Pre-Design Study for the Evaluation of Recovery Act Green Jobs, Health Care, and Other High Growth Competitive Grants

White Paper

April 6, 2010

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EXECUTIVE SUMMARY

The recent economic crisis that began in December of 2007 with a housing crisis and a contraction in available credit has led to a steady decline in jobs and rising unemployment rates across the country (Bureau of Labor Statistics [BLS], January 2010). From December 2007 to December 2009, the national unemployment rate rose from 5.0 percent to 10.0 percent. Although the number of unemployed persons in the nation rose from 7.7 million to 15.3 million during this time, still more individuals are either underemployed or have given up looking for work.

In February 2009, President Obama signed the American Recovery and Reinvestment Act (ARRA) into law to address the employment challenges facing America's workforce. With a total value of $787 billion, the Act's purposes include preserving and creating jobs, assisting people most affected by the recession, and promoting economic efficiency and long-term economic benefits (U.S. Congress 2009). A key aspect of the Act was its urgency, as reflected by its enactment shortly after the start of the new Congress and administration.

A major goal of ARRA is to train and assist workers for successful employment in high-growth and emerging industries, including the efficient energy and renewable energy sectors. As part of this goal, the U.S. Department of Labor (DOL) has created six grant-providing programs, four of which have been under DOL's consideration for an evaluation of their effectiveness: (1) Energy Training Partnership (ETP) Grants; (2) Pathways Out of Poverty (Pathways) Grants; (3) State Energy Sector Partnership (SESP) and Training Grants; and (4) Health Care Sector and Other High-Growth and Emerging Industries (Health Care) Grants.[1] Although these grants share many similar characteristics, they differ by providing funding to different types of grantees and, in some cases, by targeting different industries. They also have different target populations, such as workers who are unemployed and in need of basic skills development, workers dislocated from other failing industries, or those who are already in high-growth and emerging industries but in need of updated skills. DOL is considering conducting an objective and rigorous evaluation of the ARRA grants.

To gain insights about how best to proceed with evaluations of these grants, DOL has contracted with Mathematica Policy Research to conduct the Pre-Design Study for the Evaluation of Recovery Act Green Jobs, Health Care, and Other High-Growth Competitive Grants. The study aims to provide DOL with information about different types of methodological approaches for conducting the evaluations.

The purpose of this white paper, provided as the final deliverable product of the pre-design study, is to provide guidance to DOL as it assesses potential methodologies for evaluating each grant. First, we discuss the features of the grants that might influence the type of evaluation design that might be appropriate. We base our discussion on information that is available through the Solicitation of Grant Applications (SGAs), as well as the announcements of grant recipients for the three sets of grants that were awarded in January 2010. Second, we explore possible objectives of an evaluation of the ARRA grants and the types of research questions that DOL might be interested in asking. These include learning about the microeconomic (individual-level) effects on workers of

[1] In addition to these four, the ARRA created two additional grant types, State Labor Market Information Improvement Grants and Green Capacity Building Grants. Although these two sets of grants have some similarities to the four listed grants, the Solicitation of Grant Applications (SGAs) for those grants did not include information regarding a potential evaluation. Therefore, they are not discussed in the remainder of this paper.

training funded by ARRA grants, learning about the macroeconomic effects of the grants on communities or industries, and learning about implementation issues associated with the grants. Third, given the high level of methodological rigor that can be achieved with a random assignment design and DOL's special interest in this evaluation approach, we explore three different types of random assignment designs that could be used for an evaluation of the ARRA grants. We assess the strengths and weaknesses of these approaches, as well as how features of the grants, such as when grantees might begin serving participants, might influence their feasibility. We conclude that, although random assignment is the most analytically rigorous of the possible methodologies, it also is more likely than other approaches to interfere with normal program activity and will likely take longer to implement. We include some estimates of the sizes of program impacts that could be detected based on different types of random assignment studies and subsets of grantees, concluding that approaches based on a subsample of grantees might be feasible options for DOL to consider.

Fourth, we explore other types of methodologies, such as regression discontinuity, that could be used should a random assignment approach be deemed either inappropriate or infeasible. Although not as rigorous as random assignment, and therefore less convincing, these approaches still could provide information of considerable value to policymakers. Finally, we summarize the relative strengths and weaknesses of all of the methodological approaches that we examined, so that their relative merits can be compared.

The purpose of this white paper is not to recommend a single methodological approach as the best way to evaluate these grants, and it does not make such a recommendation. Rather, it explores the advantages and disadvantages of different methodological approaches so that DOL can consider its options for proceeding with an evaluation of these grants.

I. INTRODUCTION

The recent economic crisis that began in December of 2007 with a housing crisis and a contraction in available credit has led to a steady decline in jobs and rising unemployment rates across the country (U.S. Department of Labor, Bureau of Labor Statistics [BLS] January 2010). From December 2007 to December 2009, the national unemployment rate rose from 5.0 percent to 10.0 percent. Although the number of unemployed persons in the nation rose from 7.7 million to 15.3 million during this time, still more individuals are either underemployed or have given up looking for work.

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A major goal of ARRA is to train and assist workers for successful employment in high-growth and emerging industries, including the efficient energy and renewable energy sectors. As part of this goal, the U.S. Department of Labor (DOL) has created six grant-providing programs, four of which have been under DOL's consideration for an evaluation of their effectiveness: (1) Energy Training Partnership (ETP) Grants; (2) Pathways Out of Poverty (Pathways) Grants; (3) State Energy Sector Partnership (SESP) and Training Grants; and (4) Health Care Sector and Other High Growth and Emerging Industries (Health Care) Grants.[2] Although these grants share many similar characteristics, they differ by providing funding to different types of grantees and, in some cases, by targeting different industries. They also have different target populations, such as workers who are unemployed and in need of basic skills development, workers dislocated from other failing industries, or those who are already in high-growth and emerging industries but in need of updated skills. DOL is considering conducting an objective and rigorous evaluation of the ARRA grants.

To gain insights about how best to proceed with evaluations of these grants, DOL has contracted with Mathematica Policy Research to conduct the Pre-Design Study for the Evaluation of Recovery Act Green Jobs, Health Care, and Other High-Growth Competitive Grants. The pre-design study aims to provide guidance as DOL assesses different types of methodological approaches for conducting the evaluations.

A. Overview of the ARRA Grants

Although few industries were untouched by the current financial crisis, some, including the auto industry, were dealt devastating blows. This has resulted in many dislocated workers with few opportunities for rejoining industries that were once robust. At the same time, those who are already unemployed have even fewer opportunities to join the workforce. In recognition of this, ARRA has appropriated substantial funds for training and employment services for workers to join high-growth and emerging industries. To promote America's long-term prosperity, these industries

[2] DOL also recently has provided State Labor Market Information Improvement Grants and Green Capacity Building Grants. Although these two sets of grants have some similarities to the four grants described, the SGAs did not include information regarding an evaluation and are not included as part of this study.

include the efficient energy and renewable energy sectors, as well as health care and others. In addition to helping new workers join high-growth industries, training will also be available for workers already in these industries to improve their skills.

To help with the dual objectives of aiding worker transitions to high-growth fields and stimulating the economy, DOL issued four solicitations to provide worker training in efficient energy, renewable energy, health care, and other high-growth sectors.[3] Taken together, there will be about 150 grants totaling $660 million. DOL is positioning itself to conduct a rigorous evaluation of the ARRA grants to ensure the efficient and effective allocation of funds for similar types of services in the future. Initial steps to perform such evaluations are clearly established in the four program solicitations by requiring that "grantees must agree to participate in such an evaluation."

B. Purpose of the White Paper

The purpose of this white paper is to provide guidance to DOL as it assesses potential methodologies for evaluating each grant. We discuss the types of research questions that can be answered with different methodologies, as well as technical and analytical issues and tradeoffs associated with different methodological approaches. We also discuss other considerations for an evaluation, such as possible types of data sources that could be used. However, because the purpose of the paper is to compare and contrast a wide range of possible approaches, and because the information available about the grants is very limited, the discussion of each potential approach is not as comprehensive as would be expected during the design phase of an evaluation.

In developing our guidance, we take as a basic premise that, all else equal, more methodologically rigorous approaches are preferred to less rigorous ones. However, the reality is that not all else is equal. Different approaches have advantages and disadvantages. For example, some approaches can answer certain types of research questions but not others, and some require certain assumptions or conditions that might not be feasible or desirable in the case of these particular grants.

The purpose of this white paper is not to recommend a single methodological approach as the best way to evaluate these grants, and it does not make such a recommendation. Rather, it explores the advantages and disadvantages of different methodological approaches so that DOL can consider its options for proceeding with an evaluation of these grants.

Our discussion is based primarily on two types of information. The first is publicly available information about the grants, which comes from the announcements of the availability of the grant funding and, where available, the grant recipients. This information provides insights about DOL's goals for the grants, the main characteristics of the grants, and contractual issues that are important to take into consideration when designing an evaluation. Our second source of information is our individual and corporate experience conducting a range of evaluations of the workforce investment system for DOL, as well as evaluations of programs and grants for other clients. Many of our studies have used a randomized controlled design; others have used quasi-experimental designs when randomization is not the best option. Although each evaluation is distinctive, in part because each program or set of grants is distinctive, we hope that this experience provides a useful

[3] The main features of the grants are described in Chapter II.

perspective on the issues that need to be considered as DOL determines how best to move forward with its plans for the evaluations.

C. Organization of the White Paper

This rest of this white paper is organized into five chapters. In Chapter II, we describe the main features of the grants, focusing especially on the features that are relevant for comparing and contrasting different approaches to evaluating the grants. Doing so lays an important foundation for the discussion in later chapters of particular methodologies. Chapter III continues the foundation-building, but with a focus on issues that would influence any type of approach, such as the objectives and types of research questions that could be asked. Our discussion of specific methodologies, and the considerations that might influences their appropriateness for use with these four ARRA grants, begins in Chapter IV. That chapter focuses on random assignment experiments, which are generally thought to be the most rigorous designs, and therefore provide the most defensible answers to research questions. In Chapter V, we discuss other types of methodologies, which are important to consider if the disadvantages of random assignment are weighed as too large for that approach to be desirable. In Chapter VI, we tie together the main points from prior chapters in a brief summary.

II. FEATURES OF THE ARRA GRANTS

In the solicitations for grant applications (SGAs) for the four ARRA-funded grants that are part of this pre-design study, DOL provided descriptions of the important features of the grants. These features include the goals of the grants; the necessary characteristics of successful grant applicants; the target populations for services; the types of services and other activities that could be proposed; and others, such as the criteria by which grant applications would be judged.

Understanding the characteristics of the grants lays a critical foundation for the discussions in later chapters, because these characteristics will have a strong influence on the types of questions that DOL might want to ask about the grants, the relative merits of different types of approaches for evaluating the grants, and, ultimately, which approach or approaches are most appropriate for DOL to consider. Therefore, in this chapter, we describe key features of the grants, emphasizing those that are most pertinent to designing a sensible evaluation.[4] Because information on the grants currently is limited, this description is inherently general. Nevertheless, it will provide a foundation for understanding the strengths and limitations of different methodological approaches for evaluating the grants.

An overview of the four types of grants is provided in Table II.1. Although the four sets of grants share many similar characteristics, they differ by providing funding to different types of grantees and, in some cases, by targeting different industries. They also have different target populations, such as workers who are unemployed and in need of basic skills development, workers dislocated from other failing industries, or those who are already in high-growth and emerging industries but in need of updated skills. Because of these differences, we discuss each of them in turn, even though they share many characteristics. We begin by describing the ETP grants (Section A), followed by the Pathways grants (Section B), the SESP grants (Section C), and the Health Care grants (Section D).

A. ETP Grants

Of the four types of grants that are part of this study, the ETP grants were the first to be awarded. Therefore, available information includes information from the SGAs and additional detail provided in DOL's January 6, 2010, announcement of grant awards.[5] (Table A.1 in Appendix A contains a list of the ETP grant winners and a few aspects of their grant plans.) In this section, we describe the main goals of the ETP grants; their size and duration; the types of organizations that could apply for and receive the grants; the service components and allowable activities; the partnerships that are expected; the target clients; and expectations of the grantees that might increase their suitability for an evaluation.

[4] Most of the information about the grants comes from the SGAs for each of the grant types. When possible, we supplement this information from DOL announcements of awards of grants.

[5] Information on the grant awards is at http://www.doleta.gov/ETA_News_Releases/20091526.cfm and http://www.doleta.gov/pdf/ETP_SGA_Award_Summaries_120409.pdf, accessed January 27, 2010.

Table II.1. Overview of ARRA Grants in the Pre-Design Study

Grant Name	Goal of Grants	Number and Amount of Grants	Duration of Grants	Entities Eligible for Grants	Target Population	Other Distinctive Features
Energy Training Partnership Grants	To prepare workers for careers in the seven energy efficiency and renewable energy industries, and related occupations	25 grants awarded, ranging from $1.4 to $5 million each; about $100 million in total	Up to 24 months	Private nonprofit organizations that are either (1) national labor-management organizations with a local network or (2) statewide or local partnerships	Workers affected by national energy and environmental policy, needing updated training about energy efficiency and renewable energy, and unemployed workers	About $25 million was to be reserved for projects serving communities affected by auto industry restructuring. Sixteen awarded grants, totaling about $63 million, included service to one or more of these communities.
Pathways Out of Poverty Grants	To supply training and placement services providing pathways out of poverty and into employment for seven energy efficiency and renewable energy industries	38 grants awarded, ranging from $2.1 to $8 million each; about $148 million in total	Up to 24 months	(1) National nonprofit entities with networks of local affiliates, coalition members, or other established partners; or (2) local entities	Adults who are unemployed, high school dropouts, and those who have a criminal record or are disadvantaged individuals living in areas of high poverty	National grantees are nationwide entities with "networks of local affiliates." Need to have 3 to 7 communities served in total, in at least 2 states. Local entities must serve a substate area only.
State Energy Sector Partnership and Training Grants	To invest in workforce sector strategies that target green industries by encouraging a strategic planning process aligning the governor's workforce vision, state energy policies, and local and regional training activities leading to employment in energy efficiency and renewable energy industries	34 grants awarded, ranging from $2.5 to $6 million, about $187 million in total	Up to 36 months	State WIBs, where the term "state" includes the District of Columbia and the U.S. territories	Workers who are affected by national energy and environmental policy or who need updated training related to the energy efficiency and renewable energy industries, veterans, the unemployed, those with a criminal record, or individuals such as at-risk youth who are seeking employment pathways out of poverty	About $25 million will be reserved for communities affected by auto restructuring. State WIBs must demonstrate partnership with the state workforce agency, local WIB or regional consortia of WIBs, and One-Stop Career Center delivery systems.

Table II.1 (continued)

Grant Name	Goal of Grants	Number and Amount of Grants	Duration of Grants	Entities Eligible for Grants	Target Population	Other Distinctive Features
Health Care Sector and Other High-Growth and Emerging Industries	To provide training and placement services for workers in certain industries, including health care sector (nursing, allied health, long-term care, health information technology), and other high-growth emerging industries (information technology, advanced manufacturing, wireless and broadband deployment, and biotechnology)	45 to 65 grants are expected to be awarded, ranging from $2 to $5 million, about $220 million in total	Up to 36 months	Public entities or private nonprofit entities, such as LWIBs, tribal organizations, labor organizations, health care providers, education or training providers, and faith-based community organizations	Workers who are unemployed; dislocated; or need training to secure full-time employment, advance in their careers, or retain their current occupations. This last category—incumbent workers—includes low-wage and part-time workers.	About $125 million is expected to be reserved for projects dedicated to the health care sector. About $25 million is expected to be reserved for projects serving communities affected by auto industry restructuring.

Sources: Solicitations for grant applications (SGAs) for each grant type (DOL 2009a, DOL 2009b, DOL 2009c, DOL 2009d, DOL 2009e, DOL 2009f, DOL 2009g, DOL 2009h).

ARRA = American Reinvestment and Recovery Act of 2009; LWIB = Local Workforce Investment Board; WIB = Workforce Investment Board.

1. Goals of the Grants

In addition to providing economic stimulus to the economy, the ETP grants are intended to prepare workers for careers in any of the seven energy efficiency and renewable energy industries and related occupations.[6] These industries include: (1) the energy efficient building, construction, and retrofit industries; (2) the renewable electric power industry; (3) the energy efficient and advanced drive train vehicle industry; (4) the biofuels industry; (5) the deconstruction and materials use industries; (6) the energy efficiency assessment industry serving residential, commercial, or industrial sectors; and (7) manufacturers that produce sustainable products using environmentally sustainable processes and materials.

The green occupations for which workers can be prepared might be in a range of different areas. Specific examples given in the SGA include transportation, green construction, environmental protection, sustainable agriculture, including healthy food production, forestry, and recycling and waste reduction. Furthermore, grantees can propose to provide training for emerging green occupations that were not explicitly listed in the grant announcement.

2. Size and Duration

The SGA specified DOL's intention to award 20 to 30 grants, ranging from $2 to $5 million each for about $100 million in total. DOL expected that about $25 million of the total funding for this type of grant would be reserved for projects serving communities that were included in a list of 312 communities that have been affected by auto industry restructuring. The grant period could be up to 24 months.

Based on the announcement of grant winners, 25 grants have been awarded, with total funding of $99,760,688. The value of each grant ranges from $1.4 to $5 million. Sixteen of the grant recipients included service provision to communities that are affected by auto restructuring as part of their plans.

3. Eligible Grantees and Grant Recipients

Eligible ETP grant applicants must be private nonprofit organizations that fall into one of two categories: (1) national labor-management organizations with local networks or (2) state or local nonprofit partnerships. Neither type of grantee can receive both an ETP grant and a Pathways out of Poverty grant, although an organization that receives an ETP grant may participate as a partner in a Pathways out of Poverty grant.

National Labor-Management Organizations. A national labor-management organization is a nonprofit entity, such as a training fund, training trust fund, or an education trust fund, for which both employers and labor organizations participate on the executive board or comparable governing body. In most cases for this category of grant recipient, a national labor-management organization would be the official grant recipient. However, the official grant recipient can be a labor

[6] In the SGA, DOL provided broad definitions of both "energy efficiency" and "renewable energy." The former pertains to programs aimed at mitigating the use of energy, reducing harmful emissions, and decreasing overall energy consumption. The latter pertains to electric energy generated from solar; wind; biomass; landfill gas; ocean; geothermal; municipal solid waste; or new, efficient hydroelectric generation capacity.

organization in cases in which a national labor-management organization is not a distinct legal entity. For all grantees of this type, however, strong involvement by both the employer(s) and labor group(s) is expected in both the administration and governance of the ETP grant.

National labor-management grantees are required to offer grant services through state, local, or regional networks affiliated with the grant recipient. DOL expected that each grantee in this category would fund from two to five subgrants or subcontracts to the affiliates that would deliver training and other grant services to participants.

Statewide and Local Entities. According to the SGA, grant recipients in this category of grantee would be statewide or local private nonprofit entities, including private nonprofit entities that serve American Indian reservations, with a joint partnership of labor organizations, employers' or industry organizations, Workforce Investment Boards (WIBs), and One-Stop Career Centers.[7] A grant recipient in this grantee category cannot receive a subgrant or subcontract funding from a grant recipient under the national labor-management category.

Grant Recipients. Of the 25 grant awards made in early January 2010, 7 are to national labor-management organizations and the remaining 18 are to state or local organizations. Collectively, the grants include 30 states and the District of Columbia, and many of the grantees plan to provide services throughout an entire state or to a large number of cities or counties within the state.

4. Partnerships to Be Formed

In structuring the grants, DOL placed a strong emphasis on ensuring that grantees developed cohesive partnerships of stakeholders in the community, thus fostering a greater likelihood of success in the grant endeavors. As such, the SGA specified mandatory partners for the grants, as well as suggested additional partners. In addition to including labor organizations and employers' or industry organizations, both types of grantees needed to demonstrate partnerships with the local WIBs and One-Stop Career Centers. They also were encouraged to include in their grant activities (1) the education and training community; (2) apprenticeship offices, such as DOL's Office of Apprenticeship or the state apprenticeship agency; (3) a state office that focuses on energy, weatherization, environmental protection, utility boards, and other agencies with experience in the renewable energy and energy efficiency industries; and (4) faith- and community-based organizations. Furthermore, grantees were encouraged to include in their grant plans other organizations implementing projects funded by ARRA to create jobs in the energy efficiency or renewable energy industries, regardless of the Federal agency through which the funding was delivered.

5. Target Clients

DOL listed several categories of workers that would be eligible for ETP-funded training and other services. These include workers affected by national energy and environmental policy, workers needing updated training about energy efficiency and renewable energy, and unemployed workers. In addition to these, grantees would be allowed to serve individuals with criminal records,

[7] Throughout this chapter, the term *state* means each of the 50 states of the United States, the District of Columbia, and U.S. territories.

individuals with disabilities, and groups not traditionally employed in construction and skilled trades, such as women and minorities.

Taken together, grant winners proposed providing services to the full range of allowable target groups and to serve at least 34,000 participants (Table A.1 in Appendix A).[8] For example, the United Auto Workers-Labor Employment and Training Corporation (UAW-LETC) plans to provide services to 430 veterans, ex-offenders, individuals with disabilities, or women in Missouri. A grant provided to the State Labor Management Cooperation Committee for the International Brotherhood of Electrical Workers and the National Electrical Contractors Association (LMCC-IBEW-NECA) will serve 2,292 unemployed and underemployed electricians in California, and a grant provided to Blue Green Alliance will provide services to dislocated workers, women, minorities, veterans, incumbent workers, and unemployed steelworkers in Minnesota. The Oregon Manufacturing Extension Partnership will provide services to 1,670 unemployed and dislocated workers and employed workers at local renewable energy industries, such as the renewable electric power and biofuels fields. Other grant recipients include workers with a wide range of backgrounds and characteristics.

6. Services and Activities Allowed with Grant Funds

Grant applicants were encouraged to propose program models that were already identified through prior evidence as effective with the grantee's target populations, including strategies for recruiting and training participants, providing supportive services to help them complete grant-sponsored training, placing them in jobs, and helping them to retain their employment.[9]

One potential benefit of using existing, promising approaches for service delivery, rather than in the development of new approaches, presumably is a greater likelihood of improved post-training outcomes for participants. In addition, it could help to ensure a shorter start-up period before participants could begin receiving services, thus bolstering the economic stimulus benefits of the grants.

The training activities are to provide workers with the skills and competencies needed by the targeted industries and to support participants' advancement along a career pathway. Occupational, on-the-job, and customized training in the seven energy efficiency and renewable energy industries listed previously could be funded by the grant. Although wage subsidies are not allowed, grantees could include as part of their plans paid work experience activities, registered apprenticeship activities, or pre-apprenticeship strategies so that participants could gain occupational skills while having earnings, if this experience will lead to permanent employment. Because DOL wanted to ensure that the training is accessible to participants, grantees could use distance learning or technology-based learning. Finally, the training is to lead to an industry-recognized degree or certificate.

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[8] The information that is available from the grant announcement does not allow a full tally of the number of participants that grantees plan to serve. In some cases, the announcement provides the number of participants that grantees expect will participate in training. In other cases, the announcement provides information on the number of participants that grantees expect will be placed in jobs after training is completed. Thus, if all grantees meet their projected targets of enrollments and outcomes, the number of participants served is likely to be larger than 34,000, which was calculated as a lower bound given the available information.

[9] The SGA specified that grantees could use up to five percent of grant funds for supportive services.

In addition to providing training, grantees could propose using a portion of grant funds for activities that support the provision of training and attainment of credentials in the target industries. Examples given in the SGA include education or training for staff that will provide training or registered apprenticeships, the development or modification of curricula to deliver training, and the development or modification of apprenticeship standards and procedures for defining and issuing credentials within the energy-efficiency occupations. Because of DOL's desire for grant services to be provided quickly, a grantee needed to demonstrate why these developmental activities were valuable for the success of the grant.

Finally, successful grant applicants will need during the grant period to develop a plan for sustaining the activities after the grant is over.

7. Suitability for an Evaluation

The SGA specified that one component of the criteria used to determine grant recipients is the applicants' suitability for evaluation, and grant applicants were asked to demonstrate their suitability.[10] To receive points in this area, grantees needed to describe their recruitment plan that could yield a large number of qualified applicants for the program, including more applicants than the number of positions available. This consideration was included in the SGA to allow DOL the option of using a random assignment evaluation design to study the impacts of the grant activities on participants' outcomes, because the presence of excess demand for services would allow the formation of a control group in this type of evaluation. Grant applicants also were to describe (1) their ability to collect participant-level information on who applies to participate; (2) their retention strategies that would minimize client attrition and help researchers track participants; and (3) their willingness to work collaboratively with outside researchers conducing rigorous, independent research. Furthermore, grant applicants were to provide an explanation for why funding of their proposed plans would enhance knowledge about ways to effectively serve individuals and communities, including those not directly affected by the proposed program. These aspects of grant applicants' plans would be important regardless of the type of evaluation design that might be used.

Grantees are required to collect participant-level data, which will be useful both for the monitoring of the program by DOL staff and a potential evaluation of the grants. Although the list of data items about participants who receive services through the ARRA grants is still being developed, the broad categories of information to be collected include (1) demographic and socioeconomic characteristics, (2) employment history, (3) services provided, and (4) outcomes achieved. This individual-level information could be used to support evaluation efforts to understand who participates in the grant activities and to assess what participants' post-training outcomes are.[11] Furthermore, because grantees will need to collect Social Security numbers from clients, it will be possible to link the grant-specific individual-level data to unemployment insurance

[10] The suitability for evaluation counted as five points out of a total of 100.

[11] This information will form the basis of aggregate statistics that are reported to DOL on a quarterly basis. Grant winners are required to track outcomes about the number of participants (1) that are served; (2) that begin training; (3) that complete training; (4) that complete training and receive a degree/certificate; (5) that complete training and are placed into unsubsidized employment; (6) that complete training and are placed into training-related unsubsidized employment; and (7) that are placed in unsubsidized employment and who continue to be employed in the first and second quarters after the initial job placement.

(UI) administrative wage data, which can serve as another source of information about participants' pre- and/or post-training employment and earnings.

B. Pathways Out of Poverty Grants

The Pathways Out of Poverty grants are similar in many ways to the ETP grants (see Table II.1). Both types of grants focus on training workers for the seven energy-efficient and renewable energy industries and occupations listed above. Both are for two years and have two categories of grantees. Applicants to both grant types were encouraged to collaborate with other efforts that are intended to foster the "greening" of the economy, regardless of whether they are funded by ARRA or another source. Furthermore, the SGAs for each grant type had a similar emphasis that the grantees demonstrate their suitability for, and willingness to cooperate with, an evaluation.

However, there are some important differences between the two sets of grants, including their funding, the size of the grants, and the types of eligible grant applicants; the partners to be included in the grants; the target communities and clients, and the expectations for services that will be offered. Information discussed in this section comes both from the SGA and DOL's January 13, 2010, announcement of grant winners.[12] A summary of key features of grant recipients is in Table A.2 in Appendix A.

