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Impacts of Regional Approaches to Rural Development

Initial Evidence on the Delta Regional Authority

John Pender, jpender@ers.usda.gov
Richard Reeder, rreeder@ers.usda.gov

Abstract

The Delta Regional Authority (DRA) began funding rural development projects in economically distressed counties in the Mississippi River Delta region in 2002. To assess the initial economic outcomes of DRA funding, we compared nonmetropolitan DRA counties with similar counties elsewhere in the same region as well as in the Southeast. Per capita income, net earnings, and transfer payments grew more rapidly in DRA counties than in similar non-DRA counties, and those impacts were stronger in counties in which DRA spending was higher. Each additional dollar of DRA spending was associated with an increase of $15 in the growth of annual personal income from 2002 to 2007, including an increase of $8 in annual earnings (primarily in the health care and social services sector) and an additional $5 in annual transfer (Government) payments (mainly due to increased medical transfer payments such as Medicare and Medicaid). Our findings suggest that investments supported by the DRA in improved medical facilities and DRA efforts to increase the supply of health professionals may be promoting additional health sector earnings and medical transfer payments.

Keywords: Regional development programs, Delta Regional Authority, economic impacts, quasi-experimental methods

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Contents

Summary

What Is the Issue?

Regional development approaches are attracting increasing attention, particularly as vehicles for encouraging rural economic development. At the Federal level, the Denali Commission was authorized in 1998 to promote regional development in Alaska and the Delta Regional Authority (DRA) was authorized in 2000 to do the same in the Mississippi Delta region. Since then, four additional regional development commissions have been authorized, and startup funds have been appropriated for two of these. Despite increased emphasis on such regional approaches, evidence of their economic impacts is limited, especially for newer programs such as the DRA. In this study, we demonstrate an approach to investigating the initial economic outcomes of such programs, using publicly available data and the best available methods to examine the DRA as a test case. We focus on changes in personal income per capita (and its components), employment per capita, and population from 2002 to 2007.

What Were the Study Findings?

The DRA began funding projects in 252 economically distressed counties in the Mississippi Delta region in 2002. In its first 7 years, the DRA invested $75 million in basic public and transportation infrastructure, business development, job training, and employment-related education. Growth in annual personal income per capita averaged about $600 higher in DRA-recipient counties from 2002 to 2007 than in economically and demographically similar non-DRA counties in the Delta Region and in the Southeast. This greater growth represented an additional increase in per capita income in DRA-recipient counties (relative to similar non-DRA counties) of about 3 percentage points over this 5-year period. Comparison of trends in per capita income in the matched groups of counties showed that these trends were very similar from 1990 to 2002, but began to diverge after the DRA began operating in 2002.

The major sources of greater growth in personal income were greater growth in per capita net earnings and personal transfer payments, both of which were statistically significantly greater in DRA counties. (Transfer payments are payments from a Government to an individual, e.g., Medicare, Social Security, etc.) These impacts were greater in counties where DRA spending per capita was larger, with each $1 of additional DRA spending per capita associated with an additional $15 in growth of personal income per capita, including $8 in additional net earnings (primarily in the health care and social service sector) and $5 in additional transfer payments (mainly medical transfer payments).

The incremental impacts of DRA spending on personal income, earnings, and transfer payments suggest that the DRA may be leveraging additional public or private sources of funds. In particular, the DRA's health programs, including funding of medical facilities and its J-1 visa waiver program to attract foreign doctors, appear to be contributing to increased health sector earnings and medical transfer payments by increasing the availability of health care services. The DRA's health awareness campaigns, such as those focusing on diabetes prevention and treatment, may also be increasing the

demand for health services. Since other public funds, such as medical transfer payments, are apparently being leveraged by the DRA, the income increments associated with DRA spending cannot be seen solely as a return to DRA investments.

We did not find statistically significant differences in growth of employment per capita between DRA and similar non-DRA counties; this result may be due to the difficulty of measuring those impacts given the small size of the program and the relatively short timeframe considered. We found some evidence of slower population growth in the DRA counties, but this difference was found to be a continuation of differences in trends in population growth prior to initiation of the DRA.

(For a list of DRA counties, see: http://www.dra.gov/about/maps.aspx/.)

How Was the Study Conducted?

We used a quasi-experimental matching approach to select non-DRA nonmetropolitan counties in the Delta region and elsewhere in the Southeastern United States that had similar economic and demographic characteristics to DRA-recipient nonmetropolitan counties prior to implementation of the DRA, and compared mean changes in outcomes between these groups of counties. We also used multivariate regression analysis on the matched groups of counties to identify the effects of the level of DRA program spending per capita on the outcomes.

