The second, On-the-Job Training (OJT), offered wage subsidies to for-profit, private sector employers who agreed to place employees onto their payrolls after an initial two-month tryout period; if they did, the wage subsidies could continue up to an additional four months.	Chicago CRED offers supervised transitional jobs, training, and support services to men in the south and west sides of Chicago who are at the highest risk of gun violence. Over a several months period, participants receive: <ul style="list-style-type: none"> • Supervised transitional jobs that pay minimum wage, with opportunity to earn more based on performance; • Training in soft and hard skills; • Intensive life coaching; and • Other support services including cognitive behavioral therapy, trauma counselling, tutoring to get a GED or high school diploma, substance abuse counselling, legal counselling, and help securing stable housing. Graduates of the program are placed into permanent, full-time jobs with private employers, with whom the program has built relationships. All permanent jobs pay \$12 to \$20 per hour.
Population served	Benefits were provided to: <ul style="list-style-type: none"> • Parents employed by a business facing closure or significant lay-offs; • Parents with children receiving CalWORKs cash assistance (i.e., TANF); • Noncustodial parents who were receiving county-funded general assistance; • Parents struggling in vulnerable families receiving child welfare services, including family preservation services; and • Parents living in a domestic violence shelter or homeless shelter (Los Angeles [LA] County Department of Public Social Services 2011).	Targets men in Chicago that are at the highest risk of being shooters or being shot. The program uses street-level recruitment efforts and places men into cohorts of about 30 participants.
Funding mechanism	Funded by federal TANF Emergency Funds.	Funded by Emerson Collective.
Employer partners	Local nonprofits; public sector employers; and private sector, for profit companies	Graduates work in local private sector jobs. Transitional jobs are provided by the program.
Types of jobs	The jobs were mostly administrative. However, at the Department of Beaches and Harbors participants were responsible for helping keep more than 50 restrooms clean, removing debris and trash from beaches and dozens of parking lots, and performing landscaping.	Transitional jobs involve work performing restoration projects in the participants' neighborhood. This can include areas such as interior home demolition, conservation, and city beautification that provide the real-life experience of private sector employment (punctuality, hard work, appropriate work behavior, etc.).
Evaluation	Evaluation was based on a randomized controlled trial design in which individuals eligible for and interested in the subsidized jobs program were randomly assigned to PWE, to OJT, or to a control group that did not have access to either of these subsidized employment approaches, though they received other types of welfare-to-work services (Glosser, Barden, and Williams 2016).	No known formal evaluation.
Effects	Employment after first year of random assignment was highest in the PWE group (92 percent), then OJT (76 percent), and finally the control group (58 percent). Earnings were higher for PWE and OJT than for the control group. These differences reflect participation in subsidized employment. By the beginning of the second year following random assignment, the PWE and OJT groups were still significantly more likely than the control group to be employed, but the differences between the two treatment groups were much smaller. PWE had better placement rates and jobs lasted longer than OJT. Few other differences between groups measured in self-reported financial well-being, or TANF receipt.	The program currently serves about 100 men in Chicago neighborhoods. In October 2017 the program helped 15 participants obtain a high school degree (Chicago CRED 2017).

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Neumark has made significant research contributions in numerous areas of labor economics that intersect with important public policy issues. These areas include labor market discrimination, the economics of the minimum wage, and the economics of aging and age discrimination, among others.

Neumark's current research agenda, as part of his activities as Director of ESSPRI, concern the long-run effects of alternative anti-poverty policies on earnings, income, poverty, and public assistance receipt (broadly defined, "economic self-sufficiency"). A good deal of this work focuses on the effectiveness of policies directed at disadvantaged neighborhoods, such as enterprise zones and other types of tax credit programs.

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The views expressed are my own, and do not necessarily reflect the views of The Hamilton Project or the Brookings Institution.