1. Grant Awards, Funding, and Grantee Types

Whereas the approximate total funding for the ETP grants is $100 million, the SGA for the Pathways grants specified that about $150 million in funding would be available. The 38 grant awards, announced January 13, 2010, totaled close to this amount, at $147,757,701.

Two types of grantees were specified in the SGA: (1) national nonprofit entities with networks of local affiliates or partners or (2) local entities. Grants awards for the first type, the national entities, range from $3 to $8 million; grant awards for the second type, local entities, range from $2 to $4 million. Of the 38 awards, 8 went to national grantees (with a total grant value of about $48 million) and 30 to local ones (with a total value of about $100 million). Collectively, the planned activities of the grant recipients are expected to take place in 27 states, the District of Columbia, and Puerto Rico.

National entities are defined as private nonprofit organizations that deliver services through networks of local affiliates, coalition members, or other affiliated partners, such as community- or faith-based organizations. Furthermore, the local affiliates must have the ability to provide services in four or more states. National grantees are expected to implement projects that serve communities located in at least two states and a range of three to seven communities served in total.[13] Similar to the ETP national labor-management grantees, Pathways grantees who receive a

[12] Information about DOL's announcement about grant winners can be found at http://www.doleta.gov/ETA News_Releases/20100039.cfm, and http://www.doleta.gov/pdf/Pathways_Poverty grants.pdf, accessed January 27, 2010.

[13] A community was defined as a geographic area located within one or more contiguous Public Use Microdata Areas (PUMAs), which are geographic statistical areas designated by the U.S. Census Bureau. For urban areas, these will be neighborhoods within a city rather than the entire city. For rural applications, it is likely that designated communities will be from one to three counties, American Indian areas, Alaska Native areas, or Hawaiian homelands. Although communities do not have prespecified sizes, it was expected that they would contain between 10,000 and 100,000 people.

grant under the category of national entities are required to fund subgrants or subcontracts to local affiliates and their partners, each of which will implement the project in its designated community. Of the eight grantees that are national grantees, most chose to serve communities in three or more states, with one planning to serve communities in six states.

The second type of grantee, local entities, are public organizations or private nonprofit organizations whose service area is limited to a single substate geographic area, such as a neighborhood, city, county, substate region, or interstate region comprised of multiple substate regions (such as Kansas City). According to the SGA, examples of potentially eligible local entities include entities such as community colleges, WIBs, community- or faith-based organizations, and tribal governments. The range of grant winners reflects this diversity. Local grantee applicants must propose serving a single, substate community.[14]

Neither type of recipient of a Pathways grant can receive an ETP grant. However, a Pathways grant recipient may participate as a partner in an ETP grant.

2. Partners

The list of mandatory partners for the Pathways grants is slightly different from that for the ETP grants. In each community served, regardless of grantee type, the partnership must include at least one entity from each of the following five categories: (1) nonprofit organizations, such as community- and faith-based organizations; (2) the public workforce investment system; (3) the education and training community; (4) public and private employers and industry-related organizations; and (5) labor organizations. By requiring that all of these types of organizations are represented in a partnership, DOL expected that grantees could take advantage of the existing resources within the community and that participants will be able to access a wide range of services that would help them to complete training, overcome barriers to employment, obtain jobs, advance along career pathways, and achieve economic self-sufficiency.

In addition to the required types of partners, grant applicants were strongly encouraged to include other partners that could contribute to the project, such as public housing agencies; community action agencies implementing the weatherization assistance programs; other organizations implementing ARRA-funded projects to support jobs in the energy efficiency or renewable energy industries; and foundations and other social service organizations that assist likely project participants.

3. Target Communities and Clients

Projects funded through the Pathways grants are to provide an integrated set of training and supportive services so that the target populations can exit poverty and gain economic self-sufficiency through employment in the targeted energy efficiency and renewable energy industries. Pathways grant recipients are planning to serve adult participants who have a range of disadvantages that might affect their labor market success. These disadvantages include (1) unemployment or underemployment, (2) the lack of a high school diploma or General Educational Development (GED) certificate, (3) the presence of a criminal record, (4) limited English proficiency or recent

[14] A local entity that receives an award under this SGA may not receive subgrant or subcontract funding through a grant awarded to a national entity under this SGA.

immigration or refugee status, and (5) residence in an area of high poverty. Other target groups include individuals who are dislocated from their prior employment, who receive public assistance recipients, or who are homeless. At least 90 percent of clients must live within the target communities to be served, which need to have poverty rates of at least 15 percent. However, up to 10 percent of clients may live outside of the target communities if they live in another high-poverty area.

Taken together, the Pathways grant recipients proposed providing services to the full range of allowable target groups described earlier (Table A.2 in Appendix A). Although the precise number that they plan to recruit and serve is not available, the number is likely to exceed 20,000.[15]

4. Services and Activities Allowed with Grant Funds

The services and activities that are expected to be offered with the Pathways out of Poverty grants are similar, but not identical, to those to be offered through the ETP grants. The activities will include recruitment and referral; training for careers in the energy-efficiency and renewable energy sectors; and job search, placement, and retention services. As with the ETP grants, applicants could propose occupational, on-the-job, and customized training activities, as well as registered apprenticeship and pre-apprenticeship programs. In addition, Pathways grantees can propose using grant funds to update training curricula if doing so is necessary to the effective provision of training. However, because of the different types of target populations for the two types of grants, these grantees—compared with the ETP grantees—are more likely to need to provide basic skills training to their participants as part of the package of services. Pathways grantees are allowed to provide basic skills training, such as adult basic education, English as a second language (ESL), and job readiness training; initial assessments of skill levels, aptitudes, abilities, and supportive service needs; and case management services. In addition, they will be allowed to use up to 10 percent of their grant funds for the provision of supportive services, which can help participants to complete training and overcome barriers to employment. In contrast, ETP grantees will be able to use up to five percent only for supportive services.

As with the ETP grants, Pathways grant applicants were encouraged to propose program models that have already been shown to be promising. DOL encouraged Pathways grantees to consider integration of occupational skills training with basic skills and work-readiness training, when needed, for low-income young adults. Grant applicants who intend to propose serving high school dropouts were encouraged to consider program models that link work-based learning and occupational skills training in the targeted industries with basic literacy and mathematics skills training to help participants obtain a high school diploma or GED. Those who intend to propose serving ex-offenders were encouraged to consider programs that, by integrating services both before and after release from prison or jail, have had positive impacts on employment outcomes. Finally, applicants intending to serve other types of disadvantaged individuals were encouraged to consider providing on-the-job training with a specific employer who agrees to hire individuals who successfully complete the training.

[15] See footnote #7 for an explanation of why the precise number is not available.

C. State Energy Sector Partnership Grants

Like the ETP and Pathways grants, the SESP grants have a strong emphasis on investments in strategies that target energy efficiency, renewable energy, and other green industries (see Table II.1). And, like the other grants, an important component of the SESP grant is the provision of training to provide workers with the skills and credentials necessary to obtain and retain employment, and to progress along career pathways in these industries.

However, the SESP grants were designed to highlight and foster the important role that states can play in these workforce sector investments and the transition to a more green economy. In this section, we describe some of the distinctive features of the SESP grants, which might influence the way in which an evaluation of these grants might be designed.

Information discussed in this section comes from the SGAs and DOL's January 20, 2010, announcement of grant awards.[16] A summary of key features of grant recipients is in Table A.3 in Appendix A.

1. Size and Duration of the Grants, Eligible Grantees, and Partners

On January 20, 2010, DOL announced the provision of $186,908,818 for the SESP grants. The 34 grants range from $2.5 million to $6 million. When the SGA was issued in June 2009, DOL expected to reserve some funds for communities affected by auto restructuring. (The SGA provided a list of 312 such communities.) In fact, 21 of the 34 grants included at least one community affected by auto restructuring. Although these grants total more than $120 million, it is not clear what portion of the grant funds will be targeted to these communities or to other communities within the states. Unlike the ETP and Pathways grants, which are expected to last up to two years, the SESP grants are for three years.

Eligible grantees consisted of the state-level WIBs of the 50 states, the District of Columbia, and the U.S. territories, although in practice all grant recipients were states and not nonstate jurisdictions. Grantees are required to partner with the state workforce agency, local WIBs or regional consortia of WIBs, and the One-Stop Career Centers within the state.

2. The SESP-Guided Energy Sector Strategy

Compared with the other ARRA-funded grants included in this pre-design study, the SESP grants have a distinctive component that focuses on the development or refinement, and ultimate implementation, of a statewide energy sector strategy, which is a workforce development approach that targets the needs of the energy sector. Furthermore, as part of their grant applications, state WIBs needed to include a charter for an SESP. The charter is to describe the SESP's purpose, goals, and functions; broadly speaking, however, the SESP's mandate will be to serve as a steering committee for the state's energy sector strategy. Ultimately, the SESP also will be responsible for ensuring that the specific SESP grant endeavors are successful.[17]

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[16] DOL's announcement of grant recipients can be found at http://www.doleta.gov/ETA_News_Releases/20100078.cfm and http://www.doleta.gov/pdf/SESP_Summaries.pdf, accessed January 27, 2010.

[17] SESP grant recipients will need to submit to DOL a sustainability plan prior to the end of the grant.

In encouraging the development and implementation of a state energy sector strategy through the SESP, DOL stressed the importance of having a comprehensive set of organizations and groups involved. The SESP is to be structured so that all of the grant partners will have an integral role in the strategic planning process that the SESP undertakes. Furthermore, the energy sector strategy is to be designed and implemented by a range of organizations and groups working collaboratively, including community- and faith-based organizations, business and industry groups, educational institutions such as community and technical colleges, the public workforce system, labor-management partnerships, and others. This statewide planning is to align with the governor's overall workforce vision, state energy policies, and feasible training activities that lead to employment in the targeted energy sectors.

In addition to the SESP charter, grant applicants had to include a list of local and regional project teams in their applications for funding. In addition to providing input about the strategy at the local or regional level, the purpose of these teams is to provide services to individuals as part of the grant efforts. Specific activities of these teams include the recruitment of training participants; the development and use of linkages with employers and training providers; and the provision of other services (such as case management, job retention, and supportive services) to support the success of the grant endeavors. As with the other types of grants, training can be in a variety of forms, such as on-the-job training blended with classroom training, customized training with an existing registered apprenticeship program or labor-management partnership, technology-based learning, or other appropriate training strategies.

3. Participants Eligible to Receive Training

Although much of the grant-funded provision of services directly to workers will be guided by the SESP energy sector strategy, ETA provided guidance in the SGA about which groups of workers are most appropriate for grant-funded activities. The local and regional teams that are to provide training and other services funded by the SESP grants must give priority to the following target populations: (1) workers affected by national energy and environmental policy; (2) individuals in need of updated training related to the energy efficiency and renewable energy industries; (3) veterans; (4) unemployed individuals; (5) individuals, including at-risk youth, seeking employment pathways out of poverty and into economic self-sufficiency; and (6) individuals with criminal records. Other individuals, such as entry-level and incumbent workers and individuals with disabilities, also may be served through the SESP grants.

Collectively, the SESP grant winners plan to provide services to this wide range of target groups (Table A.3 in Appendix A). Based on their proposals, the grantees are likely to serve at least 54,000 participants.[18]

D. Health Care Sector and Other High-Growth and Emerging Industries Grants

As the title of this set of grants clearly indicates, these grants focus on training workers in the health care sector and other high-growth and emerging industries (see Table II.1). This industry focus makes this set of grants distinctive among the four sets that are part of this pre-design study.

[18] See footnote #7 for an explanation of why the precise number of participants is not available.

Despite this difference, however, many of the basic features of the Health Care grants are similar to other grants. The Health Care grants' allowable activities and services, for example, have a similar focus as the Pathways grants, because both types of grants might be especially likely to serve participants in need of comprehensive services. Grantees will be expected to work with participants from the time they are recruited, through training, job placement, and job retention. Grantees were strongly encouraged to propose providing training services that would lead to portable employer- or industry-recognized certificates or degrees and enable participants to progress along a career pathway or lattice. Furthermore, as with other grant types, the training could be in a variety of types (such as through Registered Apprenticeships, classroom training, on-the-job training, or customized). Applicants were encouraged to use existing training curricula but could propose modifying existing curricula if a strong justification for doing so was given. As needed, grantees could offer initial assessments, basic skills training, English as a Second Language (ESL), job readiness training, and case management. In addition, they could use up to 10 percent of grant funds for supportive services to assist participants as they receive other services. And, grant applicants were encouraged to propose program models that had been found to be successful for participants who are similar to the applicants' target groups.

In addition to the similarities with other grant types in terms of the allowable activities and services, Health Care grantees were encouraged to try to coordinate with other ARRA programs, and applications are being assessed, at least in part, for their suitability for evaluation.

As with our discussions about other grants, therefore, we focus the rest of our discussion in this section on the distinctive aspects of this set of grants.

1. Goals of the Grants

As noted earlier, the Health Care grants are intended to help workers receive training and job placement services in certain industries, including the health care sector and other high-growth emerging industries. Examples of health care fields for which individuals might be trained include nursing, allied health, long-term care, and health information technology. If an applicant is interested in providing services for a high-growth or emerging industry other than health care, DOL encouraged grant applicants to define the industry within the context of their state or regional economy; to be considered a high-growth or emerging industry in a local area, the grant applicants would need to demonstrate that the industry is projected to add a large numbers of new jobs to the economy, that technology and innovation require new skill sets for workers, or more generally that it is a new and emerging industry projected to grow. As examples of emerging industries, the SGA mentioned information technology, advanced manufacturing, wireless and broadband deployment, and biotechnology.

2. Size and Duration

As of the SGA release date, DOL expected to provide a total of about $220 million in funding, through 45 to 65 grants that would range from $2 to $5 million. About $125 million of the funds were to be reserved for projects dedicated to the health care sector. About $25 million was expected to be reserved for projects serving communities affected by auto industry restructuring.

3. Eligible Grantees

In contrast to the ETP and Pathways grants, each of which specified more than one category for grant applicants, the Health Care grants did not distinguish among grant applicants on the basis of their organizational structures. Eligible applicants for the Health Care grants include public entities or private nonprofit entities, such as LWIBs, tribal organizations, labor organizations, health care providers, education or training providers, and community- and faith-based organizations.

4. Partnerships to Be Formed

As with other grant types, DOL emphasized the importance of strong partnerships that would ensure that grant winners would be able to provide a comprehensive set of services to participants. Winning grant applicants need to include at least one entity each from (1) the public workforce investment system; (2) public and private employers, such as health care providers when appropriate, and industry-related organizations; and (3) the education and training community. DOL also encouraged applicants to include other partners, such as nonprofit organizations, labor organizations, entities implementing related projects funded by ARRA, foundations, and state and local social service agencies.

5. Target Clients

Health Care grantees will be allowed to serve individuals who qualify for services based on a wide range of characteristics. Target groups of participants include unemployed workers; dislocated workers; and incumbent, low-wage workers or those who need training to secure full-time employment, advance in their careers, or retain their current occupations. In addition, grantees can target individuals on public assistance, high school dropouts, individuals with disabilities, veterans, and individuals with limited English proficiency.

III. POTENTIAL OBJECTIVES AND CONSIDERATIONS OF AN EVALUATION

The design of an evaluation is intricately linked to the study objectives and research questions of interest. As such, it is useful to discuss broadly the potential objectives and research questions of an evaluation of the four sets of ARRA grants that are included in this pre-design study before turning to the details of the designs, because the design that is most appropriate will depend on the research question(s) of interest.

In order to identify the objectives of an evaluation, we first recall the goals of the ARRA grant funding. Each grant type is designed with specific goals in mind: to provide workers in need of career development or retooling with the skills and credentials necessary to obtain and retain employment. With the ETP, Pathways, and SESP grants, the focus is on employment in seven energy efficiency and renewable energy sectors; the remaining grant focuses on health care and other high-growth and emerging sectors. The grant types also have target populations in mind. For instance, three grant types (ETP, SESP, and Health Care) have specific monies set aside to target individuals affected by auto industry restructuring. Pathways grants are designed to move a highly disadvantaged population (including high school dropouts and individuals with criminal records) out of poverty and into employment.

The grants may also provide macroeconomic benefits, such as economic stimulus during a recessionary period and aiding in the expansion of target occupations and industries, particularly those in the energy efficiency, renewable energy, and health care fields. Although the grants support these efforts, their main focus is training workers for jobs, and grant funds cannot be used for direct job creation.

With the main grant goals in mind, in this chapter we present a series of potential objectives that may be considered in the evaluation of ARRA grants (Section A). Because the grants are primarily intended to train and obtain employment for individual participants, we then discuss some of the considerations that must be taken into account for any design that aims to assess participant-level outcomes (Section B).

A. Potential Objectives of an Evaluation

We consider three possible objectives of an evaluation that could be of interest to DOL: (1) learning about the microeconomic effects of training on workers, which examines outcomes at the individual level, (2) understanding the macroeconomic effects of the temporary funding stream in funded communities, which examines outcomes at the community level, and (3) examining implementation and operational issues involved in deploying ARRA grants. Although DOL has expressed special interest in the first of these (evaluating the microeconomic effects of training on workers), we also discuss the other possibilities because they could provide valuable information and could be done either in concert with an analysis at the individual level or instead of such an analysis.

1. Learning about the Microeconomic Effects of Training on Workers

If the evaluation is to determine whether training has an effect on the outcomes of workers, an approach in which data are gathered and analyzed at the individual-worker level is appropriate. In the context of ARRA grants, such an evaluation could answer the following research question: "What was the impact of access to ARRA grant funding on participants' outcomes?" The results of this type of evaluation will need to be put in the context of the unique environment in which ARRA

grants are awarded, including high unemployment and severe economic dislocation in some parts of the country. However, in addition to answering the specific question of what impact the ARRA grants had given the current economic circumstances, the results from a study that aims to assess the effect of training on workers can more generally provide guidance about how policy initiatives can be shaped in the future, even after the economy recovers. The issues related to the selection of outcomes and potential sources of data for a study conducted at this level are discussed in detail in Section B.

2. Understanding the Macroeconomic Effects of the Temporary Funding Stream

Although DOL has expressed interest primarily in the effects of ARRA-funded training on workers, examining the effects of the ARRA grants in funded communities may be informative, given that the grants support two macroeconomic goals: economic stimulus and development of the green economy through training of workers. Such an evaluation would answer the following research question: "What was the impact of ARRA grant funding on the macroeconomic conditions in communities that received grants?"

If the evaluation is to determine whether ARRA grants more generally had an effect on macroeconomic health, then an approach in which data are gathered and analyzed at the community or local labor market level is appropriate. An analysis of effectiveness at the macroeconomic level would examine broader trends in the local economies that received grants. For instance, the evaluation could examine outcomes such as the unemployment rate and the job creation rate in green industries or health care industries, depending on the focus of each grant type. Given the diversity in the specific types of industries and occupations that the grants target, care would be needed to ensure that the outcome measures that are chosen for examination are pertinent and available for each grantee. Chapter V discusses some methodologies that could be used to examine these types of outcomes.

3. Examining the Implementation and Operational Issues Involved in Deploying ARRA Grants

Learning about the experiences of grantees as they implement ARRA grants could provide valuable information both to policymakers and service providers nationwide as future efforts build upon the work of ARRA grantees. Even seasoned service providers in the workforce investment system are likely to be challenged as they implement programming funded by ARRA grants; the grants offer states and communities the opportunity to provide training and other services to help workers transition to careers in high-demand, emerging industries not previously targeted within the workforce investment system. Among these challenges are establishing or enhancing their partnerships and processes; developing or modifying curricula and certification standards in emerging occupations; recruiting and serving participants; or other facets of grant operations.

In addition to examining the general challenges that ARRA grantees are likely to face in implementing their grant activities, two facets of the grants are especially well suited for an implementation evaluation. The first facet is grantees' development or modifications of products to assist in training activities. Because many of the grant types target emerging industries, some grantees plan to develop or modify training models and the curricula used in training to accommodate the focus on these new industries. Pathways grantees, which target a highly disadvantaged population, may also develop innovative remediation techniques to improve the skills

of individuals they serve. An implementation study would be an appropriate way to examine grantees' efforts to develop, modify, and implement these new curricula.

The second facet of the grants that is well suited to an implementation evaluation pertains exclusively to the SESP grants. Examining operational issues might be especially important for these grants, which have a strong emphasis on the development and implementation of a statewide energy sector steering committee. The purpose of this committee is to guide the development (or refinement) of the state's energy sector strategy both during and after the end of the ARRA grants. The techniques that are used in an implementation study are the most appropriate ones to assess the effects of this steering committee.

Even if an evaluation of the ARRA grants does not have these implementation issues as its primary focus, an implementation study could be an important component of any evaluation of the grants. A detailed implementation study could collect data on services received by participants, where they were received, the duration of service receipt, and so on. This information could be used to interpret any findings about the effects of the grants. For example, if no micro- or macroeconomic effects are found, then one could use information about the services received by participants to try to gain insights about why that is the case. Even when beneficial effects are found, implementation information is useful for gaining insights about activities that might need to be duplicated in the future to achieve similar results, and which aspects could be enhanced.

An implementation study typically involves the collection and analysis of data through discussions with or surveys of grantee staff, observations of grant activities, discussions with and/or surveys of participants, or other methods. Although an implementation study is likely to be at least one component of an evaluation of the ARRA grants, focusing on this type of analysis is outside of the scope of this pre-design study. The details of an implementation study are likely to depend greatly on whether or how DOL pursues the other objectives for an evaluation, which we discuss in this white paper.

B. Considerations for a Study of the Effect of Training on Individuals' Outcomes

Because DOL is likely to desire an approach examining outcomes at the individual level, we focus here on two key considerations that guide the design and interpretation of this approach: (1) understanding the types of activities in which individuals in the control or comparison groups engage and (2) recognizing factors that may influence what the grants can accomplish. In this section, we discuss each of these in turn. We conclude by discussing some outcomes that could be examined, given these important considerations.

1. Need for an Understanding of the Counterfactual

Answering a question about the impact of the grant on individual outcomes relies on some type of comparison of the outcomes of individuals who receive ARRA-funded training with those who do not. Regardless of the specific design used (for example, random assignment or comparison group), it is essential that an evaluation includes a firm understanding of the types of activities in which individuals in the control or comparison group (known as the "counterfactual") engage. For this particular evaluation, it is important to note that a great deal of funding is currently available for

training through a number of sources. In addition to the four ARRA grant types, sources such as One-Stops may have additional funding for training.[19]

The activities the nonparticipants engage in strongly influence the research questions that can be examined. To illustrate this, consider two extremes. In the first, individuals in the comparison group receive no training from other sources; this would enable an evaluation of the impact of ARRA-funded training compared with no treatment at all. The second is the opposite case, in which all members of the comparison group received the same amount of training as ARRA participants but through a different source, such as regular WIA formula funding. Then an evaluation of the impact of ARRA-funded training on employment outcomes, such as employment status and quarterly earnings, would actually be an evaluation of the impact of ARRA-funded training *relative to other sources of training funds*.

Depending on how similar these alternative training services are to the training funded by ARRA grants, the evaluation might not be capable of identifying the differential effects of ARRA-funded training. If it is likely that members of the comparison group can easily seek comparable training elsewhere, one could potentially mitigate this by applying a more narrow definition of the outcomes of interest. For instance, one could examine employment status in ARRA-targeted industries specifically, rather than employment status in any industry.

Because the types of individuals targeted by the grants varies considerably, it is important to recognize that the counterfactual may vary by the types of individuals receiving services. For example, One-stop Career Center customers might get regular (formula-funded) WIA services if the ARRA grants are not available. However, the counterfactual for incumbent workers might simply be no training at all if the grants are not available. The counterfactual for treatment group members recruited from a community-based organization might be some other type of training service altogether.

The considerable variation in the populations and industries targeted by ARRA grants, and the services the participants in the comparison or control condition can receive could affect the interpretation of the study's findings, as well as the ability to detect significant impacts. Thus, it is important to explore these factors in the design phase of an evaluation in order to construct suitable comparison or control groups.

Finally, it is worth mentioning that members of the counterfactual group face economic circumstances that almost certainly differ from those they would face if the grants were deployed in a more stable economic environment. This may influence the extent to which different types of people seek job training services and, perhaps, which types seek ARRA-funded training versus training funded through other sources. Although this limits the extent to which the impact of ARRA funding can be generalized to a time of relatively better economic circumstances, it does not necessarily indicate that it will be more difficult to detect impacts.

[19] For example, since October 2009, DOL has announced grants of $447 million in Trade Adjustment Assistance funds and $55 million in Green Capacity Building Grants, in addition to many state-specific grant awards.

2. Factors Influencing What the Grants Can Accomplish

Any evaluation of ARRA grants will have to be conducted in light of additional factors that could influence what the grants can accomplish. First, certain grant types, namely Pathways, target a highly disadvantaged population. It might be more difficult and more costly to effect change within this group of participants than within other groups of workers. To some extent, all of the grants target job training to be used in emerging industries. It is possible that there simply are not enough available jobs in these industries to provide employment for many ARRA-funded training participants. Moreover, because it is grantees' first experience working with these industries, it may be more difficult to place individuals in those jobs that are available.

However, the evaluation designs proposed in Chapters IV and V of this paper mitigate concerns of these influences on the estimation of program impacts on participants' outcomes; impacts are estimated by comparing the outcomes of individuals who receive ARRA-funded training with the outcomes of those who do not, holding all else equal. The control or comparison groups are created so that, to the extent possible, their members have similar characteristics and face similar sets of job openings as participants. Thus, estimation of program impacts will not be biased by these influences. In addition, as discussed in Section A, a qualitative component could supplement a quantitative analysis of the ARRA grants' impacts on participants' outcomes, providing insights about these contextual influences on grant activities and outcomes.

3. Potential Outcomes to Be Examined

The outcomes of interest to the evaluation must be selected according to the research questions of interest, taking into account the issues surrounding the counterfactual and the targeted individuals and industries, as previously described. Studies of the effects of training or employment services often include as outcomes employment status and earnings, measured for a period of one to several years after enrollment in a program. However, different outcomes for an ARRA grants evaluation might be warranted because the grants are specifically targeted toward occupations in certain industries and to specific populations of individuals. Therefore, we highlight two types of outcomes: (1) general labor market outcomes and (2) outcomes tailored to the evaluation of ARRA grants.

1. Quarterly earnings and employment status are labor market outcomes commonly examined in evaluations of training programs. These outcomes can be available through unemployment insurance (UI) wage records linked to participant data using participants' Social Security numbers (SSN); as noted in Chapter II, ARRA grantees are required to collect SSNs so that participant information can be linked to these records.