The findings are robust to alternative methods of selecting the comparison groups of counties; use of alternative sets of variables for matching; including or dropping groups of counties for which confounding factors were present (such as counties heavily affected by Hurricane Katrina or the presence of other development or health programs); and use of alternative starting and ending years in the comparisons.

Introduction

Regional economic development approaches are again becoming popular. More than 60 years after the Tennessee Valley Authority (TVA) was initiated as part of the New Deal and more than 30 years after the Appalachian Regional Commission (ARC) and Economic Development Administration (EDA) were established as part of President Johnson's War on Poverty, Congress authorized the Denali Commission in 1998 and the Delta Regional Authority (DRA) in 2000 to promote rural development in Alaska and the Mississippi Delta region, respectively. Since 2000, four additional regional rural development commissions have been authorized--the Northern Great Plains Regional Authority (NGPRA), Northern Border, Southeast Crescent and Southwest Border Commissions—and limited startup funds have been appropriated for two of these.[1]

Other regional or place-based development policies are also being pursued through enterprise zone programs enacted in numerous States since the early 1980s, the Federal Empowerment Zone and Enterprise Community program initiated in 1993, and other Federal and State policies and programs. In addition, there are several recent initiatives among Federal agencies to promote regional development approaches, following guidance from the Office of Management and Budget promoting "place-based policies" (Orszag et al., 2009). [2] The 2009 Office of Management and Budget guidance defines "place-based" policies as those that "leverage investments by focusing resources in targeted places and drawing on the compounding effect of well-coordinated action."

The growth of regional development approaches is due in part to positive impacts found in evaluations of earlier programs such as the ARC (Isserman and Rephann, 1995; Brandow et al., 2000; BizMiner/Brandow Co. Inc. and Economic Development Research Group, 2007) and the EDA (Barrows and Bromley, 1975; Martin and Graham, 1980; Burchell et al., 1997; Burchell et al., 1998; Haughwout, 1999; Arena et al., 2008). There is also substantial and rapidly growing literature on the impacts of State and local enterprise zones (e.g., Papke, 1994; Boarnet and Bogart, 1996; Dowall, 1996; Bondonio and Engberg, 2000; Lambert and Coomes, 2001; O'Keefe, 2004; Bondonio and Greenbaum, 2007; Elvery, 2009; Neumark and Kolko, 2010), and several recent studies of the Federal Empowerment Zone program (Oakley and Tsao, 2006, 2007; Busso and Kline, 2008; Hanson, 2009; Krupka and Noonan, 2009; Busso, Gregory, and Kline, 2010; Hanson and Rohlin, 2010). Small or no effects of State and local enterprise zones on employment are found in most of these studies, while some recent studies find robust impacts on local housing values.[3] Some studies of the Federal Empowerment Zones program find significant positive impacts on several indicators (e.g., Busso and Kline, 2008; Krupka and Noonan, 2009; Busso, Gregory, and Kline, 2010; Hanson and Rohlin, 2010), although the results vary across different studies and methods. To date, however, there have been no published studies assessing the outcomes of the DC or the DRA. This study addresses this information need, investigating initial outcomes of the DRA on rural development outcomes in the Mississippi Delta region.

[1]The two that have received some startup funds include the Northern Border Commission and the Southeast Crescent Commission. These newly authorized commissions are discussed further in a later section of the paper.

[2]Some have argued instead for "people-based" policies, which give money to individuals who decide for themselves how and where they wish to invest it (for example, see Glaeser, 2005). However, many economists maintain that some sort of "place-based" policies are required to overcome local market imperfections such as an inadequate supply of public goods in poor places (Crane and Manville, 2008) and imperfect mobility of labor and capital in remote places (Hite, 1997; Partridge and Rickman, 2008).

[3]Several good reviews of earlier literature on these programs are available (e.g., Peters and Fisher, 2002, 2004; Fisher and Peters, 1997; Wilder and Rubin, 1996; Bartik, 1991; Eisinger, 1988).

The DRA is a partnership among the Federal Government and 8 Delta States, targeting 252 economically distressed counties (for a full list of the counties, see: http://www.dra.gov/about/maps.aspx/). It initiated operations in 2001 and began funding projects in 2002. Between 2002 and 2009 the DRA invested $75 million in projects related to basic public infrastructure, business development, transportation infrastructure, job training, and employment-related education. The program reports that it leveraged an additional $354 million in other public investment and $1.5 billion in private investment during this period.