Endnotes

1. Based on the U.S. Department of Agriculture Economic Research Service's Rural–Urban Commuting Area Codes (grouped into four categories in which core, high-commuting, and low-commuting areas for metropolitan, micropolitan, and small-town areas are collapsed together), poverty is actually the highest in micropolitan areas (18.3 percent) and small-town areas (18.2 percent).
2. Micropolitan areas are defined as those areas with primary cities of between 10,000 and 50,000 residents.
3. Gobillon, Selod, and Zenou (2007) review theoretical models and hypotheses regarding spatial mismatch.
4. Hellerstein, McInerney, and Neumark (2010) find similar evidence for Hispanic employment.
5. Much of the discussion in this section draws from Neumark and Simpson (2015).
6. The potential for network effects to enhance the effects of job creation policies in poor areas might counter some of the criticisms of place-based policies, such as the argument 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). On the other hand, network effects could diminish the effects of some kinds of place-based policies. For example, a policy that leads employers to relocate to an area could do little to boost employment opportunities of local residents if the employees of the relocating companies are not networked to local residents.
7. Indeed, labor market networks that are stratified by race or ethnicity could help explain the racial mismatch evidence presented in Hellerstein, McInerney, and Neumark (2010) and Hellerstein, Neumark, and McInerney (2008).
8. Peer or neighborhood effects can also imply externalities between individuals (see Topa and Zenou 2015). For example, the presence of nonemployed residents might lead other residents to remain nonemployed by changing norms of behavior (Wilson 1987); conversely, creating some employment can have virtuous effects on others.
9. Ladd (1994) introduces the clarifying label of place-based people strategies to refer to policies that are geographically targeted, but with the intent and structure of helping disadvantaged residents in the targeted areas. RCJS is probably best viewed as belonging to this category since it targets lower-income residents of low-income areas.
10. For summaries of the Gautreaux and MTO programs, and reviews of findings, see, e.g., Chetty, Hendren, and Katz (2016), Duncan and Zuberi (2006), Ludwig et al. (2013), and Rosenbaum and Zuberi (2010).
11. Blumenberg (2004) discusses the difficulty of urban-to-suburban (reverse) commuting.
12. As an example, California's enterprise zones (discontinued in 2013) were intended to be areas with job-creation potential that were near (or overlapping with) federally designated targeted employment areas (TEAs); TEAs are Census tracts where more than half the population earned less than 80 percent of median area income. The most significant benefit provided within California enterprise zones was a hiring credit to businesses. Potentially undermining, in part, the distributional goals of the programs, a worker living in a TEA qualified for the hiring credit regardless of their characteristics.
13. Spending on the federal enterprise zone program through 2000 totaled nearly \$400 million in block grants and \$200 million in employment credits, with federal expenditures for the first six years of the program estimated at about \$850 per zone resident. Enterprise communities had grants of just under \$3 million, versus \$100 million (\$40 million) for the initial urban (rural) empowerment zones (U.S. Government Accountability Office 2006), and no dedicated hiring credits. In 2000 an additional program (renewal communities), with related but different criteria, was established, offering a hiring credit and other benefits. See Community Renewal Tax Relief Act of 2000.
14. Holzer, Quigley, and Raphael (2003) find mixed evidence on hiring of minorities from an expansion of mass transit in the San Francisco Bay Area to more-outlying areas (making reverse commuting easier).
15. Bartik (2004) notes that earlier related programs focusing on distressed communities include urban renewal in the 1940s and 1950s, model cities during the War on Poverty, and community block development grants.
16. Kline and Moretti (2014b) provide a largely theoretical discussion of the welfare economics of local economic development programs. Evidence from other countries is discussed in Neumark and Simpson (2015).
17. At the same time, Crane and Manville (2008) suggest that it may be possible 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 specifying, for example, that developers who capture the higher land values devote resources to higher wages, affordable housing, social services, etc.
18. That said, the relationship between empirical findings and welfare implications is complex. Busso, Gregory, and Kline (2013) point out that, in a standard model, a larger employment response can imply greater deadweight loss from distortions in behavior, whereas when labor is immobile—and hence there is less scope for employment increases in targeted areas—the welfare gains are more likely to accrue to residents (workers, specifically), rather than property owners. Alternatively, as Kline and Moretti (2014b) point out, when there are labor market frictions that generate spatial heterogeneity in unemployment, place-based policies such as hiring subsidies in certain locations can increase employment (lower unemployment) in the targeted area and increase welfare, in which case the focus on job creation might be better aligned with effects on welfare.
19. One approach is to identify control areas that are similar to the enterprise zones but where enterprise zone policies did not apply, matching treated and control areas based on similarity of residential and employment characteristics (e.g., Elvery 2009; O'Keefe 2004). Alternatively, control areas can be chosen based on geographic proximity—on the assumption that economic conditions and other relevant policies are very similar in nearby areas. For example, Billings (2009) uses a spatial discontinuity model, looking at employment growth in Colorado's enterprise zones within a quarter mile of the zone boundary and using the area outside the zones within a quarter mile of the zone boundary as the control group. And Neumark and Kolko (2010) use detailed geographic information system (GIS) maps of California's enterprise zones to pick out very narrow control rings (1,000 feet wide) around the zones. An alternative approach 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; these control areas are likely to be more similar on the unmeasured variables associated with enterprise zone designation (Busso, Gregory, and Kline 2013; Neumark and Kolko 2010). Yet another approach is to deal more explicitly with the endogenous selection of areas for zone designation. For example, Hanson (2009) compares employment outcomes in federal empowerment zones with unsuccessful applicant areas. But he also instruments for zone applicant success based on the political influence of the zone's congressional representative.