   Although grantees are required to collect some information on participants' engagement in training and other services, it is often the case with studies of the effects of training on workers' outcomes that the utility of the administrative data would be enhanced by the use of a follow-up survey; this could be a source of rich, detailed information about individuals' participation in ARRA-funded training and other services as well as the characteristics of their post-training jobs. For both the survey and administrative data, the follow-up period must be long enough after participants become involved in grant activities to observe meaningful training and employment outcomes. Follow-up periods for studies that Mathematica has conducted to evaluate training programs often range from one to several years.

2. Targeting grants to occupations in certain industries may warrant examining more specific outcomes than those traditionally examined in these types of evaluations. For instance, rather than looking at the employment status of individuals who received training regardless of the industry in which they found employment, it may be more meaningful (and more indicative of program effectiveness) to examine employment and earnings in the industries targeted by the grant. For example, this approach would suggest examining the employment and earnings in health care and related industries for those individuals who received training through Health Care grants; for Energy Training Partnership Grants one could examine the employment status in the seven targeted energy efficiency and renewable energy industries.

   Targeting grants to certain groups of individuals may also influence the selection of outcomes of interest. For example, the Pathways grants are targeted to adults who are unemployed, high school dropouts, have criminal records, or are disadvantaged individuals and living in areas of high poverty. For these individuals, changes in receipt of social services or rearrest rates may be outcomes of interest. For other grant types, progression along a career pathway as indicated by job promotion or salary increases may be important.

As mentioned previously, the individuals with whom ARRA grant participants are being compared may also receive some types of training. This argues for adopting one or both types of tailored outcomes in order to identify program effectiveness. Both of these types of outcomes would likely have to be collected through a survey of participants. Some pertinent information relating to these outcomes might be available through administrative data or a grant-specific data set. However, because survey questions can be tailored to the specific topics of interest, it is likely that the quality of the information available through a survey would be higher.

Regardless of the source of the outcomes data, as well as the methodological approach for conducting the evaluation, it will be important to limit the number of outcomes of interest to a small number. The likelihood of finding a statistically significant impact estimate when none exists increases as the number of examined outcomes increases. Although statistical techniques can be used to adjust for the increased likelihood of falsely detecting a statistically significant impact, the generally preferred approach is to limit the number of outcomes by which the success of an intervention is judged to a small number prior to data collection and analysis.

IV. RANDOM ASSIGNMENT IMPACT DESIGNS

DOL has as its goal a rigorous evaluation of the ARRA grants. Although evaluations can take different forms, a random assignment evaluation is often referred to as the gold standard in evaluation design. When designed and implemented well, random assignment evaluations can provide very convincing evidence about the impacts of the intervention under study, because its results are based on the highest available degree of statistical rigor.

In examining different types of methodological approaches to evaluating the ARRA grants, we focus in this chapter on random assignment; other possible approaches are discussed in Chapter V. We take as a premise that a random assignment evaluation would be intended to estimate the impacts of the grants on participants' outcomes. That is, we assume in our discussion that community-wide outcomes, which were discussed in Chapter III, are not a primary focus of a random assignment evaluation design.[20]

In this chapter, Section A lays a foundation for the chapter by providing an overview of key components of a random assignment study.[21] We then describe three possible ways in which a random assignment study of the grants could be designed (Section B). In Section C, we describe some considerations related to the details of the random assignment process. In Section D, we describe considerations about the timing of the grants and random assignment. Finally, in Section E, we present estimates of the minimum detectible impacts (MDIs) that might be feasible to achieve with two of the random assignment designs in which DOL has expressed the most interest.

A. Overview of Design Features

At a conceptual level, random assignment is an extremely simple method for evaluating the impacts of programs or services, such as those offered by the ARRA grants. At the same time, it is an extremely difficult approach to implement well. In this section, we provide a description of how random assignment works and some of the components that are most pertinent for consideration when planning an evaluation of the ARRA grants.

1. Basic Approach and Strengths of the Method

The goal of any random assignment evaluation is to yield precise, unbiased estimates of the effectiveness of the intervention under examination. Random assignment experiments involve randomly assigning individuals (or other units of analysis) into two or more research groups, with each group offered the option to receive a different set of services. When implemented carefully,

[20] It is theoretically possible to use random assignment to assess community-wide outcomes. However, doing so would likely involve the random assignment of either (1) grant applicants into recipient or non-recipient status or (2) communities that grant recipients would want to serve into grant-funded communities or non-funded communities. Each of these approaches would require a major deviation from the current direction of the grants. The first of these approaches seems infeasible because the awarding of grants has already begun. The second of these approaches does not seem feasible for grantees that plan to serve only one community, and it would require a major restructuring of the way in which multi-community grantees will unfold their grant plans.

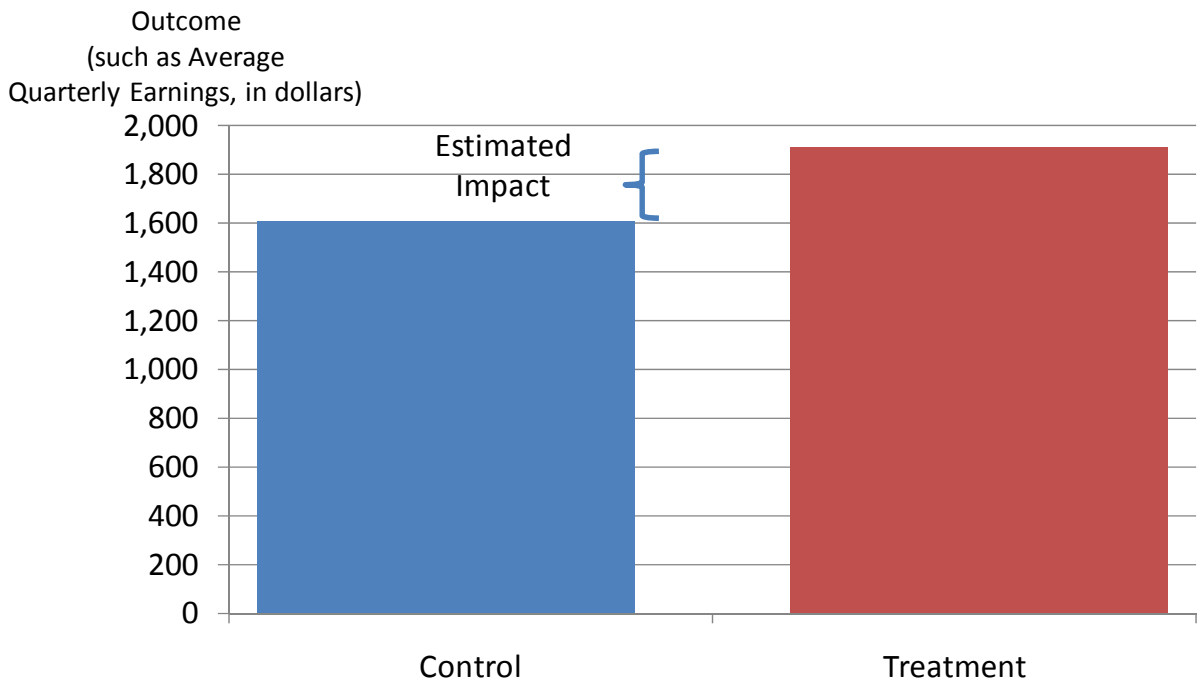
[21] Many of the conceptual points described for this type of study would also be pertinent for the other types of methodologies, which are discussed in Chapter V. However, given DOL's focus on random assignment as a possible approach, we provide details about them in this chapter and avoid repetition in the later chapter.

random assignment creates groups that are almost identical in their characteristics prior to the random assignment process; the only systematic difference between them is in whether they are exposed to the intervention, such as ARRA grants-funded training, after being randomly assigned to a group. As a result, we can confidently conclude that the treatment caused any observed differences in the average outcomes between the two groups (Figure IV.1). Furthermore, the simplicity with which results can be interpreted provides additional appeal to this design.

2. Important Components for a Random Assignment Evaluation

Despite the simplicity of interpretation of the results from a well-designed and well-implemented random assignment study, the underpinnings of the study can be complex. A random assignment study needs to have certain features, and other features are highly desirable even though technically not necessary.[22] Here, we focus on four of the most important and common components of this type of study design.

Figure IV.1. Impact Estimation from a Random Assignment Design



Note: This figure uses hypothetical data to demonstrate a random assignment design with an outcome such as average quarterly earnings on the Y-axis.

There must be excess demand for intervention services. As implied earlier, a random assignment design of the ARRA grants would require that people who express an interest in

[22] Chapter III contains a discussion of three important considerations when developing a design for an evaluation: (1) the importance of defining the research questions to meet policymakers' areas of interest; (2) the need for a clear understanding of the counterfactual; and (3) the need to focus the outcomes of interest to a small number, and what those outcomes might be. Although these issues are important aspects of a random assignment design, we do not discuss them again in this chapter.

receiving grant services can be randomly assigned either to a treatment group that is eligible to receive services or to a control group that is not. Thus, grantees will need to have excess demand for their services—that is, more eligible individuals requesting services than can be served—so that all program slots are filled and that the sample sizes of the treatment and control groups are adequate for the impact analysis. As explained in Chapter II, grant applicants were encouraged to demonstrate their suitability for an evaluation, including their ability to generate excess demand for their services. DOL indicated its intention to take this characteristic into consideration when determining which applicants would win grants; five points of a possible 100 grantee evaluation points were given for suitability for an evaluation. If ARRA grantees are asked to be part of a random assignment evaluation, therefore, they must either naturally have excess demand for their grant-funded services, be able to generate the extra demand through aggressive marketing of their services to their target groups, or modify their grant plans to include a more expansive set of target groups (which will lead to more applicants who are eligible for services).

Regardless of the approach chosen to yield excess demand for services, the grantees that are part of a random assignment evaluation would be required to deny services to members of the control group.[23] Based on our experience conducting random assignment evaluations for other studies for DOL and other clients, grantees are often reluctant to do this, even though this method for restricting service access can be perceived as a fair way to determine access to limited resources.

Although we do not know for sure, it is possible that the current weak economy also might affect how ARRA grantees respond to a suggestion about using random assignment. On one hand, they may recognize already that they cannot possibly serve all individuals who might need their services; thus, they might be more amenable than is often the case to the prospect of using random assignment to determine who is eligible for services. On the other hand, they may find it especially hard and unpalatable to deny services to individuals who express interest in receiving services, because the weak economy might make it harder for the individuals to obtain jobs without assistance.

All random assignment studies must have a control group, but one way to mitigate the potential concerns of ARRA grantee staff—if a random assignment design of the ARRA grants is used—is to limit the portion of study sample members who are assigned to the control group. Typically, random assignment of half of the sample members to the treatment group and half to the control group is the most efficient allocation and gives an evaluator the best chance of detecting impacts. However, a study design that assigns fewer than half of sample members to the control group might be more palatable to grantees, while only slightly reducing the ability to detect impacts.

Random assignment designs should have samples of sufficient size to yield adequate statistical power and policy-relevant minimum detectible impacts. A rigorous evaluation of an intervention's impacts requires a sample size that allows the detection of impacts that are large enough to be policy relevant. Minimum detectable impacts (MDIs) are the smallest true impacts on outcomes for which there is a high probability (80 percent) of detecting them; the smaller the MDI,

[23] In some random assignment studies, control group members are given access to treatment group services, but only after the study has completed data collection. Thus, technically, they are delayed in their receipt of services and not fully denied access to these services. This distinction is unlikely to be useful for an evaluation of ARRA grants, because the grants are time-limited and because control group members are unlikely to want to wait until after the study to pursue training.

the greater the statistical power of the design. For example, an MDI of $200 in average quarterly earnings means that if the quarterly earnings for the treatment group are $200 more than those of the control group, then the study will be able to detect this difference with a high probability. In addition to certain technical assumptions used for calculating a study's MDIs, key factors influencing a study's power include (1) the study's sample size; (2) whether the results are to be generalized to a population that is broader than the study sample members; (3) the standard deviation in the outcome (or outcomes) of interest; and (4) the extent to which the outcome(s) vary across sample members within clusters (typically, sites) or across clusters.[24] Typically, assumptions about the last two of these factors are made on the basis of evidence from prior research.

To help assess the MDIs that might be achieved with different types of random assignment designs for an ARRA grants evaluation, we present results of a power analysis in Section E. We focus on two types of outcomes, employment and quarterly earnings, because these are often the primary outcomes of interest in evaluations of DOL-funded training and reemployment services. It is likely that results would differ somewhat if other types of outcomes, such as employment in industries targeted specifically by the grants, are used.

Baseline and services data are important for strengthening a random assignment study. In addition to the outcomes data that were discussed in Chapter III, random assignment studies often collect two types of data on study participants: (1) baseline data describing the characteristics of customers prior to random assignment and (2) service receipt data. The baseline data can be used to verify that random assignment created groups that have similar characteristics. Furthermore, baseline data can enhance the impact analysis, by describing the groups served by the program under study (such as the ARRA grants) and by defining subgroups of interest in the impact analyses. Such data also can be used to improve the precision of impact estimates. Although baseline data are often collected through the completion of forms developed specifically for the evaluation, some of the relevant data may come from forms completed as part of individuals' applications for services or through administrative records.

Data on the receipt of services by both treatment and control group members also is important to collect. These data can be used to ensure that control group members do not receive intervention services (in this case, ARRA grant services) and to assess the extent to which treatment group members receive these services. In addition, analysis of these data can help to interpret impacts on outcomes. In the case of the ARRA grants, the grantee-collected data will cover service receipt only for treatment group members. One source of data on services received by both the treatment and control group members might be WIA files maintained by states, coupled with financial data maintained by LWIAs to provide information on the value of individual training account (ITA) amounts. However, these data files will not cover receipt of most types of services that are not funded by WIA. Another source that could provide data that are more comprehensive, as well as

[24] When sample members are clustered within a grantee site, they are likely to face similar economic conditions and have similar demographic characteristics to each other; therefore, their outcomes are likely to be more similar to each other than to sample members in other sites. This correlation in outcomes within a site would increase the variances of the impact estimates relative to those from a simple random sample of the same size because the variance formulas must include a term representing the between-site variance in the outcome measures. Thus, when sample members are clustered, a larger sample size is needed to achieve a certain MDI compared to what would be the case with a simple random sample.

standardized across study sites, could be a survey of the treatment and control groups sample members.[25]

Another type of data that is often, but not always, collected as part of a random assignment study is data on the benefits and costs of the program. As a useful supplement to an impact evaluation, these data could provide answers about the net benefits (or costs) of the services that participants receive, measured in monetary terms.

The analysis approach can provide answers to a range of questions of interest. As noted earlier, an advantage of a random assignment study design is its simplicity. When executed properly, simple comparisons of the differences in outcomes between the treatment and control groups can give unbiased estimates of the impacts of the intervention. However, using the data described earlier, and possibly qualitative data collected through an implementation component to the study, enhancements to this basic approach could improve the quality of information that is gained from the study and provide additional insights about issues of interest to policymakers. As alluded to previously, additional analyses in an evaluation of ARRA grants could be used to describe who participated in grant-funded services and what types of services they received. These analyses could provide information about the quality of implementation and whether some types of participants benefitted from grant services more than others. And, depending on how nearly complete follow-up data are, it might be appropriate to assess whether any biases arise from different rates of response to follow-up data collection.

B. Possible Approaches to Using Random Assignment for ARRA Grants

In this section, we discuss three types of approaches to conducting a random assignment evaluation of ARRA grantees. For all approaches, we assume that study sample members at a participating grantee site will be assigned to either a treatment group or a control group.[26] The approaches are (1) randomly assigning all individuals at all grantees; (2) randomly selecting a subset of grantees and then randomly assigning all individuals; and (3) focusing on a nonrandom subset of grantees and then randomly assigning all individuals.

1. Randomly Assign All Individuals at All Grantees

One possible approach for conducting a random assignment study of the ARRA grants would be to include all grant participants in the evaluation. This approach has considerable strengths and weaknesses to it. Its main strength is its ability to provide answers to questions about the impacts of the grants nationwide, with the most statistical power of any of the approaches. Furthermore, if control group assignment rates are high enough, it might be possible to provide answers to questions about impacts for specific sets of grantees with a high degree of statistical precision.

However, the scope of the grants in terms of both the number of participants to be served and their geographic locations means that a study that includes all grantee participants would be

[25] If a follow-up survey is to be conducted, identifying and contact information collected at the point of random assignment can help in the process of locating participants for the survey.

[26] A refinement of these approaches would involve the subsampling of grantee participants at a study site to determine inclusion in the evaluation. We do not explore this issue in this paper, given its complexity and the breadth of approaches that are discussed.

extremely expensive. Although the precise target numbers of participants that grantees expect to serve is not currently available, our calculations based on available information suggest that the numbers are in the tens of thousands for each grant type. The 25 ETP grantees are likely to strive to serve at least 34,000 participants; the 38 Pathways and 34 SESP grantees are likely to strive to serve at least 20,000 participants and 54,000 participants, respectively. Furthermore, the geographic coverage of the grants is quite large, a factor that would add to the cost of this type of comprehensive approach to random assignment. The ETP, Pathways, and SESP grants are *each* expected to operate in at least 29 states or other jurisdictions, such as the District of Columbia or Puerto Rico. Thus, conducting random assignment at all grantees (for even one of the grant types) would likely require setting up random assignment in hundreds of sites, training staff and monitoring procedures at these sites, processing tens of thousands of forms to collect baseline data, and collecting administrative data from numerous agencies if this type of data are used for measuring participants' outcomes.

Because of the great cost of including all grantees from one or more grant types in an evaluation, we do not explore this approach in greater detail in the rest of this chapter.

2. Randomly Selecting a Subset of Grantees and then Randomly Assigning Individuals at Them

One way to mitigate the cost disadvantages of the first approach to random assignment, while preserving some of its' advantages, is to randomly select a subset of grantees for inclusion in the study and then randomly assigning sample members at those study sites only. Through the random selection of sites, this approach—like the first approach—will be able to answer questions about the impacts of the grants nationwide. Importantly, focusing on a subsample of grantees could reduce the heavy cost burden associated with random assignment of sample members at all grantees. In addition, it might still be possible to obtain impact estimates for subgroups by stratifying the grantees along dimensions of interest prior to random selection.

However, a drawback of this approach is that it introduces a clustering effect, which reduces the precision of the impact estimates compared to a situation in which all grantees or a purposive selection of grantees are included in the study. Furthermore, given the variance across grantees in the per-grantee number of participants to be served, this approach runs the risk of a "bad draw" leading to the random selection of a set of grantees with very small target numbers of participants. Stratifying the grantees by size before random sampling and/or sampling sites proportional to size would help to avoid this problem and improve statistical power without introducing large design effects.

3. Focusing on a Nonrandom Subset of Grantees and then Randomly Assigning Individuals at Them

An alternative approach to random selection of grantees is to select intentionally a subgroup of grantees on which to focus the evaluation. After doing so, study sample members would be located at these sites only and—as with the other approaches—would be randomly assigned to either the treatment or the control group.

Purposeful selection of grantees could be based on any of a number of policy-related or logistical reasons. For example, policymakers might want to focus an evaluation on a subset of ETP or SESP grantees that are serving communities affected by auto restructuring. Or, they might be

particularly interested in looking at the Health Care and Other High-Growth and Emerging Industries grants that are focusing on the health care industry only. In addition, policymakers might be interested in the subset of grantees that plan to serve a particular type of target group, such as dislocated workers or high school dropouts, or that plan to use a particular service delivery method, such as online learning or apprenticeship programs. Logistical considerations, such as the timing of when grantees are expected to serve participants or a grantee's conduciveness to participating in the evaluation, might also be important considerations for selecting grantees.

Compared with the approach of randomly sampling grantees from among all grantees, this approach provides more statistical power because uncertainty due to the site sampling is not introduced into the study design. Furthermore, to the extent that policymakers have special interest in some grantees more than others, this approach would enable the evaluation to focus on the grantees of interest, although the ability to generalize to other grantees would be lost. (In technical terms, this approach would "lose external validity.") Conclusions from the study would pertain to those grantees only, and not all grantees. Finally, it also might involve a smaller investment in both time and resources to secure the cooperation of sites that have been purposefully selected, compared with an approach of random selection of grantees, even if grantees are contractually required to participate in this type of study.

C. Details of the Random Assignment Process

A thorough consideration of the details of conducting the random assignment process at study sites is necessary for an experimental evaluation of grant services to be successful. These details include the point at which random assignment should be inserted in the flow of participant intake, the methods for the collection of baseline data and the assignment of individuals to treatment and control groups, monitoring of random assignment procedures, and restriction of control group members' access to services.

Although a comprehensive investigation of these issues requires more information about the grantees' plans than is currently available, we discuss two considerations of primary importance for the design of a random assignment evaluation of the ARRA grants: (1) the point at which to insert random assignment and (2) restriction of control group members' access to services.

1. The Point at Which to Insert Random Assignment

A very stylized and simplistic model of an intake and random assignment process begins with an individual's expression of interest in receiving intervention services. After that, their eligibility for services is determined. Those who are not eligible for services are excluded from the study. Those who are eligible are asked to provide baseline data, which is used to further confirm the individual's eligibility. Only after that step does the individual go through random assignment. However, our experience in conducting random assignment evaluations for DOL and other clients suggests that many details of this process need to be considered before a random assignment study can be successful.

In this section, we focus on a few important issues related to the intake and random assignment process for grant services. We begin by describing potential implications for random assignment of the number of locations that could serve as intake points. We next focus on implications of the types of locations. Finally, we discuss issues regarding how the point of random assignment might influence the ability to estimate the impacts of the grants on participants' outcomes.

An important requirement of the random assignment process is that random assignment occurs at all places where grant participants can go through the intake process for grant services. This is so that there is no circumvention of the process.[27] Grantees of the four ARRA grant types are likely to have a different number of locations through which intake can occur. Two of the SESP grantees, for example, plan to provide services in more than 40 counties in their states. In contrast, some of the Pathways Out of Poverty grants plan to serve one or a few communities within a city. A wide geographic dispersion of grant services will likely complicate and increase the expense of the training of staff on random assignment and the monitoring of the procedures.

In addition to the number of locations, the types of locations through which intake could occur is important, since random assignment procedures will need to be implemented at each type of location. The range in types of locations at which intake could occur is likely to vary across the four grant types since the four SGAs had different requirements about the types of partners that had to be included in grant efforts. Although many grantees will likely recruit at least some of their participants through the One-Stop Career Center system, they might differ in the other sources from which participants are recruited. For example, compared to the Pathways Out of Poverty grantees, some ETP, SESP, and Health Care grantees might be more likely to obtain participants through their partnerships with employer and union organizations, since these grants have more of an emphasis on serving incumbent workers. These cases might warrant special considerations or procedures because individuals assigned to the treatment and control groups might work in close proximity to each other or come in contact with each other in other ways. In contrast, the Pathways Out of Poverty grant participants might not be as closely affiliated with each other through official organizational ties, which might influence the way in which the procedures for random assignment can be designed.

Finally, the four different grant types might differ from each other in terms of expected impacts of the intervention, given a particular point of random assignment. If an evaluation's primary research question pertains to the impacts of access to any grant-funded services, a natural point for random assignment would be as soon as an individual is found eligible for grant-funded services and before receipt of those services. Because of the grants' focus on the provision of training, many grantees are likely to place individuals in grant-funded training immediately upon establishing eligibility. However, in the case of the Pathways grants especially, the first type of grant-funded service might be something besides training. Because Pathways grantees are more likely than other ARRA grantees to serve participants with low levels of basic skills and less attachment to employment, their participants might be less likely to receive any services, including training services, even when they are assigned to the treatment group during a random assignment study. While this expected pattern might or might not influence a decision about where in the flow of intake random assignment should be inserted, it would be important for an evaluation to take into account the influence of this potential dilution of intervention services on impact estimates. For a given evaluation sample size, a higher portion of treatment group members who do not receive grant services leads to a lower ability to detect meaningful impacts.

[27] As discussed in both Sections B and D, DOL could choose to conduct a random assignment study that does not include all grant participants. However, the point here is that any exclusion of some grant participants must be an intentional, carefully-assessed part of the design, rather than an omission based on poor execution of the design.

2. Restriction of Control Group Members' Access to Services

Another consideration related to the details of the random assignment process pertains to the types of services that might be denied to control group members. Again, the issues here are complex and warrant further investigation during the design phase of a random assignment evaluation. But, we raise two issues for further investigation.

First, the evaluation would need to determine whether control group members, who would be denied access to training provided directly through ARRA funds, would be allowed to use non-ARRA funds to participate in training that only exists as a result of ARRA grant-funded activities. Under some circumstances, DOL is allowing grantees to use grant funding for (1) the development or modification of training curricula, (2) the education or training of instructors who will provide training, and/or (3) the development or modification of standards and procedures for defining and issuing credentials. Thus, these ARRA-funded activities could have a broad influence on the types of training that are available in a grant community.

Second, the evaluation of the ARRA grants will need to determine whether control group members are prevented from accessing any and all services funded through other ARRA grants deployed in the same local area. DOL imposed few restrictions upon whether organizations could apply for or participate in more than one of the four ARRA grants discussed in this pre-design study. One restriction was that a state or local ETP grant recipient could not receive a subgrant or subcontract from an ETP national labor-management grant recipient. Another was that a Pathways grant recipient could not receive an ETP grant, although it may participate as a partner in an ETP grant. Given the limited scope of these restrictions, and in some cases the breadth of geographic coverage of the grants, it is likely that more than one ARRA grantee (from either the same or different grant types) will provide services to a particular community included in an evaluation. As noted above, some of the SESP grants include plans to cover a wide geographic area in a state. But, even for more narrowly focused grants, such as two local Pathways grants serving St. Louis, Missouri, there is overlap in the PUMAs in which the grant recipients are targeting.

D. Scheduling Considerations that Influence All of the Random Assignment Designs

As discussed in Chapter II, three of the four types of ARRA grants were awarded in January 2010, and grantees are currently developing their plans to begin enrolling and serving participants. Although DOL is eager to begin an evaluation of the ARRA grants and is striving to do so as quickly as possible, an important factor that influences the feasibility or desirability of different types of evaluation methodologies pertains to the schedule of both the grants and any evaluation that DOL undertakes. Issues related to the timing of grant and evaluation activities are particularly important for a random assignment design, because—compared with other types of evaluation methodologies—development and implementation of this type of design is especially time-consuming.