In this study we investigate the initial outcomes (during 2002 to 2007) of the DRA's investments in nonmetropolitan DRA counties. The outcome variables used in the assessment include county-level changes in personal income per capita and its components, employment per capita, and population. This study is of an exploratory nature, seeking to identify whether significant differences in outcomes can be measured for an economic development program as small as the DRA after only 6 years of implementation. It is not a formal impact evaluation, but rather a test of whether available data and econometric methods can discern potential impacts and help to illuminate the possible mechanisms of impact. If some initial impacts are evident, this may point to useful avenues of further research to better understand these impacts, and to rule out alternative explanations. If no significant impacts are evident, it does not mean that the program had no impact; rather it may simply mean that the impacts that have occurred are not measurable with the data and methods available, given the relatively small size of the initial DRA funding levels, or that a longer time must elapse for measurable impacts to occur.

The next section provides background on regional approaches to rural economic development in general, and the third section discusses the DRA in particular. The fourth section briefly discusses the methods and data used in the analysis and presents the main results (more detailed discussion of the methods, data, and results are provided in appendices A, B, C and D).

Regional Approaches to Rural Development

Rationale for Regional Approaches

Rural development researchers and practitioners have long recognized that local communities cannot fully control development on their own and that collaboration with neighboring communities at a regional level can be invaluable. In recent years, regional approaches to development have grown in importance because of the increasingly competitive global economy. The ability of rural communities to compete in the global marketplace depends on important regional characteristics such as workforce capacity, natural and social amenities, and infrastructure (Weiler, 2004). Shaffer, Deller, and Marcouiller (2006) maintain that to achieve long-term community development, the rural community in today's global economy must make use of "both internal and external resources to achieve change, drawing on its own strengths and capabilities and looking beyond its boundaries for supplemental resources." This requires "collaboration and partnership building within and across communities."

One of the main reasons why regional approaches may be desirable involves spillovers (externalities)—where activities in one place affect people in neighboring places. Much of the recent research on spillover impacts concerns local infrastructure and facilities, such as roads (Pfaff et al, 2007; Voss and Chi, 2006), railroads (Clark, 2006), airports (Espey and Lopez, 2000), telecommunications (Yilmaz, Haynes, and Dinc, 2002), nuclear power plants (Folland and Hough, 2000), and universities (Anselin, Varga, and Acs, 2000). However, research has also examined some of the spillovers associated with development processes, including the rural impacts of urban sprawl (Thomas and Howell, 2003; Heimlich and Anderson, 2001; Carruthers and Vias, 2005; Byun, Waldorf, and Esparza, 2005). Spillovers can have either positive (e.g., spillovers of knowledge from one locality to another) or negative impacts (e.g., pollution and congestion). In either case, regional approaches can increase the ability of communities to take such spillovers into account in their development programs, taking advantage of positive spillovers and minimizing or compensating communities for negative ones.

Rural areas have been found to be affected differently by urban growth-related spillovers, depending on distance to an urban area, extent of commuting, and other factors. This has led to the conclusion that policies designed to stimulate the development of urban and rural areas (policy shocks) are likely to have different impacts in different places, or regions (i.e., a one-size-fits-all approach is not optimal) (Henry and Barkley, 1997; Renkow, 2003, Partridge et al., 2007; Partridge et al., 2008). Hence, regional approaches can improve on uniform nationwide approaches to development policies.

Regional approaches are viewed as particularly important in helping poor places compete in the global economy (Scott and Storper, 2003). In addition, the increasingly popular "sector strategies" for economic development generally operate at a regional level, requiring collaboration across the jurisdictional boundaries of workforce investment areas, community college districts, economic development regions, municipal jurisdictions, and county jurisdictions, among others (Ligot-Gordon et al., 2008). Regional approaches can also help to address noneconomic issues, such as environmental protection, transportation access, and the availability of health services (Richgels and Sande, 2009).

Regional collaboration has several additional potential benefits, including that it:

- allows for a larger scale of operation of infrastructure and public services, which can take advantage of economies of scale, thereby lowering costs to residents and allowing more services to be provided
- allows for a more optimal location of facilities and services–in the places where they cost the least and can have the most benefit
- reduces excessive competition among the region's communities when trying to attract businesses
- allows for the formulation of regional development strategies aimed at developing one or more economic specializations that could help the region compete in the global economy
- relies on concentrated, regionwide local planning and grant-writing resources, making it easier to obtain Federal and State economic development assistance

Despite these advantages, regional efforts do not often come easily in a country with a strong tradition of local independence. Local political leaders are accountable only to the voters in the locality that elects them, which can lead to difficulties in getting local leaders to support regional policies unless they clearly benefit their jurisdictions. This can be especially problematic when neighboring communities have a long history of competition and conflict, resulting in a lack of trust and working relationships needed for collaboration (Lackey et al., 2002). In addition, since much regional policy is designed and initiated by organizations that lack traditional local sources of funding (such as local property and sales taxes), providing local funding both for the planning and implementation of regional projects can be challenging.[4]

[4]Local matching funding is generally required, even with Federal regional development programs.