20. As an example, 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).
21. One way to garner evidence on spillover effects is to posit differences across control areas in the likelihood of these effects arising. For example, positive spillovers are probably 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. These approaches are probably less useful in ruling out negative spillovers, since such spillovers may also come from farther away, with employers making longer-distance moves to take advantage of zone benefits.
22. Evidence of effects of enterprise zones on commercial property values might be more compelling. Burnes (2012) provides evidence of capitalization of enterprise zone benefits in California into commercial real estate prices
23. The cross-state variation in estimated effects is also hard to interpret. 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. Ham et al. (2011) do estimate a large employment effect for Florida (not statistically significant), and Florida has a large hiring credit, yet Elvery's (2009) estimates for Florida for the previous decade are consistently negative.
24. Busso, Gregory, and Kline (2013) 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.
25. The large block grants were for purposes such as business assistance, infrastructure investment, and training programs. As examples, according to Rich and Stoker (2007) some of the top priorities across the empowerment zones were business development (Atlanta); workforce development (Baltimore, including career and family support centers and customized training to recruit and train zone residents for specific jobs); human services (Chicago, including a health and wellness center); human services (Detroit, including an innovation fund to support community programs to stabilize families); business development (New York, including a loan fund targeting small businesses, and large grants for a General Motors Auto Center and the Harlem USA Retail and Entertainment Complex); and business development (Philadelphia).
26. One could imagine a place-based policy proposal somewhat different from mine—one that tries to leverage what may have been the positive effects of the large block grants in the Empowerment Zone Program. But, at this point, my view is that it is difficult to determine what kinds of block grants worked and why. (Rich and Stoker 2010 suggest that block grants might have been the most promising feature of the empowerment zones, but also emphasize that other features, such as local governance structures, likely impacted whether these block grants were effective.) Moreover, the distributional effects of empowerment zones may not have been beneficial.
27. Hanson (2009) instruments for zone designation using representation of the areas encompassing the proposed zones on the powerful U.S. House Committee on Ways and Means, which he posits will affect zone selection but not be correlated with unobserved economic conditions (for which he presents some evidence). 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 instrumental variable (IV) estimates indicate no effect on employment and a positive but insignificant effect on poverty.
28. When the authors 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 of less than \$10,000. They also present evidence of increases in the share of people with higher education (i.e., some college or more), consistent, perhaps, with inflows of higher-skilled people into the areas designated as empowerment zones. 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 (though not quite statistically significant) that the increase in the share of households with less than \$10,000 in income occurs in the higher-poverty tracts. The authors' conclusions differ from those of Freedman (2013), who suggests, "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" (340). However, this is not based on as comprehensive a distributional analysis as in Reynolds and Rohlin (2015), but rather seems to derive from evidence of the positive effects discussed earlier, coupled with no effect on median income in the ACS data.
29. Appendix table 1 does not provide a comprehensive review of all research on U.S. enterprise zones, which is burgeoning. It covers what I view as the main studies that use compelling research designs or are cited frequently. There are some other recent studies not included in appendix table 1: Zhang (2015) studies the effects of enterprise zones in one city (Louisville), and finds positive effects on manufacturing and services employment, albeit with a questionable IV strategy based on only preintervention neighborhood characteristics. Smith (2015) studies federal empowerment zones and renewal communities created in the 2000s, in California and Tennessee, using a propensity score-matching estimator and data from the National Establishment Time Series. There were two different treatments—hiring credits and economic development grants—available in different periods in the empowerment zones, but only hiring credits in the renewal communities. Across the four empowerment zones and five renewal communities he studied (separately), his results sometimes point to a positive aggregate impact of the hiring credit on the level of jobs, but generally not on the trend. For empowerment zones, he does not find an effect of grants on the level or trend.
30. NMTC funds are channeled through community development entities (CDEs), often banks or financial institutions, that have to meet several criteria, including serving or providing capital to low-income communities and people. The tax credits flow to investors that make equity investments in the CDEs.
31. Concluding that the program reduced poverty is also problematic because there is no statistical evidence of employment effects from the Longitudinal Employer-Household Dynamics data (from Census). The point estimates are positive but have standard errors three times as large, and compositional shifts could also lead to higher employment.
32. ACS data from 2010 indicate that 50.4 percent of black residents, 44.1 percent of Hispanic residents, but only 20.3 percent of white residents live in areas where the poverty rate is 20 percent or higher (see Bishaw 2014 for more descriptive evidence). At the same time, poverty rate differences between these groups are much smaller (see Macartney, Bishaw, and Fontenot 2013). Thus, a far greater share of nonpoor black residents live in high-poverty areas than do nonpoor white residents.
33. See the summary of the evidence in Erickson et al. (2008).
34. There might be an allowance made for a lower retention rate if a recession hits in the intervening period.
35. See, e.g., *The Wall Street Journal* (2017), National Association of Home Builders (2017), and Lagasse (2018). See Accenture, BurningGlass, and Harvard Business School (2014) and Chanmugam, Smith, and Worrell (2014) for survey evidence.
36. There is some evidence consistent with a bias toward lower-paying jobs, although the evidence is mixed. Freedman (2013) reports employment effects are concentrated in jobs paying less than \$40,000 and distributed among manufacturing and construction as well as wholesale and retail trade. Billings (2009) finds effects in construction, manufacturing, wholesale trade, and services. Hanson and Rohlin (2011), studying effects on new establishments, find that the retail and service sectors benefited the most from empowerment zone hiring credits. I am not aware of evidence regarding tenure of jobs created because of hiring credits.
37. There is also an equity motivation for place-based policies to try to redistribute jobs and income to places where jobs are scarce and incomes are low—and ideally, of course, to create more jobs and raise income in the aggregate. Austin, Glaeser, and Summers (forthcoming) invoke this spatial equity argument to argue for targeted employment credits in broad areas of the country where joblessness is high. They also suggest that this argument