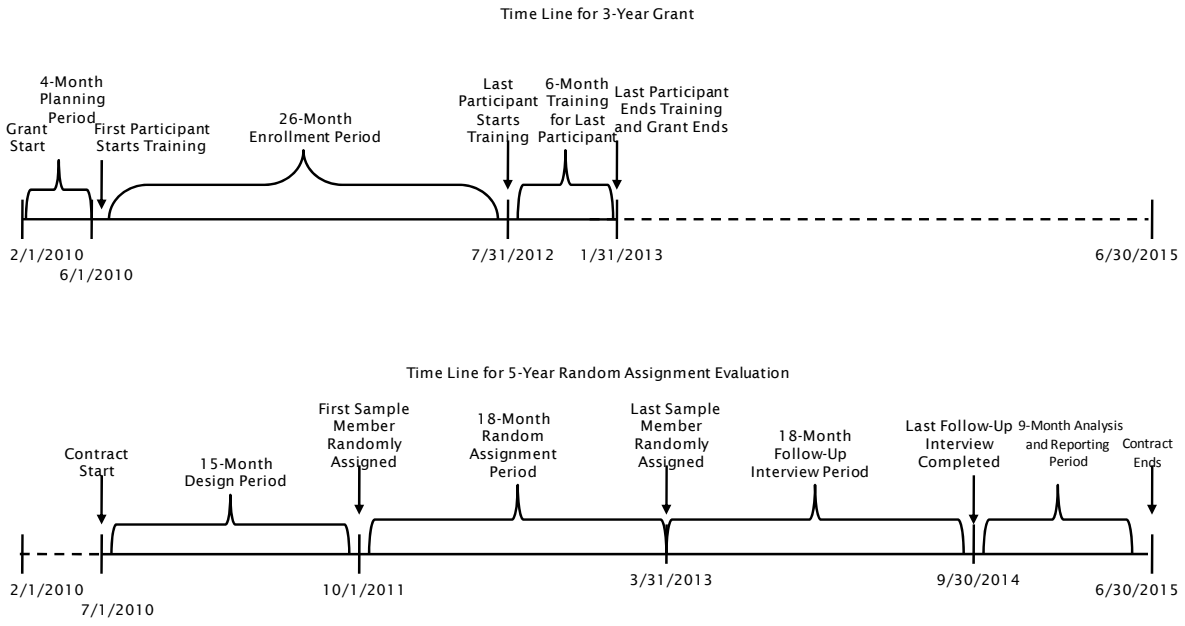
In this section, we discuss issues related to the timing of the grants and a random assignment evaluation. After specifying the assumptions that we have made for our analysis, we discuss two implications. The first implication is that, under certain assumptions, plausible time schedules for the grants and evaluations are likely to reduce considerably the number of clients who could be part of a random assignment evaluation of the three-year grants and might even make random assignment infeasible for the two-year grants. The second is that, under the same assumptions, it is

likely that an evaluation time schedule can include a sufficiently long follow-up period to measure outcomes of interest to DOL.

1. Assumptions About the Time Lines of the Grant and Evaluation Activities

Figure IV.2 presents two hypothetical time lines: one for the three-year grants and one for the evaluation. For the purposes of our analysis of the time lines for the grants and an evaluation, we have made four assumptions about the grant activities. First, we have assumed the grants are three-year grants (as is the case for the SESP and Health Care grants) and start on February 1, 2010. We have chosen this start date for illustrative purposes, but a start date that is earlier by a few weeks would not greatly influence the points discussed here.[28]

Figure IV.2. Hypothetical Time Lines for 3-Year Grant and 5-Year Random Assignment Evaluation



[28] DOL announced the award of the ETP grants on January 6, 2010; the Pathways Out of Poverty grants on January 13, 2010, and the SESP grants on January 20, 2010.

Second, we have assumed that grantees take four months for planning prior to enrollment of the first participant. This time will be used for start-up activities, such as hiring of staff, coordination among grant partners, and—in some cases—the development or modification of training curricula or certification standards. In Figure IV.2, the left side of the time line for the grants shows that, given a start date of February 1, 2010, and a four-month planning period, the grantees will be ready to enroll their first clients around June 1, 2010.

Third, we have assumed that the typical length of training is six months. The Individual Training Account (ITA) experiment found that, at the 15-month follow-up interview, the average number of weeks that participants spent in training within five quarters after random assignment was between 25 and 29, although 20 to 25 percent of the ITA sample members were still in training at the time of the follow-up interview. The lack of full follow-ups for the ITA sample members suggests that the average length of training funded by the ARRA grants might be longer, but a mitigating factor is the duration of the grants. A better estimate of the length of training is likely to depend on the details of the grantees' plans.

Finally, we have assumed that grantees will complete their participant enrollment in time for the last enrolled participants to complete their training at the end of the grant period. Implicitly, this means that the last enrollments will occur six months prior to the end of the grant period, because we have assumed that the average length of training is six months. This also means that, for the last enrolled participants, any post-training services that the grantee or its partners provide, as well as any follow-up data collection, will need to be funded through sources other than the ARRA grants.[29] As a result of this assumption, the right side of the grants time line in Figure IV.2 shows that, given a contract end date of January 31, 2013, the grantees would need the last clients to start training around July 31, 2012.

For the evaluation schedule, we have made three key assumptions. The first is that the evaluation starts on July 1, 2010, and is a five-year contract.

The second assumption is that it takes 15 months from when the contract starts until the first sample member goes through the random assignment process. Between the time an evaluation contract is awarded and when random assignment of participants can begin, several steps must be completed, including the development of the evaluation design, baseline and consent forms, and the database that will be used to conduct random assignment, as well as the Office of Management and Budget (OMB) approval process. This process will include the submission of a draft OMB package for DOL's review and revisions to the draft based on DOL's feedback; the publication of and waiting period required with a *Federal Register* notice; and the period during which OMB reviews the package and the package is revised before approval is given. Typically, the time between the initial submission of the draft package to DOL for review and the receipt of clearance is six to nine months. With a start date of July 1, 2010, for an evaluation contract and a 15-month design period prior to the start of random assignment, the first sample member could go through random assignment on October 1, 2011 (Figure IV.2).

[29] Based on email communication with DOL on January 26, 2010, DOL expects that some participants might still be enrolled in training when a grant ends. In addition to participant follow-up, which grantees are expected to do throughout the grant period of performance, DOL will be able to calculate the "common measures" outcomes for participants through the use of unemployment insurance (UI) wage records data.

Based on our prior experience, allowing 15 months for the design phase of a random assignment evaluation would be a tight schedule, but it could be feasible. For example, Mathematica's ITA and Job Corps experiments included an 18- to 19-month time frame and a 16- to 17-month time frame, respectively, before random assignment began. However, it is possible that an evaluation of the ARRA grants might not need as long a pre-random-assignment design period as these two studies did. In the case of the ITA study, a considerable amount of time was spent designing the three categories of treatment services that would be compared to each other; it is unlikely that the need to develop and define the intervention exists to this extent with ARRA grants.

For Job Corps, considerable time was needed because the applicants of many locations nationwide were to go through random assignment. If DOL were to choose either the second or the third approaches to random assignment (described earlier) for an evaluation of the ARRA grants, then the evaluation would take place in a much smaller number of locations than was the case for the Job Corps study. Furthermore, to the extent that ARRA grantees agreed as a condition of grant receipt that they might be required to participate in a random assignment study, the evaluation of these grants might not need as much time as is often required to convince grantees or sites to participate.

Our third assumption is that the evaluation needs nine months before the contract end date for the analysis of data, preparation of the draft final report, and revision of the report in response to DOL's comments.

Of course, changes in these assumptions about the grant or evaluation time lines will change the conclusions of this analysis. For example, grantees might begin enrolling clients at a slower pace than we have assumed if they spend a considerable amount of time developing training curricula before enrollments begin. DOL might be able to have the evaluation started sooner than July 1, 2010, and it might desire a longer or shorter evaluation period. Furthermore, should DOL choose to use a random assignment design that purposively selects grantees (that is, the third of the random assignment approaches discussed in Section B), part of the selection process could include consideration of grantees' time lines for serving participants.

Although our analysis of grant and evaluation time lines is inherently dependent on the assumptions described here, and therefore inherently subject to imprecision, the analysis can serve as a starting point for thinking about two key issues: (1) the portion of the grant participants that could be included in a random assignment study and (2) the length of the follow-up period in the evaluation. We discuss these issues in the next two subsections.

2. Implications about the Portion of the Grantees' Clients Who Could Participate in Random Assignment

Based on the assumptions about the grants time line, it would be possible for the grantees to have a 26-month enrollment period, from June 1, 2010, to July 31, 2012. The hypothetical evaluation time line (shown as the bottom time line in Figure IV.2) indicates that this 26-month enrollment period overlaps only partly with a plausible random assignment period. The October 1, 2011, start date for random assignment is 16 months into the grantees' 26-month enrollment period, leaving only a 10-month period of overlap between the grantees' enrollment and the evaluation's random assignment time window. The overlap is for 38 percent (= 10/26) of the grantees' enrollment period.

If grantees enroll clients at a steady pace, then only about 38 percent of their clients would be subject to random assignment. Based on our observations of grant enrollments for other studies, it is often the case that grantees have a slow start-up period. If that is the case for the ARRA grants, then it is possible that a higher percentage of clients would enroll after the start of random assignment. However, because grantees are likely to be eager to distribute grant funds, especially given the current high unemployment rates, they might have a strong incentive to enroll participants as quickly as possible. Grantees that are successful in responding to this incentive might enroll much more than 62 percent of their clients prior to the start of random assignment, with considerably fewer than 38 percent available for random assignment.

Although the Health Care and SESP grants are for three years, the ETP and Pathways grants are for two years. For these grants, the hypothetical time line would look very similar to that shown in Figure IV.2, except that the grant end date and the last client's enrollment date would need to be one year earlier. This suggests that the grants would need to enroll clients in about 14 months—not 26 months—and enrollment would end around July 31, 2011, which is prior to when random assignment could be expected to start. In order for there to be overlap between the grantees' enrollment period and the evaluation's random assignment period, one or more of the assumptions about the time lines would need to be adjusted. For example, a four-month overlap could be possible if (1) the evaluation start-up period is 13 months instead of 15 months and (2) the average length of training is two months instead of six months. In this case, random assignment would start August 1, 2011, and enrollment would end November 30, 2011; the four-month overlap would allow 24 percent (= 4/17) of the participants to go through random assignment.

For both the two- and three-year grants, the percentage of grant participants who can be included in the random assignment process will have a strong influence on the MDIs that can be detected through a random assignment evaluation. We discuss this issue further in Section E.

3. Implications about the Follow-Up Period for the Observation of Post-Training Outcomes

The hypothetical grant and evaluation time lines also indicate that a random assignment evaluation of three-year grants would have an ample follow-up period. Given an evaluation contract end date of June 30, 2015, and the need for nine months for the analysis and reporting periods, the last follow-up data collection would need to occur by September 30, 2014 (Figure IV.2). For administrative records data, such as UI wage records, this would be the date by which the data would need to be provided to the evaluator. (UI wage records data extracts provided by this date would probably contain information through June 30, 2014, given the time lag necessary for states to provide an evaluator with the data after processing employers' reports of their employees' earnings.) With the last clients of a three-year grantee having started training around July 31, 2012, the schedules would allow a follow-up period from July 31, 2012, to September 30, 2014 (26 months for interviews and probably about 23 months for administrative data).

If DOL wanted to include as part of an evaluation a follow-up survey to measure participants' post-training outcomes, September 30, 2014, would be the latest feasible date for the last follow-up survey to be completed. However, it appears there would be adequate time in the evaluation schedule to include this type of follow-up survey. Determining the best length of time after random assignment for a follow-up survey to be conducted involves balancing two often-competing factors. One is the desire to ensure that the follow-up period is sufficiently long so that participants could complete their activities (such as grant-funded training). The other is ensuring that the survey is not

so long after the activities and outcomes of interest occur that survey respondents have difficulty recalling information accurately.[30] Some evaluations mitigate the tension between these factors by including two follow-up surveys, one of which collects information on short-term outcomes and the other of which collects information about longer-term outcomes. However, this approach adds to the cost of the evaluation compared with use of a single follow-up survey. In the case of an evaluation of the ARRA grants, it is likely that DOL would have flexibility for determining how to balance these issues to best meet its needs for either short-term or long-term outcomes information. For illustrative purposes in Figure IV.2, we have shown an 18-month random assignment period and an equally long follow-up interview period. However, these lengths could be adjusted based on DOL's preferences for how long the follow-up period should be.

E. Example Minimum Detectible Impacts for Random Assignment Studies of the ARRA Grants

Should DOL choose to use a random assignment design for an evaluation of the ARRA grants, it will be important to ensure that the design selected will yield adequate levels of precision for the estimation of program impacts on participants' outcomes. As explained in Section B, an MDI is the smallest true impact for which there is a high probability of detection (typically, assumed as an 80 percent probability); the smaller the MDI, the greater the statistical power of the design. For example, an MDI of 7.5 percentage points in the probability of employment means that if the employment rate for the treatment group is at least 7.5 percentage points higher than that of the control group, then the study will be able to detect this difference with a high probability.

To provide guidance to DOL about the statistical precision that might be expected under different approaches to random assignment, and to assess the tradeoffs involved, we present a set of illustrative MDI calculations under different scenarios. Although this analysis provides several insights for planning an evaluation of the ARRA grants, we caution that making a sound decision about proceeding with a particular evaluation design would require more precise analysis, based on additional information and further investigation.

For all scenarios, we have calculated MDIs separately for the three types of grants (ETP, Pathways, and SESP) awarded as of the end of January 2010 using publicly available information about the target number of participants that grantees plan to serve.[31] We have not provided MDI calculations for the Health Care grants because information about the grant winners was not available for inclusion in this analysis. We begin by explaining the types of variation that we take into account in the MDI calculations. Then, we discuss patterns in the MDIs that have implications for a random assignment evaluation design.

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[30] For example, Mathematica's original ITA study used a 15-month follow-up period. Although this length was adequate for observing the post-training outcomes of many sample members, DOL subsequently contracted with Mathematica to conduct a longer-term follow-up study, in part because some sample members were still in training at the time of the 15-month follow-up interview.

[31] As explained in Chapter II, the publicly available information on grant winners' plans is limited. In some cases, we cannot tell the target number of participants to be served by grantees, because this information is unavailable or unclear. In these cases, we have made assumptions that facilitate our analysis.

1. Types of Variation Taken into Account in the MDI Calculations

We have calculated the MDIs for a range of different circumstances to illustrate how they might vary based on different assumptions and strategies that DOL might want to use in an evaluation of the ARRA grants. In this section, we discuss the dimensions that we have allowed to vary.

a. The Random Assignment Approach that Is Used

In Section B, we presented three possible approaches for conducting a random assignment evaluation. The first (Approach # 1) includes all grantees in the evaluation. The second (Approach # 2) involves a random sampling of a subset of grantees to yield impact estimates that are representative of all grantees. The third (Approach # 3) involves purposeful selection of a subset of grantees. In all three approaches, individuals at study grantee sites are randomly assigned to either a treatment or control group. We have restricted our MDI analysis to Approaches # 2 and # 3 because cost considerations are likely to make Approach # 1 very unappealing to DOL.

b. The Percentage of the Evaluation Sample Members that Are Assigned to the Control Group

As discussed in Section A, randomly assigning fewer than half of all study sample members to the control group can make participation in a random assignment study more appealing to grantees, because they have to deny services to fewer individuals deemed eligible for grant services. However, a 50–50 treatment-control group assignment rate provides the greatest degree of statistical precision for a given sample size. We have calculated MDIs based on two scenarios: The first is that half of sample members (50 percent) are assigned to the control group; the second is that one-quarter (25 percent) are assigned to the control group.

c. The Percentage of Grantees' Participants that Can Be Included in Random Assignment

As explained in Section D, timing issues related to when the grantees are likely to begin serving participants compared with when an evaluation is likely to be able to start random assignment might dramatically reduce the portion of participants that could be included in this type of evaluation. Based on the time line analysis in Section D, we have calculated MDIs under different assumptions about the percentage of grant participants that can be included in random assignment. For the three-year SESP grants, we have assumed that either 30 percent or 40 percent of participants are included. For the two-year ETP and Pathways grants, we have assumed participation rates of either 10 or 20 percent.

d. The Source of the Outcome Data

Two sources of outcome data that are frequently used in evaluations of training and employment services initiatives are follow-up surveys and UI administrative wage records (according to the SGAs, DOL has already expressed interest in using the latter for the ARRA grants). The source of the data is important because of differential rates of availability of the data for the impact analysis. For this study, we have assumed there will be an 80 percent response rate to a survey of

5,000 sample members and a 100 percent "response rate" for the administrative records data (that is, all sample members will have administrative records data).[32]

e. The Number and Types of Grantees that Are Included

DOL has expressed interest in learning about how sensitive the MDIs are to the number of grantees that might be included in a random assignment evaluation. The ETP grantees can be distinguished on the basis of whether they include auto restructuring communities and whether they are national or local grantees. The Pathways grantees also can be distinguished on the basis of whether they are national or local grantees. The SESP grantees can be distinguished on the basis of whether they include auto restructuring communities. Based on the number of types of grantees, we have devised several different subsets of grantees as possible candidates for consideration for an evaluation. However, we recognize that the subsets that we have examined are not exhaustive. In order to derive hypothetical sample sizes for the calculations, we generated an estimate of the average grantee size by grant type and subtype of interest (such as grantees serving auto restructuring communities for ETP grants) using publicly available reports of the number of participants each grantee intended to serve.

f. Factors Not Taken into Account

For our MDI calculations, we have not taken into account two factors that may prove important for further consideration. First, we have not adjusted our MDIs for the higher likelihood of detecting statistically significant, but spurious, impacts when more than one outcome of interest is examined. Adjusting for this increased likelihood would increase the MDIs.

Second, we also have not taken into account the possibility of varying the number of sample members that are included from each study grantee. For example, instead of assigning all eligible individuals who express interest in receiving grant services to either a treatment or control group, one could randomly assign them to one of three categories: a treatment group, a control group, and a group that is not part of the study. Data for individuals assigned to the third group would not be collected or analyzed. The desirability of this approach would depend on cost considerations, the total number of potential sample members given participants' enrollment patterns over time (as discussed in Section D), and the effect of the third group on statistical power. However, we do not explore this option because Section D indicates that, even with two groups only, the potential sample sizes for an evaluation might be considerably smaller than the total number of participants that grantees will serve.

Another way to vary the number of sample members who are included in the study would be to subsample sample members at study grantees based on the communities in which the sample members are located: for grantees that plan to serve more than one community, one could randomly sample which community or communities are to be included in the evaluation. This approach has the potential to reduce evaluation costs when study grantees plan to serve a wide

[32] More precisely, we assumed a survey sample size of the lesser of 2,500 sample members in each of the treatment and control groups and the number available based on our other assumptions. The number of sample members in each group was not necessarily the same for a given set of assumptions. In addition, for administrative data analysis, we have used the typical assumption that sample members for whom administrative records data are not found have "records" of zero earnings. Thus, we have assumed that all sample members have records even if a match is not found between his or her Social Security number and the administrative records data files.

geographic area. However, examining this issue is not feasible at this point, given the number of options that are already explored in this analysis and the information that is currently available about the grantees. A design that uses subsampling within grantees also would increase the MDIs.

2. Example MDIs Based on Different Random Assignment Scenarios and Assumptions

We begin our presentation of illustrative MDIs using the ETP grants. Table IV.1 shows a range of MDIs based on variations in the dimensions discussed earlier, such as the percentage of sample members that are assigned to the control group and the percentage of grant participants that are included in the study sample. For example, as shown in the first row of numbers in Table IV.1, an evaluation would be able to detect a $181 impact on average quarterly earnings, under the following assumptions: (1) the 16 auto restructuring grantees are included in the study, (2) they are purposefully selected (Approach # 3), (3) 10 percent of grant participants are part of the random assignment process, (4) survey data are used as the source of the outcome, and (5) 50 percent of sample members are assigned to the treatment group and 50 percent to the control group.

Because of the many different assumptions and scenarios for which we calculate MDIs, it is hard to draw an overarching conclusion about whether the MDIs are "big" or "small" compared to what might be desirable for an evaluation of the ARRA grants. However, having in mind some MDIs from other studies that Mathematica has conducted for DOL might be useful for assessing whether these example MDIs for the ARRA grants are, relatively speaking, big or small.[33] For example, for the current impact evaluation of the WIA program that Mathematica is conducting, we calculated MDIs using samples of adult and dislocated workers (Bellotti et al. 2009). For the full sample, the MDIs were reported as $157 using survey data and $123 using administrative data. For a subgroup of 50 percent of sites, the MDIs were reported as $195 and $153 based on survey and administrative data, respectively. For a study of the Trade Adjustment Assistance Program, for which Mathematica currently is serving as a subcontractor, we estimated MDIs of $242 and $122 in quarterly earnings for the full sample using survey data and administrative data, respectively (Schochet 2005). Finally, for a study of a program designed to promote entrepreneurship, for which Mathematica served as a subcontractor, the evaluation design report showed MDI estimates of $1,326 for annual earnings from any job (= $332 per quarter) for a sample of 2,000 treatment and 2,000 control group members (McConnell et al. 2004). In practice, a slightly larger number of participants (4,198) went through the random assignment process than was reflected in the MDI calculations, which means that the actual MDIs might have been slightly smaller than those reported in the design report (Benus et al. 2008). Despite the perspective that the MDIs from other studies gives for assessing whether the example ARRA grant MDIs are big or small, a final assessment of whether or not an MDI for a particular design is adequate would depend on the hypothetical impact to be expected.

[33] The interventions planned for the ARRA grants differ from the interventions that have been or are being assessed through the other studies cited here. Therefore, one might have different expectations about the impacts that are induced by the different programs. For example, a program that is very costly and intensive might be more likely to generate large impacts than one that is less costly and less intensive. Thus, DOL might deem acceptable an MDI for one study but not for another study.

Table IV.1. Minimum **Detectible** Impacts on the Percentage Employed and Average Quarterly Earnings, Energy Training Partnership Grants

	Random Assignment Approach # 2 (Randomly Subsampling Grantees) [Survey, Administrative Data]		Random Assignment Approach # 3 (Purposefully Selecting Grantees) [Survey, Administrative Data]	
	Percentage Employed	Average Quarterly Earnings ($)	Percentage Employed	Average Quarterly Earnings ($)
50–50 Treatment-Control Group Assignment Rate				
10% of Grant Participants Included in Random Assignment				
The 16 auto restructuring grantees[a]	n.a.	n.a.	.069, .062	181, 162
10 grantees (5 national and 5 local)	.091, .085	238, 221	.077, .069	202, 180
All 7 national grantees	n.a.	n.a.	.090, .08	235, 210
5 national grantees	.125, .114	325, 298	.114, .102	298, 266
All 18 local grantees	n.a.	n.a.	.059, .053	155, 140
10 local grantees	.094, .086	244, 224	.084, .075	219, 197
5 local grantees	.158, .147	412, 383	.133, .119	349, 312
20% of Grant Participants Included in Random Assignment				
The 16 auto restructuring grantees[a]	n.a.	n.a.	.049, .044	128, 115
10 grantees (5 national and 5 local)	.074, .070	192, 182	.054, .049	143, 128
All 7 national grantees	n.a.	n.a.	.063, .057	166, 149
5 national grantees	.097, .090	252, 234	.080, .072	210, 188
All 18 local grantees	n.a.	n.a.	.042, .038	110, 98
10 local grantees	.073, .068	190, 177	.059, .053	155, 139
5 local grantees	.128, .121	333, 316	.094, .084	247, 221
75–25 Treatment-Control Group Assignment Rate				
10% of Grant Participants Included in Random Assignment				
The 16 auto restructuring grantees[a]	n.a.	n.a.	.080, .071	209, 187
10 grantees (5 national and 5 local)	.101, .093	264, 243	.089, .080	233, 208
All 7 national grantees	n.a.	n.a.	.104, .093	271, 243
5 national grantees	.141, .128	366, 334	.132, .118	344, 307
All 18 local grantees	n.a.	n.a.	.069, .061	180, 161
10 local grantees	.105, .096	274, 250	.098, .087	254, 227
5 local grantees	.175, .162	457, 421	.155, .138	403, 360

Table IV.1 *(continued)*

	Random Assignment Approach # 2 (Randomly Subsampling Grantees) [Survey, Administrative Data]		Random Assignment Approach # 3 (Purposefully Selecting Grantees) [Survey, Administrative Data]	
	Percentage Employed	Average Quarterly Earnings ($)	Percentage Employed	Average Quarterly Earnings ($)
20% of Grant Participants Included in Random Assignment				
The 16 auto restructuring grantees[a]	n.a.	n.a.	.057, .051	149, 132
10 grantees (5 national and 5 local)	.08, .075	209, 196	.063, .057	165, 147
All 7 national grantees	n.a.	n.a.	.074, .066	192, 171
5 national grantees	.107, .099	278, 257	.093, .083	243, 217
All 18 local grantees	n.a.	n.a.	.051, .043	132, 114
10 local grantees	.080, .075	209, 194	.069, .061	180, 160
5 local grantees	.139, .130	362, 339	.109, .097	285, 255

Source: Author's calculations based on publicly available information about grantees' plans for serving participants.

Note: Because of the likelihood that the cost of conducting an evaluation using Random Assignment Approach # 1 would be prohibitive, we do not include in the table the MDIs using this approach.

Survey sample sizes assumed in calculations are the lesser of 2,500 or the total number of expected sample members in each group.

The MDI formula used for the calculations in Approach 2 is as follows:

$$factor \times \sigma \sqrt{\rho(1-R^2_{site})(1-c)\left(\frac{2\times FPC}{\#\,sites}\right)+(1-\rho)(1-R^2_{ind})\left(\frac{1}{r\times N_T}+\frac{1}{r\times N_c}\right)}$$

where σ is the standard deviation of quarterly earnings ($1,250) or employment status (0.48) based on results from previous similar studies, *FPC* is the finite population correction, *r* is the response rate (0.80 for the survey, 1.00 for administrative records), R^2 is 0.20 both at the site and individual levels, the intraclass correlation ρ is 0.04, the correlation of treatment and control groups within sites *c* is 0.70, N_T and N_C are pertinent sample sizes for treatment and control groups, and *#sites* is the total number of sites selected in the approach being considered. The MDI calculations assume two-tailed tests. We assume 80 percent power and a 5 percent significance level. *factor* varies as a function of the number of sites being considered, but generally falls in the range of 2.8 to 3.19. Using variables that are defined in a similar way and with similar values, the MDI formula used for the calculations in Approach 3 is as follows:

Table IV.1 *(continued)*

$$factor \times \sigma \sqrt{(1 - R_{ind}^2)\left(\frac{1}{r \times N_T} + \frac{1}{r \times N_c}\right)}.$$

[a]A grantee is designated as an auto restructuring grantee if it has any component that is targeted to an auto restructuring community. Not all grantees that plan to serve a community affected by auto restructuring will devote all of their grant resources to these communities.

MDI = minimum detectable impact.

n.a. = not applicable.

Across the wide range of design strategies that we considered, several general patterns in the MDIs emerge. For simplicity, we focus our discussion on the average quarterly earnings as an outcome using survey data, although the same patterns are found using the percentage employed and/or administrative data.

First, including a larger number of grantees greatly helps to reduce the MDIs. This finding is to be expected given the influence of the clustering of the sample on the MDIs. With random assignment Approach # 3, a 10 percent rate of inclusion of grant participants and a 50–50 treatment-control assignment rate, for example, an increase of the number of local grantees from 5 to 10 decreases the MDI from $349 to $219. A further increase in the number of local grantees to 18 decreases the MDI further, to $155. In a similar way, under the same assumptions, an increase in the number of national grantees from 5 to 7 leads to a decrease in the MDI from $298 to $235.