Another potential drawback of regional approaches concerns the difficulty of getting support for policies that address the diverse needs of small towns or different segments of the population. To get sufficient support for regional policies, those supporting such activity must appeal to the majority of the region's voters and to the local power structure, including important local leaders of business and government. However, some towns and some segments of the population may not approve of these majority-driven policies, especially if their needs and interests differ substantially from that of the region as a whole. For example, the interests of the poor and other minorities may not always receive significant attention from regional policies, even though such policies may have more potential to address such problems than do single-jurisdiction policies. Thus, even where regional policies are in place, they may fall short of achieving economically and socially optimal outcomes.

Even with these problems, the number of regional partnerships for economic development has increased in recent years (Olberding, 2002). However, researchers generally view the current structure of Federal, State, and local governments and their development programs as having weak capacity to address regional challenges (John, Brooks, and McDowell, 1998) and doing little to foster these kinds of partnerships in rural America (Drabenstott and Sheaff, 2002). As a consequence, Federal regional development programs have been created to encourage more regional activity, particularly in the more distressed regions.

History of Federal Regional Development Programs

New Deal and Great Society Programs

Federal regional development programs have a long history, going back to the Tennessee Valley Authority (TVA), created in 1933 (Roth et al., 2002). The TVA's nonpower programs were terminated in the late 1990s, leaving communities in this region (and others) to rely on programs that date to the 1960s. The two most notable of these programs are the Appalachian Regional Commission (ARC) and the Economic Development Administration (EDA), both created in the 1960s.[5] Both provide general administrative funding to multi-county economic development districts that plan and implement their projects, and Federal funds are used to leverage State, local, and private investments in their projects. Both programs have emphasized investments in infrastructure, but they have also financed other kinds of projects that fit into the strategies devised by the multi-county entities. In addition, both programs target their investments to the most economically distressed regions. The main differences between these two programs are that the EDA is a nationwide program, whereas the ARC is confined to a designated multi-State region, and the ARC is a Federal-State partnership, in which governors of ARC's member States have a vote in deciding which projects to support (unlike the EDA).

[5]USDA began its own regional program at roughly the same time, the Resource Conservation and Development (RC&D) program (Gadsby, 2002). RC&D is similar to EDA, being a nationwide program that funds projects for multicounty entities. However, its objectives are different and its funding levels are lower ($51 million in fiscal year 2009).

Although other Federal programs also emphasize regional approaches—most notably transportation and environmental programs—the ARC and EDA are the largest programs aimed at comprehensive economic and community development at a regional level. They are also among the Federal programs that have received the most significant program evaluations over the last 20 years (this literature is reviewed in a later subsection).

New Regional Development Commissions

Two new regional development programs (the Denali Commission and the Delta Regional Authority) were created during the latter years of the Clinton Administration. The Denali Commission Act was authored by Senator Ted Stevens of Alaska and enacted in October 1998 (PL105-277). Its overall goals are to lower the cost of living and raise the standard of living in Alaska, with a focus on remote communities. Its specific objectives include delivering cost-effective services; promoting rural development; and providing additional power generation and transmission facilities, advanced telecommunications, water and sewer systems and other infrastructure, plus job training and other economic development services, especially in distressed areas. Subsequent legislation in November 1999 expanded the mission to include health care facilities (Denali Commission, 2001).

The Delta Regional Authority (DRA) was authorized in December 2000 by Title V of the FY 2001 Omnibus Appropriations Act (PL106-554); cited in the law as the "Delta Regional Authority Act." This occurred 12 years after the seven-State Lower Mississippi Delta Development Commission (LMDDC) was established. That Commission was chaired by then-Arkansas Governor Bill Clinton and published its Delta Vision report in 1990, which advocated the creation of a permanent regional planning and development entity in the Delta region. Ten years later, President Clinton's Delta Initiative, led by Transportation Secretary Slater, published the Delta Vision, Delta Voices report, which also advocated such an entity (U.S. Department of Transportation, 2000). Later that year, with the support of President Clinton and key members of Congress, the DRA was enacted.