- is reinforced by the potentially higher marginal returns to reducing economic disadvantage in areas of concentrated disadvantage, referring to high distress areas. As they note, they find some evidence for the “perfectly unsurprising view that you can reduce non-employment more in places where non-employment is currently high” (4).
38. Much of this discussion of hiring credits comes from Neumark (2013). The general negative assessment of hiring credits is echoed in standard labor economics textbooks (e.g., Borjas 2010; Ehrenberg and Smith 2009).
 39. Part of the reason for more-positive conclusions than for hiring credits with narrowly targeted hiring incentives might be related to an absence of stigma, and perhaps even positive attributions, for veterans.
 40. Much of this discussion draws on Neumark (2016a).
 41. States could be reimbursed for increased welfare-related spending in one of three areas, up to 80 percent of a cap for each state; one of the areas of spending was subsidized jobs. Lower-Basch (2011) reports that spending on wage subsidy programs under the TANF Emergency Fund totaled \$1.32 billion. There was some additional funding (an extra \$1 billion under ARRA) via Community Services Block Grants that could be used for these programs.
 42. Recent research has tried to provide evidence on the effects of the programs, although much of it faces challenges in drawing causal inferences. It seems most natural to evaluate subsidized jobs programs, such as training programs, based on post-participation effects on employment and earnings. However, Pavetti, Schott, and Lower-Basch (2011) argue that countercyclical programs intended to keep people working during a downturn should be evaluated based on the number of unemployed people placed in jobs, regardless of how long-term the effects are because, for example, these jobs might be viewed as a substitute for going on unemployment insurance (UI). In the context of RCJS, one might substitute other kinds of public assistance programs for UI as the alternative to a paying job.
 43. They also report consistent evidence from an employer survey, in which 76 percent indicated that they retained at least one subsidized worker after the subsidy period ended, and overall that 37 percent of workers were retained.
 44. There is a longer-standing history of transitional jobs programs in the United States; see Bloom (2010). These programs are somewhat different because they focus explicitly on the hard to employ (e.g., welfare recipients or the previously incarcerated). Two recent evaluations described in Bloom do not find long-term effects on employment or earnings. In my view, the hard-to-employ focus of these programs makes the findings less applicable to RCJS, but I do include a large-scale evaluation as part of my proposal, recognizing that it is an open empirical question whether RCJS will work, and that evaluation can also help refine the program to strengthen the features that deliver benefits.
 45. There is, of course, a vast literature on jobs programs and training programs, which I do not review here. My focus is on programs with features shared with RCJS (such as a neighborhood focus, working with community-based organizations, or an urban improvement goal). Even so, I do not claim to have assembled an exhaustive list of programs that meet this criterion.
 46. Some of this information is based on personal communication with Mac Elabad, senior manager, Workforce Federal Programs at Southwest Solutions (June 2018).
 47. This prepared the workforce for the increased demand for blight removal in Detroit; in the wake of Detroit’s depopulation, nearly 85,000 blighted structures and vacant lots were identified for removal or further evaluation. See Detroit Training Center (n.d.) and Blight Removal Task Force (n.d.).
 48. Apparently Phases II and III of the program dropped employment subsidies, because employers were ending jobs when the subsidies stopped. This kind of behavior may help rationalize the provision of RCJS that ties subsidies to retention.
 49. For examples of this kind of data used in research, see Dague, DeLeire, and Leininger (2017) and Mendenhall, DeLuca, and Duncan (2006).
 50. It would also be possible to use IRS data, as in Gelber, Isen, and Kessler (2016), although these data are annual, not quarterly.
 51. There are numerous examples from the Jobs Plus Program, including Kato et al. (2003) and Riccio (1999).
 52. They report that block grants totaled \$386 million, hiring credits \$200 million, and other tax credits \$55 million. However, one important difference is that Busso, Gregory, and Kline provide estimates of the increase in net jobs. The net job gains from implementing RCJS could be smaller than gross gains if there is crowding out of other employment the participants would otherwise take, although there could be other jobs created if the neighborhood improvement is effective.
 53. 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 could 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 might or might not be what policymakers intended.
 54. See Matthews 2018 and an explicit proposal in Paul et al. (2018).
 55. For alternative views, see Collander (2016) and Neumark (2016b).
 56. For example, Collander (2016) sees nothing wrong with guaranteed jobs that dig and fill up holes, since he views the main merits of the proposal as providing a wage floor at which people can be employed
 57. One other point emphasized in Austin, Glaeser, and Summers (forthcoming) is that subsidizing employers rather than workers (via the EITC, for example) can be more effective in the presence of binding minimum wages—which are increasingly prevalent given the many states and even cities that have adopted historically high minimum wages. Worker subsidies work by reducing market wages, which can be constrained by minimum wages; see Neumark and Wascher (2011) for evidence on minimum wage–EITC interactions.