Second, increasing the percentage of the grant participants that are included in the study reduces the MDIs, but not as much as does increasing the number of grantees. Take the scenario in which 5 local grantees and 10 percent of grant participants at these grantees are included in the study. As noted earlier, the average quarterly earnings MDI is $349. One way to double the sample size would be to include 10 grantees rather than 5. Another way would be to include 20 percent of grant participants rather than 10 percent. The first approach—doubling the number of grantees—drops the MDI to $219. In contrast, doubling the percent of participants who are included from among the 5 grantees would reduce the MDI to $247. All else equal, therefore, inclusion of more grantees is likely to be a better approach for improving the precision of the impact estimates than would be increasing the sample size within grantees. (Of course, cost and other considerations might need to be taken into account, because increasing the number of grantees will be a more costly approach.)

Third, for a given number of grantees, the MDIs are lower when national grantees are included instead of local grantees.[34] For example, using Approach # 3, with a 50-50 treatment control group assignment rate and 10 percent of grant participants included in the evaluation, the MDI for average quarterly earnings is $298 when 5 national grantees are selected and $349 when 5 local grantees are selected. This is because, based on the information available through DOL's announcement of grant winners, national grantees are larger on average than are local ones.

Fourth, Approach # 2 yields larger MDIs than does Approach # 3 for a given number or type of grantees. For example, a 10 percent rate of inclusion of grant participants and a 50–50 treatment-control assignment rate, random selection of 10 grantees (5 national and 5 local) through Approach # 2 yields an MDI of $238. Purposeful selection of the same number and type of grantees through Approach # 3 yields a $202 MDI. This pattern occurs because Approach # 2 introduces an additional level of statistical uncertainty in the analysis compared with Approach # 3.

Fifth, as expected, assigning only 25 percent of the sample members to the control group increases the MDIs, although in most cases the increase is not dramatic. A 75–25 treatment-control assignment rate, in which 10 grantees (5 national and 5 local) and 10 percent of grant participants are

[34] Because we are not taking into account different weighting or stratification schemes that could be employed when selecting a mix of national and local grantees, our analysis does not provide definitive insights about the tradeoffs in benefits to the MDIs of selecting a mix of national and local grantees versus selecting all of one or the other type.

included in the study through Approach # 3 leads to a $233 MDI, in contrast to the $202 MDI when a 50–50 assignment rate is used.

These patterns in the MDIs also are found for the Pathways and SESP grantees (Tables IV.2 and IV.3, respectively).[35] However, when results are compared across grant types, several additional insights emerge. Again, for discussion purposes, we focus on the average quarterly earnings outcome based on survey data, but the patterns also hold for the employment status outcome and/or administrative data.

First, the MDIs are larger for the Pathways grants than they are for the ETP grants, all else equal. For example, using random assignment Approach # 2, a 20 percent rate for participants' inclusion in the evaluation, a 50–50 treatment-control assignment rate, and the selection of 10 local grantees, the MDI for the ETP grants is $190 (Table IV.1). In contrast, it is $304 for the Pathways grants (Table IV.2). This pattern emerges because, based on the publicly available information about grantees' plans, the target number of participants for Pathways grantees is smaller than that for ETP grantees. On average, the ETP grants are about $4 million and include plans to serve about 1,380 participants; in contrast, the averages for the Pathways grants are about $3.9 million and 540 participants.

Second, the MDIs for the SESP grants are relatively small compared with the MDIs shown for the ETP and Pathways grants. For example, an average quarterly earnings MDI of $111 can be obtained if 40 percent of SESP grant participants go through random assignment, if the treatment-control group assignment rate is 50–50, and if 10 grantees are purposefully selected for inclusion in the study through Approach # 3. This MDI is much smaller than the MDIs for the ETP and Pathways grantees except for cases in which a large number of ETP or Pathways grantees is selected. The low MDIs for the SESP grants occur for two reasons. One is that the SESP grants are expected to serve an average of about 1,600 participants, which is a greater number than is the case for the other two types of grants.[36] The second is that, because the SESP grants are for three years and not two, we have assumed that a higher percentage of grant participants can be included in a random assignment study.

[35] The subgroups of grantees shown in Tables IV.2 and IV.3 differ slightly from the subgroups shown in Table IV.1 because we have focused our analysis on examples of subgroups that we think are likely to be the most interesting to DOL; our selections depend on the numbers of grantees for different grant types and the sizes of the MDIs.

[36] The SESP grants are about $5.5 million on average. As with the other grants, some activities other than direct service provision to participants are likely to be included as part of the grants. However, this might be the case especially for the SESP grants, which include the development of a steering committee to develop (or refine) and guide the state's energy sector initiatives.

Table IV.2. Minimum Detectible Impacts on the Percentage Employed and Average Quarterly Earnings, Pathways Out of Poverty Grants

	Random Assignment Approach # 2 (Randomly Subsampling Grantees) [Survey, Administrative Data]		Random Assignment Approach # 3 (Purposefully Selecting Grantees) [Survey, Administrative Data]	
	Percentage Employed	Average Quarterly Earnings ($)	Percentage Employed	Average Quarterly Earnings ($)
50–50 Treatment-Control Group Assignment Rate				
10% of Grant Participants Included in Random Assignment				
10 grantees (5 national and 5 local)	.125, .114	325, 298	.112, .101	295, 264
All 8 national grantees	n.a.	n.a.	.109, .098	286, 256
5 national grantees	.159, .145	413, 377	.147, .132	386, 346
All 30 local grantees	n.a.	n.a.	.079, .070	207, 185
10 local grantees	.156, .141	406, 368	.148, .133	388, 347
20% of Grant Participants Included in Random Assignment				
10 grantees (5 national and 5 local)	.097, .090	252, 235	.080, .071	208, 186
All 8 national grantees	n.a.	n.a.	.077, .069	202, 181
5 national grantees	.121, .112	315, 291	.104, .093	273, 245
All 30 local grantees	n.a.	n.a.	.056, .050	146, 131
10 local grantees	.117, .107	304, 279	.105, .094	275, 246
75–25 Treatment-Control Group Assignment Rate				
10% of Grant Participants Included in Random Assignment				
10 grantees (5 national and 5 local)	.140, .128	364, 333	.130, .116	340, 304
All 8 national grantees	n.a.	n.a.	.126, .113	330, 295
5 national grantees	.180, .163	468, 425	.171, .152	447, 400
All 30 local grantees	n.a.	n.a.	.091, .081	238, 213
10 local grantees	.177, .160	461, 418	.171, .153	448, 401
20% of Grant Participants Included in Random Assignment				
10 grantees (5 national and 5 local)	.107, .099	278, 258	.092, .082	241, 215
All 8 national grantees	n.a.	n.a.	.089, .080	233, 209
5 national grantees	.135, .124	351, 323	.121, .108	316, 283
All 30 local grantees	n.a.	n.a.	.064, .058	169, 151
10 local grantees	.131, .120	341, 312	.121, .108	317, 284

Table IV.2 *(continued)*

Source: Author's calculations based on publicly available information about grantees' plans for serving participants.

Note: Because of the likelihood that the cost of conducting an evaluation using Random Assignment Approach # 1 would be prohibitive, we do not include in the table the MDIs using this approach.

Survey sample sizes assumed in calculations are the lesser of 2,500 or the total number of expected sample members in each group.

The MDI formula used for the calculations in Approach 2 is as follows:

$$factor \times \sigma \sqrt{\rho(1-R_{site}^2)(1-c)\left(\frac{2\times FPC}{\#sites}\right)+(1-\rho)(1-R_{ind}^2)\left(\frac{1}{r\times N_T}+\frac{1}{r\times N_c}\right)}$$

where σ is the standard deviation of quarterly earnings ($1,250) or employment status (0.48) based on results from previous similar studies, *FPC* is the finite population correction, *r* is the response rate (0.80 for the survey, 1.00 for administrative records), R^2 is 0.20 both at the site and individual levels, the intraclass correlation ρ is 0.04, the correlation of treatment and control groups within sites *c* is 0.70, N_T and N_C are pertinent sample sizes for treatment and control groups, and *#sites* is the total number of sites selected in the approach being considered. The MDI calculations assume two-tailed tests. We assume 80 percent power and a 5 percent significance level. *factor* varies as a function of the number of sites being considered, but generally falls in the range of 2.8 to 3.19. Using variables that are defined in a similar way and with similar values, the MDI formula used for the calculations in Approach 3 is as follows:

$$factor \times \sigma \sqrt{(1-R_{ind}^2)\left(\frac{1}{r\times N_T}+\frac{1}{r\times N_c}\right)}.$$

[a]A grantee is designated as an auto restructuring grantee if it has any component that is targeted to an auto restructuring community. Not all grantees that plan to serve a community affected by auto restructuring will devote all of their grant resources to these communities.

MDI = minimum detectable impact.

n.a. = not applicable.

Table IV.3. Minimum Detectible Impacts on the Percentage Employed and Average Quarterly Earnings, State Energy Sector Partnership Grants

	Random Assignment Approach # 2 (Randomly Subsampling Grantees) [Survey, Administrative Data]		Random Assignment Approach # 3 (Purposefully Selecting Grantees) [Survey, Administrative Data]	
	Percentage Employed	Average Quarterly Earnings ($)	Percentage Employed	Average Quarterly Earnings ($)
50-50 Treatment-Control Group Assignment Rate				
30% of Grant Participants Included in Random Assignment				
All 21 auto restructuring grantees[a]	n.a.	n.a.	.040, .026	104, 68
10 auto restructuring grantees	.069, .065	180, 170	.050, .045	132, 118
15 grantees	.057, .050	149, 131	.042, .031	110, 82
10 grantees	.070, .068	183, 176	.043, .039	114, 102
8 grantees	.083, .080	217, 209	.050, .045	132, 118
5 grantees	.125, .121	325, 314	.072, .065	190, 170
40% of Grant Participants Included in Random Assignment				
All 21 auto restructuring grantees[a]	n.a.	n.a.	.040, .023	104, 59
10 auto restructuring grantees	.064, .061	168, 160	.044, .039	114, 102
15 grantees	.057, .048	149, 125	.042, .027	110, 71
10 grantees	.070, .065	181, 169	.042, .034	111, 88
8 grantees	.080, .077	208, 201	.044, .039	115, 102
5 grantees	.119, .116	311, 303	.063, .056	165, 147
75-25 Treatment-Control Group Assignment Rate				
30% of Grant Participants Included in Random Assignment				
All 21 auto restructuring grantees[a]	n.a.	n.a.	.041, .030	106, 79
10 auto restructuring grantees	.075, .070	196, 183	.059, .052	154, 136
15 grantees	.060, .053	157, 139	.046, .036	120, 94
10 grantees	.076, .071	198, 185	.053, .045	138, 117
8 grantees	.089, .084	231, 220	.059, .052	155, 136
5 grantees	.131, .126	342, 328	.084, .075	219, 196
40% of Grant Participants Included in Random Assignment				
All 21 auto restructuring grantees[a]	n.a.	n.a.	.040, .026	104, 68

Table IV.3 (continued)

	Random Assignment Approach # 2 (Randomly Subsampling Grantees) [Survey, Administrative Data]		Random Assignment Approach # 3 (Purposefully Selecting Grantees) [Survey, Administrative Data]	
	Percentage Employed	Average Quarterly Earnings ($)	Percentage Employed	Average Quarterly Earnings ($)
10 auto restructuring grantees	.071, .065	185, 170	.053, .045	139, 118
15 grantees	.058, .050	150, 131	.043, .031	111, 82
10 grantees	.073, .068	190, 176	.048, .039	126, 102
8 grantees	.085, .080	222, 209	.053, .045	140, 118
5 grantees	.125, .121	325, 314	.072, .065	190, 170

Source: Author's calculations based on publicly available information about grantees' plans for serving participants.

Note: Because of the likelihood that the cost of conducting an evaluation using Random Assignment Approach # 1 would be prohibitive, we do not include in the table the MDIs using this approach.

Survey sample sizes assumed in calculations are the lesser of 2,500 or the total number of expected sample members in each group.

The MDI formula used for the calculations in Approach 2 is as follows:

$$factor \times \sigma \sqrt{\rho(1-R^2_{site})(1-c)\left(\frac{2\times FPC}{\#sites}\right)+(1-\rho)(1-R^2_{ind})\left(\frac{1}{r\times N_T}+\frac{1}{r\times N_c}\right)}$$

where σ is the standard deviation of quarterly earnings ($1,250) or employment status (0.48) based on results from previous similar studies, *FPC* is the finite population correction, *r* is the response rate (0.80 for the survey, 1.00 for administrative records), R^2 is 0.20 both at the site and individual levels, the intraclass correlation ρ is 0.04, the correlation of treatment and control groups within sites *c* is 0.70, N_T and N_c are pertinent sample sizes for treatment and control groups, and *#sites* is the total number of sites selected in the approach being considered. The MDI calculations assume two-tailed tests. We assume 80 percent power and a 5 percent significance level. *factor* varies as a function of the number of sites being considered, but generally falls in the range of 2.8 to 3.19. Using variables that are defined in a similar way and with similar values, the MDI formula used for the calculations in Approach 3 is as follows:

$$factor \times \sigma \sqrt{(1-R^2_{ind})\left(\frac{1}{r\times N_T}+\frac{1}{r\times N_c}\right)}.$$

Table IV.3 (continued)

[a]A grantee is designated as an auto restructuring grantee if it has any component that is targeted to an auto restructuring community. Not all grantees that plan to serve a community affected by auto restructuring will devote all of their grant resources to these communities.

MDI = minimum detectable impact.

n.a. = not applicable.

V. OTHER TYPES OF METHODOLOGIES

Although random assignment is generally considered to be the most rigorous approach for estimating impacts of an intervention, other types of methods can be used to estimate impacts when random assignment is infeasible or undesirable. The main limitation of these methods, such as those that rely on a comparison group, stems from the fact that all nonexperimental designs suffer to some degree from a risk that factors other than the intervention might explain the patterns in outcomes that are observed. In such a situation, one cannot have confidence that the intervention itself (such as ARRA-funded training) is responsible for any detected effects.

In this chapter, we describe three nonexperimental designs: regression discontinuity (RD), comparison group, and interrupted time series designs. Although each of these designs is less rigorous than a random assignment design, other features of these designs may avoid or mitigate some of the implementation challenges associated with random assignment, as discussed in Chapter IV.

Although DOL has expressed an interest in pursuing an evaluation of individual-level outcomes, we include a brief discussion of designs that could be used to examine community-level outcomes because these designs could complement an analysis of individual-level outcomes. In Section A, we describe the RD design in general and two variations on that design that would be appropriate for an evaluation of ARRA grants. Section B describes two comparison group designs, one based on examining individual-level outcomes, and the other based on examining grantee-/community-level outcomes. Finally, Section C briefly describes an interrupted time series design.

Variations of each of these three designs could be implemented to look at either individual- or community-level outcomes.[37] Our discussions in these sections focus on the analytical differences in these methodologies, given the scope of this pre-design study. However, if policymakers would like to use one of these approaches, the decision about which methodology is preferable will likely need to take into account other considerations that are not discussed here, such as the costs of the different designs.

A. Regression Discontinuity with Individuals in Winning Grantees or Grant Applicants

As described in Chapter IV, the goal of a random assignment design is to ensure that the individuals in the treatment and control groups are similar on both observed measures, such as age and education level, and unobserved measures, such as underlying motivation. This equivalence of the two groups enables estimation of what would have happened to treatment group members in the absence of the intervention (in this case, the ARRA grant-funded training).

Although random assignment designs use a random selection process to assign individuals to the treatment and control groups, another method, called RD, takes advantage of a systematic, nonrandom allocation into a treatment group that can receive the intervention and a group that

[37]As discussed in Chapter III, examples of outcomes for individuals who are offered training financed by an ARRA grant include employment status and average quarterly earnings. Examples of community-level outcomes, which look at the whole community rather than the individuals within it, could include the number of new jobs created in the target industries.

cannot. A recent study indicates that, under certain conditions, an RD design can provide impact estimates comparable to those of a random assignment study conducted on the same population (Cook & Wong, forthcoming).

In this section, we first provide an overview of the RD design generally and the conditions under which it provides valid impact estimates. We then describe two variations of the general design that would be appropriate for an evaluation of ARRA grants. The section concludes with a discussion of the relative strengths and weaknesses of the RD designs considered.

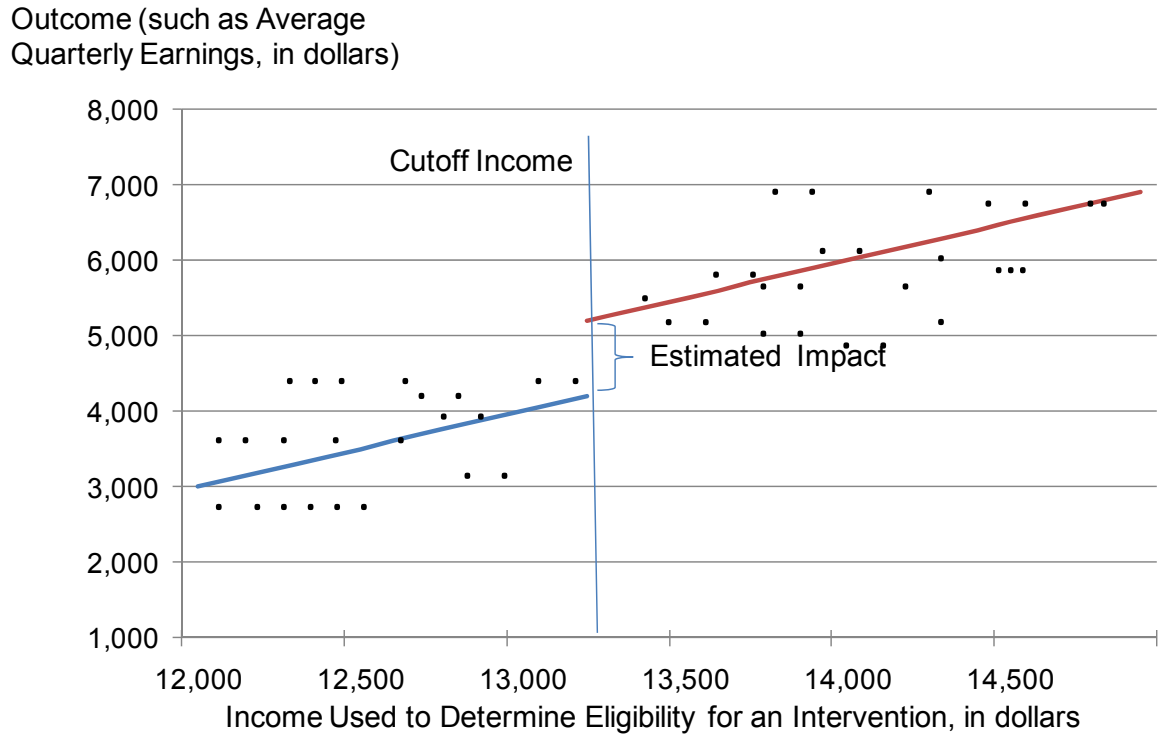
1. Overview of RD Designs

RD designs can be applied when a rule based on a continuous numerical variable is used to determine the eligibility of a person or entity for an intervention. This rule can be used to generate two groups—eligible and ineligible for the intervention—that can be distinguished from each other on the basis of a cutoff score. For example, applicants for grant funding might be reviewed and given a score that is used to determine those applicants who receive funding and those who do not. The group of applicants that receives funding forms the treatment group; the other group, which does not receive funding, forms the comparison group. Of critical importance for this design to provide valid estimates of effects, the score must be assigned according to well-designed and replicable rules that were adhered to during the scoring process.

When using an RD design, the effects of the intervention being studied—for example, the grant funding mentioned earlier—is estimated *exactly at the eligibility cutoff score* as the difference in outcomes between the group that receives the intervention and the group that does not. More specifically, a line or curve is fit to the data for the intervention group and another line or curve is fit to the data for the comparison group to model statistically the relationship between the outcome of interest and the eligibility score (see Figure V.1). The estimated impact of the intervention is the difference in outcomes for individuals or entities just below and just above the cutoff score. Stated differently, the intervention has a nonzero impact if there is a "discontinuity" in the two lines or curves—hence the "regression *discontinuity*" name for this study design. All types of evaluation designs need to have large enough samples of people or entities to ensure that effects of an intervention can be estimated precisely, but an evaluation that uses an RD design needs a much larger sample (on the order of three times as large for nonclustered designs [Goldberger 1972] and three to four times as large for clustered designs [Schochet 2009]) than an evaluation of the same intervention that uses random assignment. This is because of the substantial correlation between the treatment status and the score variables that are included in the impact models.

For this design to be appropriate, it is critical that applicants just above and just below the cutoff score are virtually identical, and differ only because those just above (or just below) the cutoff qualify for the intervention. Ultimately, this means that the scoring variable is the only variable that determines one's assignment to the treatment or comparison group. This condition can be checked through a statistical examination of whether the treatment and comparison groups are alike on key characteristics measured prior to the start of the intervention.

Figure V.1. Visual Interpretation of a Regression Discontinuity Design



Note: This figure uses hypothetical data and regression lines to demonstrate a regression discontinuity design with an outcome such as average quarterly earnings on the Y-axis and annual income used to determine eligibility for an intervention on the X-axis.

2. RD Designs for an Evaluation of ARRA Grants

We consider two types of RD designs for an evaluation of ARRA grants. The first design, which we refer to as the individual-level RD, uses a cutoff value based on income at the individual level at winning grantees to examine individual-level outcomes of interest. The second design, which we refer to as the grantee-level RD, uses variation in the scores of grant applicants to divide grantees into treatment and comparison groups. With this latter approach, outcomes could be examined at either the individual or community level.

a. Individual-Level RD with Winning Grantees

An individual-level RD approach would compare the outcomes of individuals in the treatment group with those in a comparison group, where membership in the treatment group is based on an observable and precise cutoff. For instance, it is often the case that there are income restrictions for participation in training; individuals whose income is too high (as determined by WIA regulations) are not eligible to participate. This income cutoff can serve to identify members of the treatment and comparison groups whose outcomes can be compared to give a credible estimate of the treatment's effectiveness. For instance, suppose the income cutoff were $20,000, so that individuals who apply for training but whose incomes are above that amount were not eligible for training. The RD design could compare the outcomes of individuals with income just above $20,000 (control group) with the outcomes of individuals with income just below $20,000 (treatment group). The intuition is that individuals close to the cutoff income limit are similar, on average, and thus the

design can generate credible estimates of treatment effects. It should be noted that it is not necessary that every grantee use the same cutoff; in fact, that could be advantageous for estimating impacts across a range of cutoff scores.

As applied to an evaluation of ARRA grants, the individual-level RD design would be appropriate for grantees in which the following three conditions are met. First, there must be some type of continuous program eligibility cutoff that can be used to establish the discontinuity. Although the grantees receiving the Pathways out of Poverty grants, which are specifically targeted to high-poverty communities, might use an income cutoff to determine an individual's eligibility for grant-funded services, ETP grantees that do not specifically target high-poverty individuals might not have any eligibility criteria that would allow an RD design to be used. Second, the cutoff must not also be used to determine eligibility for other, non-ARRA services, as this would make it difficult to isolate the effect attributable to ARRA grants. For instance, Pathways out of Poverty grants may use the federal poverty line to determine eligibility, but since this also determines eligibility for a host of other services it would not be an appropriate cutoff for an RD design. Finally, the cutoff must not be subject to widespread manipulation on the part of participants or program staff at the grantee.

b. Strengths and Weaknesses of an Individual-Level RD Approach

The RD design in general has several advantages compared with other types of designs. Compared with a random assignment design, this approach has two advantages. First, it often can be applied even after programming has begun, as long as the scoring process has been documented. Second, the approach requires minimal interference with normal program activity; in short, grantees are likely to be less burdened under this design than a random assignment design. Compared with other nonexperimental methodologies, discussed later in this chapter, this approach is more rigorous; when the scoring rule is fully known and determined exogenously, selection bias is minimized.

However, there are a few potential drawbacks to this design. First, as previously discussed, it would apply only to grant types in which there is some eligibility threshold for training, such as the income cutoff mentioned earlier. Second, in order for this design to produce credible estimates, it is necessary that there be minimal manipulation of the score used to determine eligibility by either individuals who desire grant services or program staff, who presumably will be eager to provide services. Third, this design requires sample sizes about three times larger than a random assignment design to estimate effects to the same level of precision. Fourth, compared with a random assignment design, external validity is more limited, because it applies only to individuals close to the cutoff score; this would be mitigated if a range of scores were used across grantees. Finally, unless extensive baseline information has been collected before an individual's eligibility determination, it may be difficult to obtain outcome data (both administrative and survey) for those who were deemed ineligible.[38]

[38] If outcome data are to come exclusively from administrative wage records, then the identifying information that is used to match with these records, such as Social Security numbers, would be necessary. If some of the outcome data are to come from follow-up surveys, then a more extensive set of identifying and locating information would be necessary to avoid differential rates of response to the follow-up survey. This information might include the individuals' names, dates of birth, addresses, and telephone numbers, and other contact information. Some of this identifying information might normally be collected as part of the process to access an individual's eligibility for grant services.

c. Grantee-Level RD with Grant Applicants

A grantee-level RD approach could use the scores assigned during the grant award process to determine treatment status. As applied to an evaluation of ARRA grants, this design would be appropriate for those grant types wherein many more applications were made than grants available, and grants were awarded based on a well-defined and adhered-to scoring process. Although information on the number of grant applicants is not available, it seems unlikely that a grantee-level RD approach would be feasible for the SESP grants. The only types of eligible grantees were state workforce investment boards, in which the term "state" pertains to the 50 states, the District of Columbia, and U.S. territories. Because 34 SESP grants were awarded, it is likely that there were too few nonwinning applicants for the grantee-level RD approach to be appropriate in an evaluation of the SESP grants. However, it seems likely that the other three types of ARRA grants had a larger number of grant applicants, which might help to ensure that this methodological approach is suitable.

If a grantee-level RD approach is used for an evaluation, the outcomes could be either at the individual level or at the community level. If individual-level outcomes are of interest, the outcomes of individuals served by the winning grantees would be compared with the outcomes of individuals in communities served by grant applicants who were not awarded a grant. But at the individual-level, it is unclear who would comprise a suitable comparison group. Although the applicants for ARRA grant funding were instructed to specify their target groups for services, should they be awarded a grant, it is likely to be challenging for evaluators to define and collect data on such individuals or a subset of them. Although an applicant who did not receive an award might serve similar types of customers to those who would have been served with an ARRA-funded grant, one could never be sure that this is the case and that there was no selection bias of who forms the individual-level comparison group sample. A best-case scenario, from an evaluation perspective, would be to focus on applicants that did not receive an award but that had specified plans to serve participants who are very similar to adult or dislocated workers. Information on these individuals' outcomes could be made available from a centralized data system maintained by the local workforce investment areas (LWIAs).[39] Even in this case, however, it would not be possible to identify which members of the comparison group would have received training at the grantee, had the grantee won; thus, there would be concern about selection bias in the sample.

In other cases, the target groups of nonwinning grant applicants might be difficult to find for inclusion in the study, such as when a grant applicant planned to serve workers who were detrimentally affected by national energy and environmental policy. These individuals might not request services or be traceable through any other method that uses a centralized data system.

If community-level outcomes are of interest, the outcomes in communities served by winning grantees would be compared with the outcomes in communities served by grant applicants who were not awarded a grant. For instance, the Quarterly Workforce Indicators could be used to

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(continued)

However, Mathematica's experience is that follow-up surveys are most likely to be successful if additional information is collected. An implication of this is that, like a random assignment design, the RD design could benefit from having the evaluation design in place prior to the start of participants' enrollment in grant activities.

[39] As discussed in Chapter III, there is a possibility that many of the individuals in communities that applied for but did not receive ARRA grants will access similar types of services through other funding sources.

provide quarterly measures of total employment; measures of change such as job flow, new hires, and separations; and average earnings by county, metro area, WIA, or by two-digit industry code.

d. Strengths and Weaknesses of a Grantee-Level RD Design

The grantee-level RD design shares the advantages noted for the individual-level RD design mentioned earlier: namely, it is a rigorous nonexperimental design, it can be applied after programming has begun, and it requires minimal interference with normal program activity. In addition, it would likely be less cumbersome to collect outcome data on communities served by grant applicants rather than individual-level data because community-level outcomes can more readily be accessed through existing data.

However, the design has several drawbacks. First, the treatment assignment rule may not be perfectly known; although DOL used a committee to determine the ranked scores of grant applicants based on well-specified criteria, it could also take into account other factors in determining awards, such as geographic representativeness, which might compromise the rank ordering necessary to establish the discontinuity. Second, given the limited number of grant applicants for each grant type, even if this approach is technically feasible, this design would have limited statistical power. Third, external validity is more limited with this approach, as it applies only to communities served by grant applicants, or individuals within them, close to the cutoff score. Finally, as discussed earlier, an examination of individual-level outcomes using the grantee-level RD approach necessitates identifying an appropriate set of individuals in communities that were not awarded grants and gathering data on them.

B. Comparison Group Designs

In some cases in which random assignment is not feasible or appropriate, and in which the conditions for an RD design are not likely to be met, it might be possible to use a comparison group design to develop an estimate of the impact of an intervention. A comparison group design attempts to compare the outcomes of people or entities that are as similar as possible with the group that receives the intervention in order to reduce potential selection bias when estimating treatment effects.

In this section, we first provide a general overview of comparison group designs, followed by two specific designs that might be used for an evaluation of ARRA grants. We also present the strengths and weaknesses associated with each of the designs.

1. Overview of Comparison Group Designs

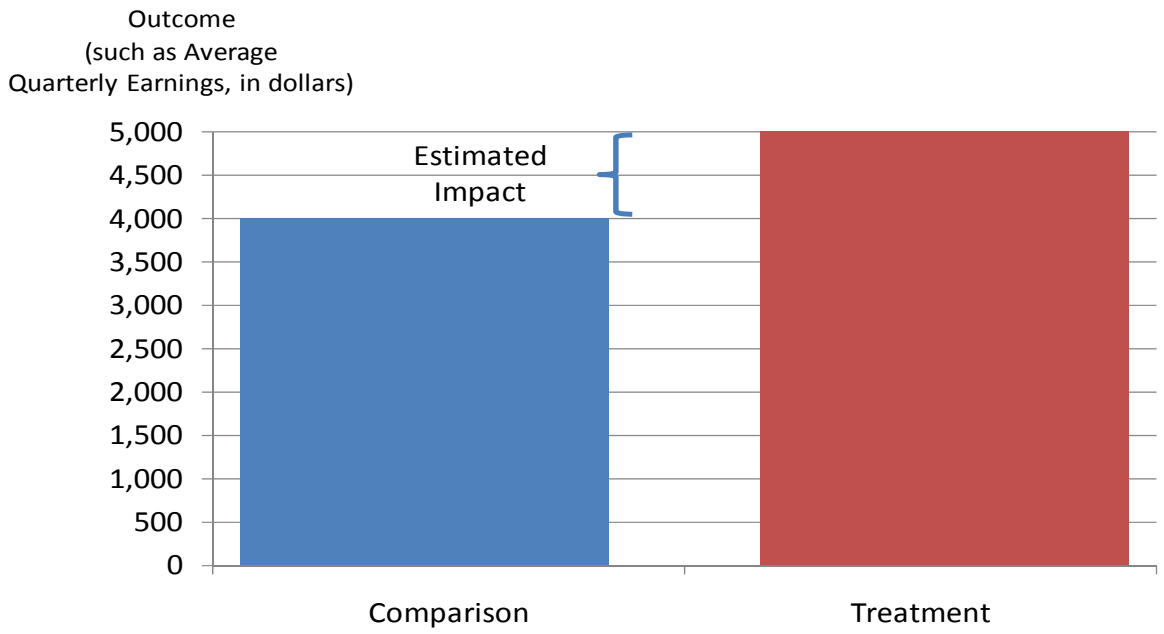
In a comparison group design, observable characteristics are used to create a comparison group composed of individuals or entities that look very similar to treatment group members. The assumption is that, because the comparison group is similar to the treatment group on observed variables, it must also be fairly similar on unobserved variables that have an influence on the outcomes of interest. This is a strong assumption, and for this reason comparison group designs are generally thought to be less rigorous than either (1) a random assignment design, which uses a random allocation to assign people to groups; or (2) an RD design, which uses a systematic allocation process to assign people to groups.

However, when a comparison group is selected, the outcomes of treatment group participants could be compared with the outcomes of the individuals in the comparison group to obtain an estimate of the impact of an intervention (see Figure V.2). As the figure suggests, the comparison group approach has similarities to a random assignment design, but there is one *very* important difference: Although researchers could try to take into account the differences between the treatment and comparison groups that they can observe and measure in a comparison group design, they cannot guarantee that they have accounted for *all* important differences between the two groups.

2. Comparison Group Designs for an Evaluation of ARRA Grants

We consider two types of comparison group designs for an evaluation of ARRA grants. The first design, which we refer to as the individual-level propensity-score matching design, uses a statistical routine to match treatment group members to comparison individuals who are observationally similar; the comparison group members are selected as the best matches to the treatment group from among a large set of potential comparison group members. Then, the outcomes of the treatment and comparison group members are compared. The second design, which we refer to as the community-level difference-in-differences design, uses variation in community-level outcomes both across time and across communities to determine treatment effectiveness.

Figure V.2. Impact Estimation from a Comparison Group Design



Note: This figure uses hypothetical data to demonstrate a comparison group design with an outcome such as average quarterly earnings on the Y-axis.

a. Individual-Level Propensity–Score Matching Design

A relatively rigorous individual-level comparison group design would first match treatment group members (that is, individuals in winning grantees) to similar individuals who did not receive the treatment via propensity-score matching. It would then assess treatment effects by comparing the outcomes of those in the treatment group with those in the matched comparison group. Statistical theory and some empirical evidence indicates that use of propensity-score matching is sufficient to remove bias between treatment and control group members due to the differences between the two groups on their observed characteristics (Rosenbaum and Rubin 1983).

Propensity-score matching to select a comparison group from among a pool of potential comparison group members entails three steps: (1) identify through a regression how the characteristics of treatment and potential comparison group members are associated with the likelihood of being in the treatment group; (2) estimate a single number (a "propensity score") for each individual to represent this likelihood; (3) identify which potential comparison group member(s) are close enough to a treatment group member on this score, and then select them to be in the matched comparison group. The resulting comparison group can then be assessed to see how similar its members are on observable characteristics to the treatment group's observable characteristics, on average. In general, the most promising comparison groups can be drawn when the treatment and comparison group members are drawn from the same local market areas, a common data source contains the matching variables for both groups, and rich pre-intervention measures of the outcomes are available.

Because ARRA grantees were given considerable flexibility in choosing the target groups that they could plan to serve, the propensity-score matching approach would need to be similarly flexible. First, the pool of potential comparison group members for each grantee that is included in an evaluation using this approach must be chosen carefully to correspond with individuals from whom participants could be drawn. For instance, some ETP and SESP grantees plan to serve workers who are affected by national energy and environmental policy. For these grantees, one could try to identify dislocated workers to use for a comparison group pool, even though there is unlikely to be a data item in LWIA records that pertains precisely to this characteristic.

Second, the precise list of characteristics to include in each propensity-score matching model is likely to depend on the specific target group chosen by each grantee. For instance, the Pathways grantees were allowed to target adults who are unemployed, high school dropouts, have a criminal record, or are disadvantaged and living in areas of high poverty. Many of these attributes will be shared by formula-funded WIA adult customers who have been and will continue to be served in the grant communities; therefore, WIA adult customers could form a potential pool of individuals from which to draw comparison group matches to the Pathways-funded participants. Given the target population of Pathways grants, it is likely important for the propensity-score model in this case to include indicator variables for having a criminal record, noncompletion of high school, and living in a high poverty area in order to ensure the comparison group members are as similar as possible to grant participants.

It might be more challenging in some other cases to define an appropriate pool of potential comparison group members and obtain sufficient data on observable characteristics to yield a high-quality match of comparison group members to the treatment group. For example, if a Health Care grantee targets incumbent workers for the provision of training, then it might not be appropriate to use WIA adult or dislocated worker customers as the source of the comparison group pool. Other

incumbent workers from similar employers might be another source for the pool, but it is likely that these incumbent workers will differ from the ARRA participants on unobservable characteristics because many of them are unlikely to pursue training and career development.

Regardless of the expectations about the quality of the source for potential comparison group members, the evaluation will need to assess the quality of the match process after a comparison group is selected. In some cases, it might be necessary to refine the propensity-score model and select another group.

Finally, we note that the propensity-score matching approach could also theoretically be conducted at the grantee level. This would involve matching (based on observable characteristics) communities served by winning grantees to similar communities that were not served by ARRA funds. However, given the potentially limited number of comparison pool members, it is probably not feasible to implement this approach.

b. Strengths and Weaknesses of Propensity-Score Matching Relative to Other Designs

The main strength of the propensity-score matching design is that it allows for an evaluation of program effectiveness even if all or most individuals received ARRA-funded training, as long as a suitable comparison group can be found. Like the RD design, the comparison group strategy using propensity-score matching at the individual level can be applied after programming has begun and with minimal interference with normal program activity. This design also takes into account a wide range of observable characteristics to minimize the selection bias associated with impact estimates.

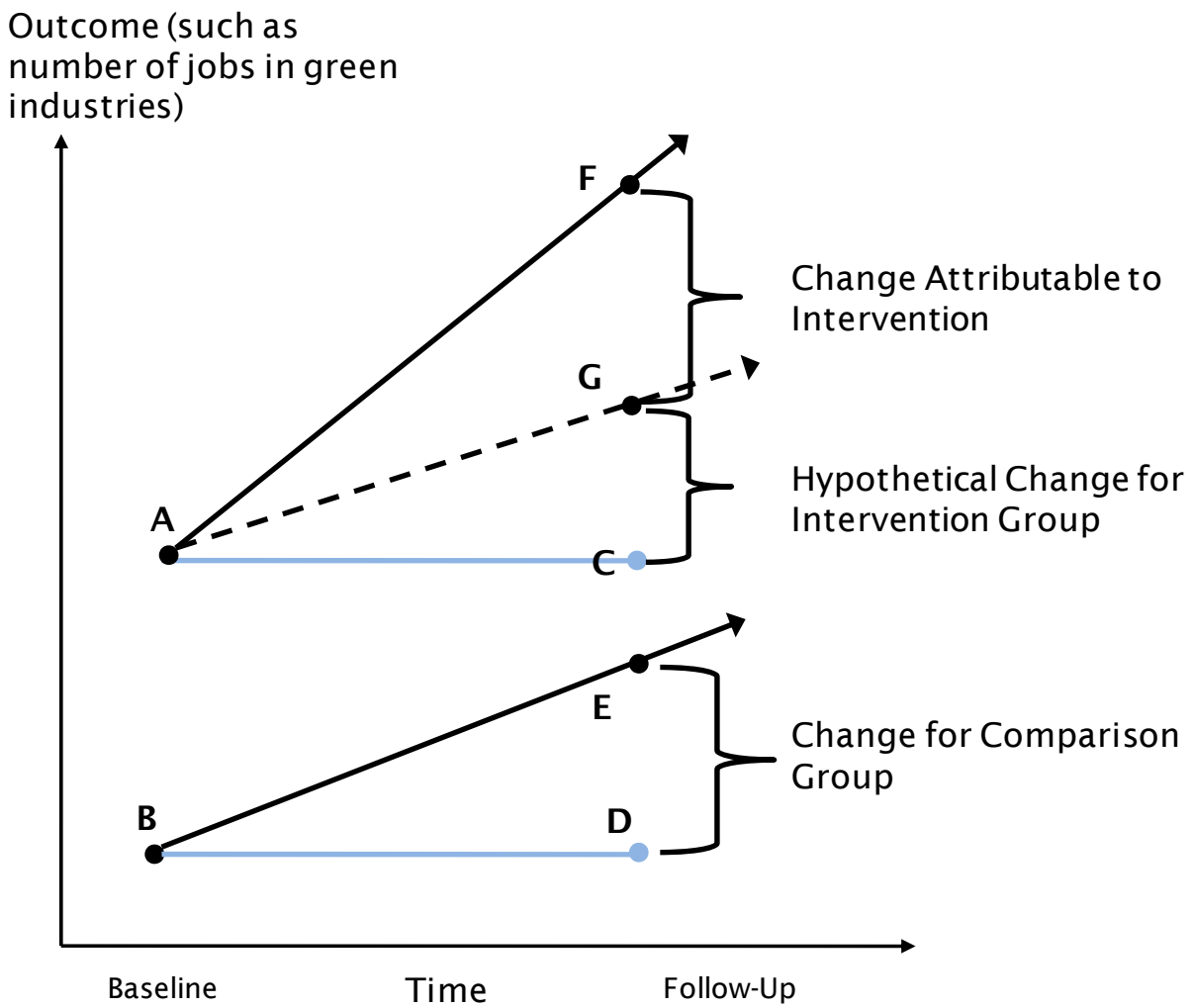
However, the approach has several potential drawbacks. First, it is not as rigorous as a well-implemented randomized control trial or RD design. Second, because of the diversity of the target populations served by grantees as mentioned previously, the pool of comparison group members may have to be tailored to each grantee, which can be a labor-intensive process. Moreover, when a suitable comparison pool has been identified, the matching algorithm requires extensive baseline data on the comparison pool members, which may not be readily available. Ultimately, this approach presents a risk that one might have to conclude that the highest-quality comparison group available differs significantly from the treatment group—a finding that would cast doubt on estimates of the impacts of the ARRA grants on participants' outcomes.

c. Community-Level Difference-in-Differences Design

A difference-in-differences design, which requires less intensive data collection than the method described previously, can be applied when using a comparison group design to examine outcomes at the community level. In this design, one needs to find a suitable comparison group that did not receive the treatment and compare the change in outcomes for the treatment group over time with the change in outcomes over time for a suitable comparison group that did not receive the intervention. The use of a comparison community helps to control for influences, such as macroeconomic trends and changes in other funding sources, occurring at the same time the treatment was administered and that might affect post-treatment levels of the outcomes in both comparison and treatment groups.

Figure V.3 shows how the difference-in-differences approach works. In the figure the change over time for the treatment group is the difference between points C and F, whereas the change for the comparison group is the difference between points D and E. However, the change over time for the treatment group can be thought of as consisting of two parts. The first part, represented by the difference between points C and G, is what would have happened to the treatment group if there were no intervention. (That is, the treatment group would have experienced the same change as was experienced by the comparison group, because point G is defined such that the slope of the lines between A and G is the same as the slope of the line between B and E). The second part, represented by the difference between F and G, is the change that is unique to the treatment group.

Figure V.3. Visual Interpretation of a Difference-In-Differences Design



Note: This figure uses hypothetical regression lines to demonstrate a difference-in-differences design with an outcome such as number of jobs in green industries on the Y-axis and time on the X-axis.

By taking the difference between pre- and post-period outcomes in the treatment group and comparing those with the difference in pre- and post-period outcomes in the comparison group, a "difference-in-differences" estimate of the program effects is obtained. For example, suppose that from 2010 to 2011 the number of green jobs created in treatment communities increased by 3,000 (that is, the difference between C and F = 3,000, after controlling for other factors). Suppose that over that same period the number of jobs created in comparison communities increased by 1,000 (that is, the difference between D and E = 1,000). Then, one could credit an increase of 2,000 jobs (= the difference between F and G) to the treatment.

Similar to the propensity-score matching approach described earlier, the suitable comparison groups would likely vary depending on the type of grant being evaluated.[40] For example, Pathways grantees need to serve high-poverty communities as defined by Public Use Microdata Areas (PUMA) data. One could use the PUMA data to identify other communities of similar size and in the same areas of the country that have similar poverty rates to form the comparison group. For the auto-related subsets of the Health Care and ETP grants, one could use the 312 auto restructuring communities that did not receive grants and were in similar geographic areas as the comparison group.

The ETP and Pathways out of Poverty grantees may have credible comparison groups if it is the case that the national networks that were awarded grants had local affiliates to whom they did not distribute funds; the communities served by affiliates who did not receive funds could be compared with the communities served by affiliates who did receive funds. Finally, the SESP grants were awarded to some substate areas and not others; the areas that did not receive ARRA funding through the grants could serve as comparison communities for those that did receive funding.

We note that a difference-in-differences approach could also be used to examine individual-level outcomes. The difference-in-differences approach benefits from extensive historical data on outcomes in order to establish the pre-intervention trends experienced by both groups. This would likely be difficult and resource-intensive to obtain at the individual-level.

d. Strengths and Weaknesses of the Community-Level Difference-in-Differences Design

The main strength of the community-level difference-in-differences design is that it allows for an evaluation of program effectiveness, even if all or most grant applicants have received funding, as long as a suitable comparison group can be found. The design can be implemented after programming has begun with no program interference (because outcomes are collected at the community level). This approach also accounts for other secular trends that may be occurring in treatment and comparison communities even in the absence of the treatment, thereby providing a more confident assessment of program effectiveness.

The main drawback to this design is that one may still have concerns that unobservable characteristics influence why some sites were part of the grant activities and others were not. For instance, in comparing the local affiliates of national networks that received funding to those that

[40] In contrast to the propensity-score matching approach, there is no statistical routine to match comparison communities. Rather, they are selected on a more ad hoc basis. For this reason, this approach is rated as less rigorous than a propensity-score matching approach.

did not, one might worry that there were unobserved reasons the national network decided to distribute funds to those particular affiliates. It might be the case that they only distributed funds to the affiliates they thought were most likely to be successful; these affiliates may also have been the most successful even in the absence of the ARRA funding.

In addition, the difference-in-differences approach requires extensive data on characteristics of grantee communities and potential comparison communities for the period preceding the intervention in order to establish a pre-intervention trend with adequate precision. Although extensive outcome data are collected at the county and metro area in sources such as the Quarterly Workforce Indicators, it may be difficult to obtain data on potential comparison communities within those broader areas. For instance, obtaining data on job creation in high-poverty communities within a county may be difficult.[41] Finally, doubts will remain as to how comparable the treatment communities are with the comparison communities that were chosen.

C. Interrupted Time Series Design

In cases in which random assignment is not feasible, there is no selection variable to use as a cutoff score for an RD design, and finding a suitable comparison group is not feasible, it might be possible to use an interrupted time series design, also known as a pre-/post- or before/after design, to estimate the impact of an intervention. In contrast to the designs discussed previously, an interrupted time series design does not involve a control or comparison group. Because this design is far less rigorous than other designs, we provide only a brief description and discussion of the strengths and weaknesses of the approach.

1. Overview of Interrupted Time Series Designs

In an interrupted time series design, outcomes of the treatment group after the intervention is implemented are compared with the outcomes of the treatment group before the intervention was implemented. This approach is based on an assumption that any differences between the outcomes before and after the intervention can be fully explained by characteristics that can be quantified and used in a statistical analysis. After controlling for these characteristics, the remaining difference between the pre-intervention outcomes and the post-intervention outcomes is attributed to the intervention (see Figure V.4). When this type of study design is used, it is very important to have data on a long history of relevant outcomes before and after the intervention, so that the pre- and post-intervention trajectories shown in the figure can be precisely estimated.

This design can be used to examine outcomes at either the individual or grantee level. For example, one could look at the outcomes of individuals before and after they participate in ARRA-funded training. Alternatively, for an analysis at the community level, one could look at community-level outcomes before and after grant receipt.

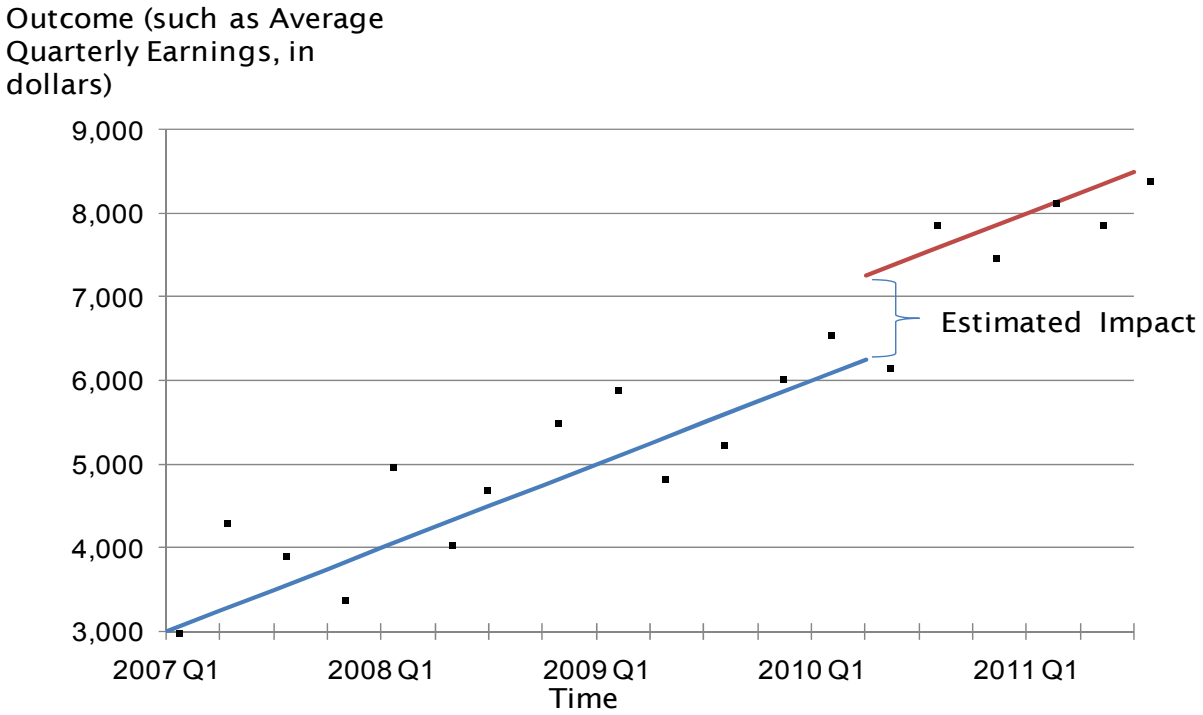
[41] As described in Chapter II, DOL did not specify a target size for the communities to be served by each grant type. However, for the Pathways out of Poverty grants, DOL expected that the target communities would contain between 10,000 and 100,000 people. For urban areas, those communities might be neighborhoods within a city rather than the entire city; for rural areas, they might be collections of between one and three counties.

2. Strengths and Weaknesses of Interrupted Time Series Designs

The interrupted time series design has the same advantages as the difference-in-differences design described earlier, with the additional advantage that finding suitable comparison individuals and communities and collecting data on them are not necessary. However, this design requires detailed baseline information data and benefits from extensive historical information, which may be difficult to collect for individuals (less so for communities).

Moreover, any advantages to this design come at a significant cost, namely that changes in outcomes could have occurred over time because of other factors that are not taken into account statistically or because of unknown or unanticipated changes that coincide in time exactly with the intervention being studied; the study could incorrectly attribute to the intervention the effects of changes that have nothing to do with it. Concerns about other changes that coincide with the ARRA grants and that might explain estimated impacts would be especially severe given the magnitude and characteristics of the recent economic recession.

Figure V.4. Visual Interpretation of an Interrupted Time Series Design



Note: This figure uses hypothetical data and regression lines to demonstrate an interrupted time series design with an outcome such as average quarterly earnings on the Y-axis and time on the X-axis.

VI. SUMMARY OF CONSIDERATIONS WHEN PROCEEDING WITH AN EVALUATION OF THE ARRA GRANTS

As explained in Chapter I, the purpose of this white paper is to provide guidance to DOL as it assesses potential methodologies for evaluating one or more of the four sets of ARRA grants: (1) Energy Training Partnership Grants; (2) Pathways Out of Poverty Grants; (3) State Energy Sector Partnership and Training Grants; and (4) Health Care Sector and Other High-Growth and Emerging Industries Grants. Although these sets of grants share many similarities, they differ by providing funding to different types of grantees and, in some cases, by targeting different industries. Furthermore, the grantees will have different target populations; some focus on workers who are unemployed and in need of basic skills development and others focus on workers dislocated from failing industries or those who are already in high-growth and emerging industries but in need of updated skills.

Using the publicly available information about the grants, as well as our prior experience conducting evaluations for DOL and other clients, we have discussed possible outcomes of interest to DOL, which are summarized in Table VI.1. We have compared and contrasted a wide range of possible approaches for an evaluation of the grants. Inherently, we have had to make many assumptions about how a potential evaluation might unfold, and our discussion of each potential approach is not as comprehensive as would be expected during the design phase of an evaluation, when more information would be available and fewer options under consideration. To facilitate a comparison of the approaches, however, we have summarized their primary strengths and weaknesses in Table VI.2.

To facilitate comparison of the relative rigors of the different methodologies, we also have provided a four-tier categorization of the rigor of each of the approaches: (1) very high, (2) high, (3) moderate, and (4) low. While this categorization inherently is somewhat subjective, it is based on our understanding of consensus views within the research community about the rigor of different methodological approaches. It also takes into account our best guesses at how the application of a methodology in the distinctive context of the ARRA grants will influence its rigor. Ultimately, we intend for this categorization to serve as a guide, rather than an absolute metric, in the assessment and comparison of the approaches.

Importantly, we have not recommended a single methodological approach as the best way to evaluate these grants; rather, we discuss the advantages and disadvantages of different methodological approaches so that DOL can consider its options for proceeding with any evaluation of these grants.