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Highlights

Many place-based policies have been unsuccessful, failing to deliver cost-effective benefits to disadvantaged communities; meanwhile areas across the county have large and rising concentration of poverty. David Neumark proposes that the federal government subsidize employment in places that are struggling, focusing on nonprofit jobs that contribute to local public goods.

The Proposals

Offer 18-month fully subsidized jobs through collaboration with local nonprofits.

Job subsidies in the first phase of RCJS will cover 100 percent of wages at or somewhat above the relevant minimum wage for an 18-month period. The jobs will be limited to workers residing in the targeted areas who are in families below 150 percent of the poverty line if the hired individual is already employed and 100 percent of the poverty line if the individual is not employed. The jobs must and contribute to revitalizing and improving the disadvantaged areas where the jobs are subsidized.

Partially subsidize a second 18-month phase of private sector jobs. Private sector jobs will be subsidized at a 50 percent rate for the first \$30,000 of annual earnings and employer eligibility for continued RCJS funds will be terminated if the retention rate of hires falls below 50 percent within one year of the end of the subsidies.

Target neighborhoods with high concentrations of poor people in extremely poor areas. The eligibility criteria for neighborhoods is four to six Census tracts in which, on average, 40 percent or more of individuals are below the poverty line

Benefits

RCJS would accomplish two goals for workers in struggling areas: first, to create a strong incentive for immediate job creation; and, second, to induce the transition of workers in subsidized jobs into higher-paid jobs in the private sector. The targeting of the proposal maximizes the impact on disadvantaged areas. In addition to creating jobs, the proposal will increase the productive potential and quality of life in the neighborhoods in which participants live.



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