Although we do not recommend how to proceed with an evaluation, the analysis presented in this paper provides insights that are likely to be useful to DOL as it proceeds with planning an evaluation of the ARRA grants. We briefly summarize the main insights below.

Three possible objectives for an evaluation could be of interest to DOL. The first, which we have focused on given DOL's expressed interest in it, is learning about the microeconomic (individual-level) effects on workers of training funded by ARRA grants. However, other objectives could include learning about the macroeconomic effects of the grants on communities or industries and learning about implementation issues associated with the grants. Studies that examine macroeconomic or implementation issues could be done either in concert with an analysis at the individual level or instead of such an analysis. Regardless of the specific objectives of an evaluation

Table VI.1. Overview of Research Questions and Considerations for the Evaluation

Type of Outcome Examined	Research Question	Example Outcomes	Considerations for the Evaluation
Individual-level	What was the impact of access to ARRA grant funding on the outcomes of participants?	An individual's probability of being employed, quarterly earnings (across industries or within certain target industries)	An understanding of the counterfactual is needed, regardless of the selected research design or approach. There is a great amount of funding for training from other sources and through ARRA grants. Therefore, any potential control or comparison groups of communities may ultimately receive funding very similar to that of successful grant applicants, and control/comparison group individuals in grantees' sites might receiving training similar to that which grant participants get. This may limit what can be learned from comparisons of outcomes of grant participants with nonparticipants.
Community-level	What was the impact of ARRA grant funding on the macroeconomic conditions in communities that received them?	Number of jobs created in target industries in the community, unemployment rate in the community	

Note: Although DOL might want to consider conducting an evaluation of implementation or operational issues associated with the ARRA grants, we do not include that type of approach in this table, nor do we discuss the issues associated with that type of evaluation in detail in this paper. DOL has expressed interest primarily in rigorous quantitative methodologies for an evaluation of the grants. Even if implementation issues are not the primary focus of an evaluation, however, an implementation study could be an important component of an evaluation.

Table VI.2. Overview of Potential Methodologies to Evaluate the ARRA Grants

Approach	Summary of Approach	Strengths	Weaknesses	Methodological Rigor
Individual-Level Outcomes				
Random assignment of all individuals at all grantees	At all winning grantees, random assignment uses a statistical process to randomly assign some proportion of individuals seeking services to a treatment group and the remainder to a control group, which may not receive grant services. It compares outcomes of those in the treatment group with those in the control group.	Random assignment ensures that there will be no systematic differences between the average characteristics of the treatment and control groups, so any detected impacts can be ascribed to the treatment. This approach has external validity and the most power to detect impacts of any design considered. If control group assignment rates are high enough, it might support impacts for specific sets of grantees with a high degree of statistical precision.	Such a wide-scale data collection effort is unlikely to be feasible due to cost considerations. Compared with other designs, it takes a long time to implement, and it may be difficult to implement after grantees have begun serving participants. These timing issues might influence either the feasibility of the approach or the size of the sample that can be included in the study. Grantees may not be comfortable withholding services for individuals assigned to the control group.	Very high
Random selection of grantees, then random assignment of individuals at the selected grantees	First, this approach uses a statistical process to randomly select a subset of all winning grantees for study participation; this is to ensure that study sites are representative of all grant recipients. Then, it uses a statistical process to randomly assign some proportion of individuals seeking services to a treatment group and the remainder to a control group, which may not receive grant services. It compares outcomes of those in the treatment group to those in the control group.	Random assignment ensures that there will be no systematic differences between the average characteristics of the treatment and control groups, so any detected impacts can be ascribed to the treatment. Focusing on a subsample of grantees can reduce the heavy cost burden associated with random assignment of sample members at all grantees. If the number of grantees sampled is large enough, then stratification on dimensions of interest prior to grantee selection might allow for informative grantee-level subgroup analyses. It has external validity.	Compared with other designs, it takes a long time to implement and it may be difficult to implement after grantees have begun serving participants. These timing issues might influence either the feasibility of the approach or the size of the sample that can be included in the study. Grantees may not be comfortable withholding services for individuals assigned to the control group. This approach leads to a loss of statistical power due to the sampling of grantees, relative to random assignment at all grantees.	Very high

Table VI.2 *(continued)*

Approach	Summary of Approach	Strengths	Weaknesses	Methodological Rigor
Random assignment of individuals at a nonrandom subset of grantees	This approach selects a nonrandom subset of grantees on which to focus, perhaps because of a policy interest in the target groups, service plans, geographic locations, or other characteristics of the grantees. The approach then uses a statistical process to randomly assign some proportion of individuals seeking services to a treatment group and the remainder to a control group, which may not receive grant services. It compares outcomes of those in the treatment group with those in the control group.	Random assignment ensures that there will be no systematic differences between the average characteristics of the treatment and control groups, so any detected impacts can be ascribed to the treatment. It has more statistical power than if randomly subsampling sites first. Focusing on a subsample of grantees can reduce the heavy cost burden associated with random assignment of sample members at all grantees and enables the study to use rigorous methods to estimate program impacts, while still focusing on particular grantees of special interest to policymakers.	Compared with other designs, it takes a long time to implement, and it may be difficult to implement after grantees have begun serving participants. As with the other random assignment designs, these timing issues might influence either the feasibility of the approach or the size of the sample that can be included in the study. However, compared with including all or a random subset of grantees, a purposeful selection of grantees might slightly mitigate these problems. Grantees may not be comfortable withholding services for individuals assigned to the control group. The approach results in a small loss in statistical power relative to random assignment at all grantees. Results pertain only to those grantees in the subset under study; there is limited external validity.	Very high
Regression discontinuity (RD) with individuals in grantees that applied for funding (RD at the individual level)	This approach assumes that at least some grantees used an eligibility cutoff score (such as income) to determine which individuals were eligible to access services. Individuals with income on one side of a preset cutoff value are assigned to the comparison group; those with scores on the other side of the cutoff are assigned to the treatment group. To determine treatment effects, outcomes of comparison group members who are near the cutoff are compared with outcomes of treatment group members who are near the cutoff.	There is minimal interference with normal program activity. This approach can be implemented after programming has begun if adequate information about the sample members and their scores is known. There is less selection bias than in other nonexperimental designs because the selection rule for receiving services is fully known.	This approach requires larger sample sizes to achieve impact estimates with the same level of statistical power as in a random assignment study (approximately three to four times). There is limited external validity, with results applicable only to grant applicants near the cutoff score for which grant applicants receive funding. Cannot be used if eligibility cutoff is same as that used for other social programs.	High

Table VI.2 *(continued)*

Approach	Summary of Approach	Strengths	Weaknesses	Methodological Rigor
RD with individuals in grantees that applied for funding (RD at the grantee level)	This approach is based on the availability of grantee applicant scores based on a prespecified scoring process that was adhered to (scores must be continuous). Grantees with scores above a preset cutoff value are assigned to the treatment group; those with scores below the cutoff are assigned to the comparison group. Outcomes of individuals at grantees that just made the cutoff are compared with outcomes of individuals in grantee communities that just missed the cutoff to determine treatment effects.	There is minimal interference with normal program activity. This approach can be implemented after programming has begun if adequate information about the grant applicants and their scores is known. There is less selection bias than in other nonexperimental designs because the selection rule for grant receipt is fully known.	It might be challenging to access information on grant applicants that did not win grants, as well on individuals in their communities. It may be difficult to determine which individuals form the appropriate comparison at the "losing grantee sites". This approach requires larger sample sizes to achieve impact estimates with the same level of statistical power as in a random assignment study (approximately three to four times). There is limited external validity.	Moderate
Comparison group using other individuals in grantee sites	This approach uses a statistical procedure and baseline variables to match individuals who received the treatment (in this case, received training financed by the grant) to individuals who had a similar propensity to receive the treatment but did not do so. Average outcomes of those who received the treatment are compared with those of the matched comparison group.	There is minimal interference with normal program activity. This approach takes into account a wide range of observable characteristics to minimize the bias associated with impact estimates.	Because of the diversity of the target populations served by grantees, finding any credible comparison group would be challenging. Extensive background characteristics, including a baseline measure of the outcome variable, are needed in order for the match to be credible.	Moderate
Interrupted time series	This approach compares the average outcomes of individuals at winning grantee sites before the treatment (that is, winning the grant) with their outcomes after the treatment.	Because the outcomes of individuals are recorded over time, the pre-intervention period acts implicitly as a comparison group for the post-intervention outcomes.	This approach requires detailed baseline information data. Although not required, it also benefits from extensive historical information. It does not rule out other secular trends that may be causing changes in outcomes over time.	Low

Table VI.2 *(continued)*

Approach	Summary of Approach	Strengths	Weaknesses	Methodological Rigor
Community-Level Outcomes				
RD with grantees that applied for funding (RD at the grantee level)	This approach is based on the availability of grantee applicant scores based on a prespecified scoring process that was adhered to (scores must be continuous). Grantees with scores above a preset cutoff value are assigned to the treatment group; those with scores below the cutoff are assigned to the comparison group. Aggregate outcomes in communities at grantees that just made the cutoff are compared with those in communities at grant applicants that just missed the cutoff to determine treatment effects.	There is minimal interference with normal program activity. This approach can be implemented after programming has begun if adequate information about the grant applicants and their scores is known. It may be easier to collect outcome data at the grantee or community level from established data sources than at the individual level. There is less selection bias than in other nonexperimental designs because the selection rule for grant receipt is fully known.	It might be hard to access information on applicants that did not win grants. Depending on the variation in the chosen outcomes, there might be considerable statistical imprecision due to the use of a small number of grant applicants (compared with individual-level data analyses). Outcome measures must be chosen carefully to ensure there is a good relationship between grantees' expected participant-level outcomes and the community-level measures chosen for analysis. There is limited external validity.	High
Comparison group using other communities	This approach uses a difference-in-differences approach to compare the changes in aggregate outcomes in winning grantees with the changes in those outcomes in a comparison community that is similar. Statistical matching approaches can be used to identify similar comparison communities.	There is minimal interference with normal program activity. This approach can be implemented after programming has begun. It accounts for some secular trends in outcomes that may be occurring at the same time as the intervention.	It might be hard to define and collect data for a suitable comparison community for each grant type. This approach requires extensive data on baseline characteristics of grantees and potential comparison communities for the period preceding the intervention. Outcome measures must be chosen carefully to ensure there is a good relationship between grantees' expected participant-level outcomes and the community-level measures chosen for analysis.	Moderate

Notes: The information in this table summarizes different types of methodological approaches that might be used for evaluating the four types of ARRA grants that are part of the pre-design study. The first column names the approach; the second column provides a brief description of the approach. Columns three and four list the main strengths and weaknesses of each approach. The final column presents our assessment of the level of rigor represented by each approach. These assessments are based on commonly accepted standards for the rigor of research designs, such as those advanced by the U.S. Department of Education's Institute for Education Sciences' What Works Clearinghouse.

Individual-level outcomes are those measured and examined at the level of individuals receiving training financed by the grant; examples of these outcomes are employment status and quarterly earnings. Community-level outcomes are those measured and examined at the level of the whole community, rather than the individuals within it; an example is the number of new jobs created in target industries.

RD = regression discontinuity.

and the research questions that it intends to answer, however, the evaluation will need to take into account the unique environments in which the ARRA grants will likely operate, including both high rates of unemployment and other sources of training in many communities.

Random assignment is the most analytically rigorous of the possible methodologies, but it also is more likely than other approaches to interfere with normal program activity and to take longer to implement. A random assignment evaluation could provide the most convincing estimate of the individual-level impacts of access to services funded by the ARRA grants. However, if DOL would like to proceed with a random assignment approach, it will be important to ensure that grantees are willing to cooperate fully with the evaluation efforts. Furthermore, issuesrelated to the potential timing of when grantees provide services and when random assignment could begin will influence (1) the portion of grant participants that could be included in an evaluation and (2) whether or not random assignment is feasible for the two-year ETP and Pathways Out of Poverty grants.

With a random assignment approach, either random sampling or purposive selection of grantees might be desirable if cost considerations make it infeasible or undesirable to include all grantees in an evaluation. The most rigorous random assignment approach would include all grantees, but doing so is likely to be very expensive because of the number and geographic diversity of the grantees. Under certain assumptions, approaches that include in the study only a subset of grantees might be able to provide minimum detectible impacts (MDIs) that are in the same range as MDIs in other studies conducted for DOL. However, because we made many simplifying assumptions in our MDI calculations so that we could consider a range of different approaches, we caution that further investigation would be warranted to conclude definitively that a specific approach could yield acceptable MDIs.

If random assignment is not feasible or desirable, alternative methodologies could be considered. Excluding random assignment, a regression discontinuity (RD) approach provides the most analytical rigor. For this approach to be appropriate, there must be a scoring variable that is (or was) used to determine whether or not individuals or groups have access to grant-funded services. This approach could be conducted at the individual level, in which outcomes of individuals who are determined eligible for grant services based on a score measure are compared with outcomes of individuals who expressed interest in receiving grant services but who, based on the score, are determined ineligible for services. An RD design also could be conducted at the grantee level, in which outcomes for either individuals or communities affiliated with successful grant applicants are compared with individuals or communities affiliated with unsuccessful grant applicants. Regardless of the details of the RD approach, however, important challenges that would need to be addressed during a design phase of an evaluation using an RD approach would include the need to ensure that (1) the comparison group is defined sensibly, especially given the variability in grantees' target groups; (2) certain technical requirements about how the scoring process is (or was) conducted are met; and (3) the necessary data can be made available for the evaluation.

Other types of methodologies, such as those using individual- or community-level comparison groups or those that do not use a comparison group, could be used for an evaluation but are less likely to provide convincing impact estimates. An important limitation of comparison group approaches is that the evaluation would be unable to state definitively that differences in outcomes between the group that has access to grant-funded services and the group that does not are not caused by unobservable differences between the groups. For the design comparing pre- and post-treatment outcomes without a comparison group, the evaluation would be

unable to state definitively that changes in outcomes over time were caused by the intervention, and not by other factors occurring at the same time.

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APPENDIX A

KEY FEATURES OF AWARDED GRANTS

Table A.1. Energy Training Partnership Grant Recipients

Grant Recipient	Location(s) Served	Award Amount	Populations Served	Serving an Auto Restructuring Area?	National or Local Grant Type	Primary Focus and Outcome
1. Northwest Energy Efficiency Council	5 counties and 4 cities in Washington State	$3,876,171	Older youth, dislocated workers, incumbent workers, veterans, women, and individuals with disabilities	No	Local	473 trained and placed in energy efficiency occupations in industries such as energy efficient building construction and retrofitting and energy efficient assessment.
2. United Auto Workers Labor Employment and Training Corporation (UAW-LETC)	2 counties in Missouri	$3,200,000	Veterans, ex-offenders, individuals with disabilities, and women	Yes	Local	Train and place approximately 430 dislocated and incumbent workers in auto-related and commercial energy fields, such as hybrid/electric auto technicians, electric auto/truck battery technicians, electric motors/devises technicians, and commercial energy technicians.
3. H-CAP Inc.	2 counties in California, the District of Columbia, 2 counties in Maryland, 5 counties in New York, and 1 county in Washington State	$4,637,551	Immigrants and minorities	Yes	National	Approximately 3,000 job seekers and environmental service workers will receive training and approximately 2,700 entry-level workers will receive certificates and training.
4. Utility Workers Union of America (UWUA), AFL-CIO	3 counties in California, 3 counties in Massachusetts, and 6 counties in New Jersey	$4,993,922	Women, minorities, older youth, and incumbent workers	Yes	National	Approximately 672 participants will be trained and placed in green and emerging occupations in the utility industry. Will operate through pre-apprenticeship and registered apprenticeship programs.
5. International Transportation Learning Center	Statewide in New Jersey, New York, and Utah; Columbus, Ohio	$5,000,000	Minorities	No	National	Train 3,640 new and incumbent workers in the public transportation industry. Will include apprenticeships.

Table A.1 (continued)

Grant Recipient	Location(s) Served	Award Amount	Populations Served	Serving an Auto Restructuring Area?	National or Local Grant Type	Primary Focus and Outcome
6. California State Labor Management Cooperation Committee for the International Brotherhood of Electrical Workers and the National Electrical Contractors Association (LMCC-IBEW-NECA)	Statewide in California	$5,000,000	Unemployed and underemployed electricians	Yes	Local	Training to 2,292 journey-level electricians.
7. Central Vermont Community Action Council Inc.	All 14 counties and 256 minor civil divisions in Vermont	$4,846,195	Unemployed, underemployed, veterans, high school dropouts, women, and individuals with disabilities	No	Local	Provide intensive case management and training to 398 individuals and training to nearly 2,000 others, focusing on energy efficiency and renewable energy industries in construction, recycling, and waste reduction.
8. E.C.I.A. Business Growth Inc.	27 counties in Iowa and portions of Minnesota and Wisconsin	$2,060,250	Unemployed and dislocated workers, high school graduates, veterans, disconnected youth, women, and minorities	Yes	Local	Will serve 392 participants with 344 completing training, through 3 career ladders related to electrical power, energy efficient assessment, and energy efficient building.
9. Institute for Career Development (ICD) Inc.	7 counties in Indiana, and 1 county each in New York, Ohio, and Pennsylvania	$4,658,983	Dislocated steelworkers	Yes	National	Will serve 2,000 participants, with expectations that 1,200 will complete training in fields such as wind farm field technicians, solar panel installers, or geothermal installers; 800 of the 1,200 will receive a credential for employment in energy generation such as construction, HVAC, and home retrofitting.

Table A.1 (continued)

Grant Recipient	Location(s) Served	Award Amount	Populations Served	Serving an Auto Restructuring Area?	National or Local Grant Type	Primary Focus and Outcome
10. National Ironworkers and Employers Apprenticeship Training and Journeyman Upgrading Fund	20 counties in the 5 states affected by auto restructuring: areas within 400 miles of La Palma, Calif.; Joliet, Ill.; West Seneca, N.Y.; Arlington, Texas; and Salt Lake City, Utah	$1,943,931	Dislocated workers	Yes	National	At least 510 ironworkers will begin training for increasing skills in the renewable wind energy sector.
11. Blue Green Alliance	7-county metro area around the Twin Cities, 8 counties in northeastern Minnesota, and 38 counties in southern Minnesota	$5,000,000	Dislocated workers, women, minorities, veterans, incumbent workers, and unemployed steelworkers	Yes	Local	Will enroll approximately 2,060 workers in training in green manufacturing techniques.
12. Oregon Manufacturing Extension Partnership	6 counties in Oregon and 3 counties in Washington	$5,000,000	Unemployed and dislocated workers and employed workers at local renewable energy industries	No	Local	It is expected that approximately 1,670 participants will complete training and 1,325 will receive a credential in the renewable electric power and biofuels fields.
13. SER Metro-Detroit, Jobs for Progress Inc.	Wayne County and Detroit, Michigan	$4,298,673	Women, minorities, and veterans	Yes	Local	Approximately 340 unemployed workers will be trained for green jobs and apprenticeship opportunities in alternative energy opportunities in building construction and retrofitting, solar, weatherization, and electrical industries.
14. The Providence Plan	Providence, Rhode Island	$3,720,000	Ex-offenders, minorities, and dislocated workers	No	Local	Approximately 1,600 low-skilled Providence residents will complete training in pre-apprenticeship, green apprenticeship, and journey-level skill upgrades in energy-efficient building construction and retrofitting and wind-generated electrical power. 800 participants will be placed in green energy jobs.

Table A.1 (continued)

Grant Recipient	Location(s) Served	Award Amount	Populations Served	Serving an Auto Restructuring Area?	National or Local Grant Type	Primary Focus and Outcome
15. Montana Electrical Joint Apprenticeship & Training Council (MEJATC)	Statewide in Montana	$5,000,000	Unemployed workers and incumbent workers	No	Local	Approximately 2,450 participants will complete training and receive credentials as pre-apprentices, apprentices, or journeymen workers in energy efficiency and renewable energy industries, such as building construction and renewable electric power.
16. Communications Workers of America (CWA) National Education and Training Trust	8 counties in Ohio	$3,969,056	Dislocated workers and veterans	Yes	National	Will provide 1,000 dislocated workers with short-term training opportunities in advanced, green manufacturing skills for work in emerging energy-related, energy storage, and clean manufacturing environments.
17. Heritage Health Foundation	Upper Monongahela Valley region of Southwestern Pennsylvania	$1,408,601	Underemployed and unemployed workers, veterans, women, and minorities	Yes	Local	Will enroll 120 trainees for work in industries such as deconstruction, building resource recovery, energy management, weatherization, and urban eco-restoration. Aims to have 65% of graduates in unsubsidized employment for at least 6 months.
18. Thomas Shortman Training Scholarship and Safety Fund	All 5 boroughs of New York City, New York	$2,802,269	Incumbent workers and minorities	No	Local	Will train 2,000 building superintendents on how to make large apartment buildings energy efficient. Also will train 30 commercial building operating engineers and 170 building analysts to benchmark the energy performance of large buildings.
19. Memphis Bioworks Foundation	21 counties in Tennessee and 5 counties in Arkansas	$2,931,103	Dislocated workers	Yes	Local	Will train 25 participants through their renewable energy training program ; up to 400 participants will receive customized training in solar installation techniques.
20. International Training Institute for the Sheet Metal and Air Conditioning Industry	5 counties in Michigan; 4 counties in Ohio; St. Louis, Missouri; East St. Louis, Illinois; 5 cities in California; 3 counties in New Mexico; and 2 counties in Texas	$4,995,188	Unemployed and underemployed individuals, veterans, minorities, and women	Yes	National	Expects that 1,200 unemployed and incumbent sheet metal workers will successfully complete customized training and transition into employment in energy efficient occupations for careers in building construction, retrofitting, and manufacturing.

Table A.1 (continued)

Grant Recipient	Location(s) Served	Award Amount	Populations Served	Serving an Auto Restructuring Area?	National or Local Grant Type	Primary Focus and Outcome
21. Labor's Community Action Inc.	Statewide in Colorado, but primarily serving Denver and Colorado Springs and, through online training, rural residents	$3,604,162	Dislocated workers, incumbent workers, and newly trained workers	No	Local	Expects to enroll 1,913 incumbent, newly trained, and unemployed construction workers and place 80 percent of them into unsubsidized employment in the energy efficient building, renewable electric power, and energy efficiency assessment industries. Training will be through either certificate upgrades, registered apprenticeship training, or pre-apprenticeship or basic skills development.
22. Austin Electrical Joint Apprenticeship Training Committee (JATC)	Statewide for Arizona; Oklahoma; Kansas; New Mexico; and Texas	$4,842,424	Unemployed workers and incumbent workers	Yes	Local	Approximately 1,000 individuals will be trained in solar-specific and smart-grid electrical competencies. Expects to place or retain 672 workers in employment.
23. Community Housing Partners Corporation	15 counties and 5 cities in Virginia	$3,865,480	Unemployed and dislocated workers	Yes	Local	Will serve approximately 380 participants; expects that 280 participants will complete education and training activities and obtain employment in energy-efficient building, construction, retrofit, and energy efficiency assessment.
24. Broward County Minority Builders Coalition	Broward County, Florida	$3,280,656	Unemployed workers, veterans, women, ex-offenders, and minorities	No	Local	Will recruit 1,000 participants and expects 700 to complete training requirements to be placed into jobs in five occupations: solar thermal system designer and installer; solar PV (photovoltaic) designer and installer (skilled); solar PV system installer (entry level); weatherization technician/installer; and LEED (Leadership in Energy and Environmental Design) Green Associate.
25. Ohio Electrical Labor Management Cooperative Committee	44 counties in Ohio	$4,826,073	Incumbent and dislocated workers, including veterans	Yes	Local	Using a registered apprenticeship model, will provide training for 1,288 dislocated and incumbent workers who will earn credentials and college credit, focusing on energy-efficient building, construction, and retrofit in combination with the renewable electric power sector.

Source: DOL's announcement of grant awards, retrieved from http://www.doleta.gov/ETA_News_Releases/20091526.cfm and http://www.doleta.gov/pdf/ETP_SGA_Award_Summaries_120409.pdf and accessed on January 27, 2010.

AFL-CIO = American Federation of labor-Congress of Industrial Organizations; HVAC = heating, ventilation, and air conditioning.

Table A.2. Pathways Out of Poverty Grant Recipients

Grant Recipient	Location Served	Award Amount	Participants Served	Type of Grant	Primary Focus and Outcome
1. Alternative Opportunities Inc.	4 PUMAs in St. Louis, MO	$2,308,200	High school dropouts, unemployed individuals, ex-offenders, and veterans	Local	200 participants enrolled to gain entry-level skills through an intensive five-module training program about building performance, weatherization/retrofitting, HVAC basics, solar thermal installation, and solar photovoltaic (PV) installation.
2. Better Family Life Inc. (BFL)	3 PUMAs in St. Louis, MO	$3,305,493	Unemployed individuals, high school dropouts, and individuals with criminal records	Local	900 participants will be trained for careers as weatherization technicians/installers, solar PV systems installers, LEED green associates, and biofuels collection technicians.
3. Boley Centers Inc.	1 PUMA in St. Petersburg, FL	$2,300,678	Disadvantaged and unemployed urban youth	Local	150 participants will take part in technology training and certification in electricity and plumbing.
4. Citrus Levy Marion Regional Workforce Development Board Inc.	1 PUMA in Ocala, FL	$2,985,175	Unemployed workers, low-income adults, high school dropouts, and individuals with a criminal history	Local	665 participants will be trained for certification and employment in green jobs such as solar installation, energy auditing, weatherization, green building products installation, organic gardening, and xeriscape growing and landscaping.
5. City of Minneapolis	5 PUMAs in Minneapolis and St. Paul, MN	$4,000,000	Individuals living in poverty, veterans, and unemployed young adults who do not have a high school diploma	Local	500 participants served in industries related to energy efficient building, construction, and retrofit; energy efficient building maintenance; deconstruction and materials use, recycling, and waste reduction; and sustainable manufacturing.
6. CNY Works Inc.	1 PUMA in Syracuse, NY	$3,715,931	Low-income individuals, ex-offenders, disadvantaged young-adults, and displaced workers	Local	750 participants will receive increased employment options in burgeoning green industries, particularly energy efficiency and bio-fuels.
7. Community College of Philadelphia	2 PUMAs in Philadelphia, PA	$3,184,428	Unemployed workers, ex-offenders, and veterans	Local	250 participants will be served in this program that will implement two entry-level green collar training programs in green manufacturing and construction/weatherization.
8. Consortium for Worker Education	2 PUMAs in Bronx, NY	$4,000,000	Individuals with limited English proficiency, veterans and eligible spouses, persons with criminal records, disconnected youth, and women	Local	425 participants will take a pretraining course in sustainable mechanical and retrofitting technologies and employment fundamentals and, upon completion, can choose from more than 30 different courses in energy efficiency and renewable energy.

Table A.2 (continued)

Grant Recipient	Location Served	Award Amount	Participants Served	Type of Grant	Primary Focus and Outcome
9. East Harlem Employment Services Inc. doing business as STRIVE	4 PUMAs in New York, NY; 3 PUMAs in Philadelphia, PA; 1 PUMA in Hartford, CT; 1 PUMA in Benton, MI; 1 PUMA in Flint, MI; and 2 PUMAs in Baltimore, MD	$4,728,419	Unemployed individuals, high school dropouts, and individuals with a criminal record	National	Will serve 1,819 participants with a comprehensive green construction training program.
10. Eastern Maine Development Corp.	1 PUMA in Piscataquis County and Penobscot County, ME	$2,109,088	Disadvantaged adult job seekers, dislocated workers, returning offenders, public assistance recipients, high school dropouts, and veterans	Local	Will provide 105 workers with retrofit and green construction opportunities driven by policies focused on housing retrofits and energy reduction.
11. Florida State College at Jacksonville	1 PUMA in Duval County, FL	$2,229,642	Unemployed individuals, high school dropouts, and individuals with a criminal record	Local	Will serve 390 participants to help prepare them for the energy efficient building construction and retrofit, renewable electric power, and energy efficient assessment industries.
12. Goodwill Industries International (GII)	3 PUMAs in Grand Rapids, MI; 1 PUMA each in Atlanta, GA; Austin, TX; Charlotte, NC; Phoenix, AZ; and Washington, DC	$7,303,634	People with disabilities, chronically unemployed individuals, ex-offenders, older workers, homeless individuals, and high school dropouts	National	Will serve 1,300 participants to help them prepare for careers in the energy efficient building construction and retrofit and renewable energy industries.
13. Grand Rapids Community College	1 PUMA in Grand Rapids, MI	$4,000,000	Unemployed workers, high school dropouts, and individuals with criminal records	Local	Will serve 1,080 participants with a focus on energy efficient building construction and retrofit, renewable energy, energy efficiency, green facilities management, and advanced battery manufacturing industries.
14. It's My Community Initiative	1 PUMA in Oklahoma City, OK	$4,000,000	Underemployed individuals and ex-offenders	Local	Will serve 236 participants, focusing on participants through industry-developed wind energy and recycling technician certification programs and on-the-job training programs.

Table A.2 (continued)

Grant Recipient	Location Served	Award Amount	Participants Served	Type of Grant	Primary Focus and Outcome
15. Jobs for the Future Inc. (JFF)	3 PUMAs in Chicago, IL; 3 PUMAs in Detroit, MI; 10 PUMAs in Los Angeles, CA; 1 PUMA in Milwaukee, WI; 2 PUMAs in Philadelphia, PA	$7,997,936	Unemployed and disadvantaged individuals	National	Will serve 1,130 participants for careers in energy efficient building, construction and retrofit and renewable electric power industries.
16. Lehigh Valley Workforce Investment Board Inc.	1 PUMA in Allentown, PA	$4,000,000	At-risk youth, veterans and eligible spouses, and underemployed and unemployed individuals	Local	Will serve 200 participants, focusing on occupations in energy efficient building, construction, and retrofit industries; renewable electric power; and energy efficiency assessment industries.
17. Los Angeles Community College District (LACCD)	3 PUMAs in the Los Angeles, CA communities of Watts, Willowbrook, and Florence-Graham	$4,000,000	Dislocated, unemployed, underemployed, low-income workers, and veterans	Local	Will train 925 participants for careers in the following occupations: green plumbing contractor, green construction manager/building analyst, HVAC contractor, solar PV or solar thermal installer, and energy efficiency auditor and installer.
18. MDC Inc.	4 PUMAs in Charlotte, NC; 1 PUMA in North Charleston, SC; 1 PUMA in Orangeburg, Calhoun, and Bamberg Counties, SC; 1 PUMA in Wise and Dickenson Counties, VA; 1 PUMA in Scott County, VA	$3,780,816	Low-wage workers and unemployed individuals	National	Will train 700 individuals with a focus on weatherization and alternative energy.
19. Mi Casa Resource Center for Women Inc.	1 PUMA in Denver, CO	$3,633,195	Unemployed individuals, high school dropouts, individuals with a criminal record, women, and minorities	Local	Will serve 500 participants with a focus on the following industries: energy efficient building construction & retrofits, renewable electrical power, deconstruction & materials use, and energy efficiency assessment (residential, commercial, & industrial).

Table A.2 (continued)

Grant Recipient	Location Served	Award Amount	Participants Served	Type of Grant	Primary Focus and Outcome
20. Mott Community College (MCC)	2 PUMAs in Flint, MI and adjoining suburbs	$3,662,403	Low-income individuals	Local	Will allow 200 participants to take part in a pre-skills training course and a 12-course training series covering the basic skills and knowledge needed to enter employment in the green construction trades, as well as the Energy Conservation Apprentice Readiness (ECAR) program and the Road Construction Apprenticeship Readiness (RCAR) program.
21. Moultrie Technical College	1 PUMA in Tift County, GA	$3,753,579	Individuals on probation, high school dropouts, residents with disadvantaged backgrounds, and displaced workers	Local	Will provide education and training activities to 260 participants for employment in three areas of the energy industry: biofuels, energy efficiency assessment, and renewable electrical power (solar, wind, biomass, and geothermal).
22. National Association of Regional Councils (NARC)	1 PUMA each in Apache Junction, AZ; Bisbee, AZ; Odessa, TX; and Dayton, OH; 2 PUMAs in Midland, TX	$7,994,999	Limited English proficiency individuals, Native Americans, and ex-offenders	National	Will train and place approximately 500 individuals in the building performance; energy-efficient building, construction, and retrofit; renewable electric power; and deconstruction and materials use industries.
23. National Council of La Raza	2 PUMAs each in San Jose, CA and Chicago, IL; 1 PUMA in San Diego, CA	$3,063,839	Low-income and unemployed individuals, and individuals with limited English proficiency	National	Will train and place approximately 161 participants in unsubsidized employment within the energy efficiency and clean energy industries.
24. Northern Rural Training and Employment Consortium (NoRTEC)	1 PUMA each in Butte, CA; Del Norte, Lassen, Modoc, and Siskiyou, CA; Shasta County, CA; and Tehama and Trinity, CA	$4,000,000	High school dropouts, at-risk youth, welfare recipients, individuals with a criminal record, unemployed and dislocated workers, and veterans	Local	For 554 participants, will provide training programs including the California Clean Energy Workforce Training Program and Green Building Pre-Apprenticeship Program.
25. Opportunities Industrialization Centers of America Inc.	1 PUMA each in Asheville, NC and Phoenix, AZ; 2 PUMAs in Broward County, FL	$4,900,000	Unemployed individuals, high school dropouts, and individuals with criminal records	National	Will train 1,350 participants for occupational areas including weatherization technician/installer; advanced and entry-level solar PV systems installer; LEED green associates; and biofuels collection and rendering technicians.

Table A.2 (continued)

Grant Recipient	Location Served	Award Amount	Participants Served	Type of Grant	Primary Focus and Outcome
26. PathStone Corp.	1 PUMA each in Rochester, NY; Scranton, PA; Juana Diaz, Santa Isabel and Villalba, PR; and Arroyo, Coamo, Guayama and Salinas, PR	$8,000,000	Unemployed individuals, high school dropouts, and individuals with criminal records	National	Will serve 1,176 participants. Will focus on deconstruction (NY); retrofit, welding, and diesel mechanics (PA); and recycling plastics and hazardous materials (PR).
27. Private Industry Council of Westmoreland/Fayette Inc.	1 PUMA in Fayette County, PA	$2,732,719	Unemployed individuals, high school dropouts, and individuals with criminal records	Local	Will serve 250 participants for careers in the energy efficient building industry, renewable electric power industry, and energy efficiency assessment industries.
28. Providence Economic Development Partnership	1 PUMA in Providence, RI	$2,489,111	Ex-offenders and low-literacy individuals	Local	Will serve 300 participants who can gain up to 5 certifications and employment in the deconstruction, energy efficiency, and green construction industries.
29. Roca Inc.	1 PUMA in Chelsea and Revere, MA	$2,398,778	High-risk youth	Local	Will serve 225 individuals. This comprehensive program will integrate academic instruction with occupational training related to the energy efficient building construction and retrofit industry; energy efficiency assessment; and sustainable manufacturing.
30. SER – Jobs for Progress of the Texas Gulf Coast Inc.	8 PUMAs in Houston, TX	$3,122,554	High school dropouts, ex-offenders, unemployed individuals, and disadvantaged individuals	Local	300 participants will receive occupational training in one of three occupations: (1) weatherization worker technician coupled with green advantage certification; (2) solar panel installer; or (3) energy rater/whole house diagnostics.
31. Southeast Community College Area	1 PUMA in Lincoln, NE	$2,331,278	Unemployed individuals, veterans, high school dropouts, individuals with criminal records, refugees, and immigrants	Local	Will serve 400 eligible participants for entry-level positions in the energy efficient construction industry.
32. Southwest Housing Solutions Corp. (SWHS)	2 PUMAs in Southwest Detroit, MI	$4,000,000	Unemployed individuals, high school dropouts, individuals with criminal records, and veterans	Local	360 trainees will be placed into jobs after participating in training programs in weatherization/deconstruction, landscaping/forestry, and urban agriculture.
33. West Hills Community College District	3 PUMAs in communities in or near Fresno and Kings Counties, CA	$3,000,000	Disadvantaged individuals	Local	Training efforts will result in degrees and certificates for 150 participants, in fields such as solar energy, water management, sustainable manufacturing, and construction.

Table A.2 (continued)

Grant Recipient	Location Served	Award Amount	Participants Served	Type of Grant	Primary Focus and Outcome
34. Western Iowa Tech Community College (WITCC)	1 PUMA in Woodbury County, IA	$3,999,459	Dislocated workers, low-income adults, and disconnected youth	Local	Will train 300 workers into one of areas related to retrofitting, renewable energy, and agricultural production; and other energy-intensive industries.
35. White Earth Band of Chippewa	1 PUMA in Mahnomen, Clearwater, and Becker counties, MN	$3,086,817	Native American high school dropouts, unemployed individuals, and individuals with ,,a criminal records	Local	Will serve 240 participants for careers in the following industries: energy efficient building, construction, and retrofit; energy efficiency assessment; and sustainable manufacturing.
36. Workforce Development of Seattle-King County	2 PUMAs in Southeast Seattle, WA	$3,639,530	High school dropouts, unemployed adults, veterans, previously incarcerated youth and adults, and other disadvantaged individuals — with a specific focus on communities of color, individuals with limited English proficiency and individuals with disabilities	Local	Will connect 450 participants with either short-term, introductory green courses and safety certifications for green construction, manufacturing, and weatherization industries or long-term training initiatives related to construction, green modular manufacturing, and a green manufacturing employment program.
37. The WorkPlace Inc.	1 PUMA in Bridgeport, CT	$4,000,000	High school dropouts, individuals with criminal records, unemployed individuals, and people facing other significant disadvantages	Local	Will serve 700 participants and will provide an overview of the core sectors of the green economy and then enable them to move into suitable training programs.
38. Worksystems Inc.	2 PUMAs in East Multnomah County, OR	$4,000,000	Native Americans, African Americans, Latinos, immigrants, veterans, individuals with criminal records, and homeless individuals	Local	Will serve 360 participants. Will enable them to obtain certificates in green manufacturing, renewable energy systems, and weatherization.

Source: DOL's announcement of grant winners found at http://www.doleta.gov/ETA_News_Releases/20100039.cfm, and http://www.doleta.gov/pdf/Pathways_Poverty_grants.pdf and accessed on January 27, 2010.

HVAC = heating, ventilating, and air conditioning; LEED = Leadership in Energy and Environmental Design; PUMA = public use microdata area; PV = photovoltaic.

Table A.3. State Energy Sector Partnership Grant Recipients

Grant Recipient	State	Auto-Affected Counties Served	Award Amount	Participants Served	Primary Focus and Outcome
1. Alabama Department of Economic and Community Affairs (ADECA)	Alabama	Montgomery, Tuscaloosa, Lowndes, Butler, Madison, Limestone, and Marshall	$6,000,000	Veterans, at-risk youth, ex-offenders, dislocated workers, incumbent workers, low-income workers, and disadvantaged individuals	Will prepare individuals for careers in five energy efficiency and renewable energy industries and establish the Alabama Center for Renewable Energy Sector Training. Will train approximately 1,442 individuals.
2. Alaska Department of Labor and Workforce Development	Alaska	None	$3,600,000	Minorities, recipients of public assistance, veterans, people with disabilities, low-income individuals, unemployed and underemployed individuals, dislocated workers, out-of-school youth, and incumbent workers	Will implement a sustainable plan to increase the supply of workers with energy-efficiency skills to support energy-efficient end-user technology and the geothermal, hydroelectric, wind turbine, and biomass industries. At least 700 workers will receive training.
3. Arizona Department of Economic Security	Arizona	None	$5,000,000	Dislocated workers, underemployed workers, veterans, incumbent workers, at-risk youth, individuals with criminal records, and individuals with disabilities	Approximately 1,502 participants will complete education and training activities. Participants will earn industry-recognized weatherization and energy-efficiency certificates, computer-aided drafting sustainability certificates, solar installer photo voltaic certificates, thermal certificates, and green building design and construction certificates.
4. Arkansas Workforce Investment Board/Department of Workforce Services	Arkansas	Cleburne, Desha, Greene, and Logan	$4,866,479	High school dropouts, offenders, unemployed workers, and other disadvantaged, at-risk individuals	Will create three energy centers of excellence that will develop and deploy materials and programs to be used by 22 two-year colleges and 7 apprenticeship programs. Will recruit 2,800 participants for referral to the training programs.
5. State of California Employment Development Department	California	Alameda and Los Angeles	$6,000,000	Dislocated workers, veterans, unemployed and underemployed workers, low-income youth and adults, new workforce entrants, individuals with disabilities, and individuals with criminal records	Each of five regional programs will offer training courses that will cover basic construction principles, principles of environmental literacy, energy fundamentals, installation of solar hot water heaters, and building retrofits. A total of 1, 000 participants will be served. Following training, participants will be placed in registered apprenticeship programs and jobs.

Table A.3 (continued)

Grant Recipient	State	Auto-Affected Counties Served	Award Amount	Participants Served	Primary Focus and Outcome
6. Colorado Department of Labor and Employment	Colorado	None	$5,998,050	Older workers, dislocated workers, incumbent workers, veterans, women, individuals with disabilities, Temporary Assistance for Needy Families (TANF) recipients, dislocated workers, and ex-offenders	Will prepare workers for jobs in the new energy economy through several activities: an energy sector entrepreneurial pilot project; a statewide apprenticeship partnership program; and a green training curriculum to create hands-on training in energy efficiency audits, wind turbine safety, and solar energy; energy scholarships for participants; youth conservation corps; and a youth outreach campaign. Will serve 1,200 participants.
7. Connecticut Employment and Training Commission	Connect-icut	None	$3,360, 000	Incumbent workers, unemployed workers, veterans, at-risk youth, ex-offenders, underrepresented ethnic minorities and recent immigrants	Will serve 895 participants, with a focus on energy efficiency assessment, green manufacturing, and environmental protection.
8. Hawaii Department of Labor and Industrial Relations	Hawaii	None	$6,000,000	Incumbent workers, low-income/low-skilled individuals, disadvantaged persons with limited English proficiency, people with disabilities, and veterans	Approximately 1,391 participants will complete education and training activities and attain either certificates or licenses, such as the HERS, LEED, solar photovoltaic installation, biofuels processing, and power plant operation.
9. Idaho Department of Labor	Idaho	None	$5,991,184	Veterans, dislocated workers, low-skill adults and youth, and other targeted populations	Will be used to create a comprehensive plan that will upgrade existing post-secondary and secondary curricula; strengthen dual-credit articulation; and provide sustainable training programs to better prepare participants for careers in energy efficiency and renewable energy industries. Will serve more than 1,400 participants.
10. Illinois Department of Commerce and Economic Opportunity	Illinois	Boone, Cook, and McLean	$6,000,000	Low-skilled individuals, new labor market entrants, dislocated workers, incumbent workers, underemployed individuals, veterans, minority contractors, disadvantaged adults and youth, individuals with disabilities, and individuals with criminal records	Approximately 1,310 participants will be trained and placed in energy efficiency occupations. Additionally, 1,694 program completers will earn industry-recognized degrees, such as building analyst and energy auditor.
11. Indiana Department of Workforce Development	Indiana	45 counties[a]	$6,000,000	Incumbent and dislocated workers, unemployed workers, adults with barriers to employment, and out-of-school youth	Will establish an Advanced Energy Training Center to facilitate the state's transition to a green economy; will develop curriculum and coordinate traditional and alternative energy technology programs. Will have 2,190 program completers and 2,075 individuals gaining employment in green-related fields.

Table A.3 (continued)

Grant Recipient	State	Auto-Affected Counties Served	Award Amount	Participants Served	Primary Focus and Outcome
12. Iowa Workforce Development	Iowa	Cass, Chickasaw, Fremont, Howard, Iowa, Louisa, Plymouth, Poweshiek, Union, and Wright	$5,997,000	Dislocated workers, unemployed individuals, and incumbent workers	Will implement a state-driven green workforce development plan with prioritized training needs, as identified by the state's green jobs task force.
13. Kansas Department of Commerce	Kansas	Allen	$5,999, 890	Dislocated workers, incumbent workers, veterans, older youth, former prisoners, and individuals seeking career pathways out of poverty	Will recruit candidates to be trained in the renewable energy operation and construction, renewable energy manufacturing and supply chain, energy transmission, biomass, and green construction industries. Will recruit 1,580 individuals, with 1,316 entering training, and 1,053 completing training and earning certification.
14. Education and Workforce Development Cabinet	Kentucky	Cumberland, Fulton, Hopkins, Pulaski, and Trigg	$4,740,457	Dislocated workers, unemployed individuals, out-of-school youth, and veterans	Proposed trainings include education for energy auditors/raters, Smart Grid technology installations, and maintenance professionals; pipefitters; and steamfitters who will receive a Green Systems Awareness certification. Approximately 444 participants will complete training and be placed in energy efficient occupations.
15. Maryland Department of Labor, Licensing and Regulation	Maryland	None	$5,793,183	Incumbent and dislocated workers, veterans and reservists, low-wage workers, and individuals with criminal records	Will improve manufacturing sustainability practices, waste stream management, and lean-to-green practices in the manufacturing sector; develop and expand green construction training options by providing training for a wide range of construction-related trades and green building; and assist workers as they pursue environmental technology careers. Approximately 1,585 participants will be trained and placed into energy efficient employment.
16. Commonwealth of Massachusetts, Executive Office of Labor and Workforce Development	Massa-chusetts	None	$5,973,657	Unemployed, underemployed, and incumbent workers	Will implement a training strategy with the following four key components: (1) target industry sectors for which the state's energy policy is acting as a driver to grow jobs; (2) target occupations with a business demand for workers who can be prepared in fewer than three years; (3) leverage the investments made through state energy training funds; and (4) leverage the workforce investment system. Will serve 1,379 participants.

Table A.3 (continued)

Grant Recipient	State	Auto-Affected Counties Served	Award Amount	Participants Served	Primary Focus and Outcome
17. Michigan Department of Energy, Labor, & Economic Growth	Michigan	Midland, Saginaw, Genesee, Bay, Roscommon, Losco, Livingston, Monroe, Wayne, Clair, Macomb, and Oakland	$5,819,999	Dislocated workers and incumbent workers	Will implement the Governor's Green Jobs Initiative and provide training in advanced energy storage, solar industries, and energy efficient construction to 1,282 participants.
18. Minnesota Department of Employment and Economic Development	Minnesota	Clearwater and Ramsey	$6,000,000	Unemployed workers, incumbent workers, individuals with limited English proficiency, low-income individuals, individuals with disabilities, individuals with criminal records, high school dropouts, youth, dislocated workers, veterans, and minority populations	Will serve 1,495 participants in the following industries: energy efficient building, construction, and retrofit; renewable energy; and biofuels.
19. Missouri Division of Workforce Development	Missouri	St. Louis, St. Charles, Lincoln, Clay, and Randolph	$6,000,000	Veterans and active duty military personnel, incumbent workers, and unemployed workers	Will provide 480 incumbent journeymen and apprentices with enhanced qualifications; 120 veterans and/or qualified military spouses will complete a Renewable Energy Apprenticeship Program; 150 participants will graduate from renewable energy degree programs at community colleges, and 60 will complete minors or certificate programs from within degree-granting engineering programs.
20. Nebraska Department of Labor	Nebraska	Dawson	$4,839,511	Workers affected by energy policy, incumbent workers, veterans, unemployed individuals, at-risk youth, and individuals with criminal records	Will serve 950 participants with training for careers in wind, biofuels, and green sustainable building technologies industries.
21. Nevada Department of Employment, Training and Rehabilitation	Nevada	None	$6,000,000	Dislocated workers, individuals with criminal records, individuals with disabilities, individuals who are homeless, veterans, former foster youth, young pregnant and single mothers, individuals receiving public assistance, and other low-income individuals	Will serve 7,125 participants with training for careers in energy efficiency, renewable energy, and other green industries.

Table A.3 (continued)

Grant Recipient	State	Auto-Affected Counties Served	Award Amount	Participants Served	Primary Focus and Outcome
22. New Jersey Department of Labor and Workforce Development	New Jersey	Middlesex and Union	$6,000,000	Out-of school youth, adults with limited basic skills, individuals with criminal histories, and individuals with disabilities	Has developed a diverse portfolio of training projects that will improve participants' access to mid-level career-path jobs; provide skill upgrades to incumbent workers to help them maintain employment and move along career paths; open new positions for unemployed and disadvantaged populations; attract younger workers and nontraditional populations to key occupations; and ensure accountability among funded training programs. Will serve about 904 participants.
23. New Mexico Department of Workforce Solutions	New Mexico	None	$5,999,989	Incumbent workers, unemployed workers, women, veterans, military spouses, and high school dropouts	Establishes a training framework with universal access to lifelong learning based on relevant local and regional labor market needs. Approximately 400 participants will be trained for solar, wind, energy efficient building, construction and retrofit, and biofuels employment.
24. North Carolina Department of Commerce, Division of Workforce Development	North Carolina	Scotland	$5,976,512	Unemployed workers, military spouses, veterans, at-risk youth, individuals with criminal records, farmers, and migrant and seasonal workers	Will provide 1,137 participants with training that leads to industry-recognized certificates in energy efficient building, construction and retrofitting industries, and energy efficiency assessment.
25. State of Ohio	Ohio	44 counties[a]	$6,000,000	Dislocated workers, veterans, women, minorities, and Appalachian residents	Focusing on wind, solar, and biomass industries, will serve about 1,600 participants and offer "stackable certificates" that can be rearranged to offer both employers and students the flexibility that is needed in a rapidly changing economy.
26. State of Oklahoma	Oklahoma	Oklahoma and Marshall	$6,000,000	Unemployed workers and incumbent workers	Will create a Center of Excellence for Energy Innovation at Tulsa Community College Northeast Campus, to provide state-of-the-art practical applications in building retrofitting and green landscaping. Will serve 1,200 participants.

Table A.3 (continued)

Grant Recipient	State	Auto-Affected Counties Served	Award Amount	Participants Served	Primary Focus and Outcome
27. State of Oregon	Oregon	None	$5,383,568	Unemployed workers and incumbent workers	Will implement the Greening of Oregon's Workforce project to develop local networks that will deliver training in green occupations. Approximately 1,039 participants will earn a degree or certificate in a targeted green industry.
28. Commonwealth of Pennsylvania, Department of Labor & Industry	Pennsyl-vania	Allegheny and Tioga	$6,000,000	Dislocated workers, youth and high school dropouts, individuals with disabilities, veterans, and individuals with limited English proficiency	Will form the Pennsylvania Center for Green Careers and will address the skills and competencies needed to fill the growing demand for 81 targeted occupations. Will use six regional projects teams to coordinate efforts and will serve 1,379 participants.
29. South Dakota Department of Labor	South Dakota	None	$2,500,000	Veterans, dislocated workers, unemployed individuals, and individuals with criminal records	With a focus on energy building, energy efficiency, energy maintenance, and biofuels industries, expecting 300 individuals to be trained and 285 to complete training.
30. Utah Department of Workforce Services	Utah	Box Elder	$4,600,000	Dislocated workers, disadvantaged youth, and veterans	Will employ an enhanced Energy Academy One-Stop model, which includes an energy core curriculum that integrates basic energy technician-level training and specific occupational training in order to prepare participants for careers in targeted green industries. Will place 1,206 participants in jobs, provide 700 participants with certificates in WorkKeys, yield 456 nationally recognized degrees or certificates, and place 225 participants in apprenticeship programs.
31. Washington State Workforce Training and Education Coordinating Board	Washing-ton	None	$5,973,635	Dislocated workers, incumbent workers, at-risk youth, low-income adults, individuals with disabilities, and veterans	Approximately 4,731 participants will be trained and placed in energy efficiency occupations, such as energy efficient construction for commercial and public buildings.
32. WorkForce West Virginia	West Virginia	Putnam	$6,000,000	Veterans, unemployed workers, and individuals with criminal records	Will use grant funds to enhance education and training providers' knowledge of targeted green industries; train current and future workers in building construction, retrofitting, and installation occupations; provide green basic skills and entrepreneurship training; and support the start up of two new community college programs related to wind energy technology and water/wastewater treatment. Will serve 2,186 participants.

Table A.3 (continued)

Grant Recipient	State	Auto-Affected Counties Served	Award Amount	Participants Served	Primary Focus and Outcome
33. Wisconsin Department of Workforce Development	Wisconsin	Kenosha and Rock	$6,000,000	Veterans, dislocated workers affected by auto industry restructuring, individuals who are currently employed in the targeted trades and in need of updated green building or green manufacturing skills training, recipients of public assistance, and other low-income individuals	Will implement the Wisconsin Sector Alliance for the Green Economics (SAGE) project to provide green skills training in construction, manufacturing, and smart grid utility occupations within the energy sector. The grantee will create new apprenticeship programs, such as weatherization technician, energy auditor, and wastewater treatment plant operator. The grantee will also enhance several existing apprenticeship programs. Will serve about 2,944 participants.
34. Wyoming Workforce Development Council	Wyoming	None	$4,495,704	Unemployed individuals, tribal members, individuals with criminal records, veterans, at-risk youth, and dislocated workers	Will use eight local training projects that will train workers in energy efficient occupations, including the use of mobile labs to provide training to individuals in remote or rural areas. Will serve about 1,041 participants.

Source: DOL's announcement of grant winners found at http://www.doleta.gov/ETA_News_Releases/20100078.cfm and http://www.doleta.gov/pdf/SESP_Summaries.pdf and accessed on January 27, 2010.

[a] The counties are listed at http://www.doleta.gov/grants/pdf/Amend-01-SGA-DFA-PY-08-20.pdf, available as of January 27, 2010.

DOL = U.S. Department of Labor; HERS = Home Energy Rating System; LEED = Leadership in Energy and Environmental Design; SAGE = Sector Alliance for the Green Economics.

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