In 2002, the Farm Security and Rural Investment Act (PL 107-171) reauthorized the DRA. Although funding was appropriated for the DRA, the funds appropriated have been well below the level authorized ($30 million per year), declining from an initial appropriation of $20 million in 2001 to $5 million in 2004, but increasing since to $13 million in 2009 and 2010 (table 1). Outlays by the DRA have been somewhat smaller than appropriations.[6] Since its inception, DRA expenditures have been much smaller than those of the EDA, the ARC, or the Denali Commission (see table 1). We discuss the DRA in more detail in a later section. The 2002 Farm Act also authorized the creation of a new regional

[6]By the end of FY 2008, the DRA had a resource balance of about $27 million, nearly half of which was obligated and the rest unobligated (DRA, 2008a).

Table 1
Appropriations and outlays for selected regional development programs

	Appropriations[1]	Outlays[2]			
Fiscal year	DRA	DRA	EDA	ARC[3]	Denali Commission
	Million dollars				
1999	-	-	355	136	1
2000	-	-	356	125	38
2001	20.0	-	356	86	11
2002	10.0	1	355	101	-14[4]
2003	7.9	6	375	74	2
2004	5.0	12	337	68	16
2005	6.0	9	332	65	49
2006	11.9	6	284	63	42
2007	11.9	8	243	67	33
2008	11.7	8	238	69	46
2009	13.0	9	243	62	60
2010	13.0	13	422	65	79

DRA=Delta Regional Authority.
EDA=Economic Development Administration.
ARC=Appalachian Regional Commission.
2010=forecast.
[1]Source: Annual U.S. Congress appropriations bills, various years.
[2]Source: Table 12.3, Historical Tables from the President's Budget, FY 2011. Available at: http://www.whitehouse.gov/omb/budget/Historicals/.
[3]Excludes Appalachian Highway Program.
[4]Negative number due to deobligated funds.

authority, the Northern Great Plains Regional Authority (NGPRA). The NGPRA has not been able to spend Federal funds because a Federal co-chair was never appointed.

The 2008 Food, Conservation, and Energy Act (PL 110-246) continued authorization of the DRA and NGPRA through 2012 and authorized three new regional development commissions: the Northern Border, Southeast Crescent, and Southwest Border Commissions (USDA/ERS, 2008). In 2009, appropriations legislation provided funding to start the Northern Border Regional Commission (NBRC), and a Federal Co-Chair has been appointed and recently confirmed in March 2010. The NBRC is therefore set to begin operations, while the other three commissions still lack a Federal Co-Chair and funding.[7]

[7]A small amount of startup funds have been appropriated for the Southeast Crescent Commission. For more information about these other commissions that have not yet started up, see Reeder (2009).

Other Federal Initiatives Related to Regional Development

Beginning in the mid-1990s, USDA administered the Empowerment Zone/Enterprise Community program that provided assistance to selected high-poverty rural places attempting to overcome barriers to development (Reid, 1999; U.S. GAO, 2004), and the Rural Economic Area Partnership (REAP) initiative that assisted rural places experiencing other problems, such as outmigration (USDA, 1994; USDA/RD, 2008; USDA/ERS, 2000, pp. 11-12). Both programs encouraged strategic regional planning and collaboration. In 2005, in response to the disasters associated with Hurricanes Katrina, Rita, and Wilma, Congress enacted legislation providing targeted tax relief to promote redevelopment in affected localities, called Gulf Opportunity Zones (Richardson, 2006).

More recently, the Obama Administration began promoting regional approaches to rural development. In August 2009, the White House issued a memorandum promoting "place-based" policies, in which each Federal agency was asked to initiate three to five place-based programs (Orszag et al., 2009). This memorandum noted that regional approaches are particularly important for rural areas. Several new USDA initiatives are associated with this new emphasis on rural regional approaches (discussed in the next paragraph). Other new Federal Government regional efforts include the Sustainable Communities Initiative of the U.S. Department of Housing and Urban Development (HUD), the U.S. Department of Transportation (DOT) and the U.S. Environmental Protection Agency (EPA), which links grants and technical assistance from these agencies for regionally integrated planning; the Small Business Administration's Entrepreneurial Development Initiative to help small businesses participating in regional economic clusters; and EDA's regional innovation clusters initiative aimed at building on regions' industrial competitive strengths.

Meanwhile, USDA began to make use of strategic planning grants and efforts to coordinate its programs with other Federal programs in "an integrated effort to advance regional development" (Vilsack, 2010). These efforts were included in USDA's fiscal year 2011 budget proposals (USDA/FSA, 2010). One of these initiatives is the Regional Innovation Initiative "to aid in planning and coordination of USDA and other sources of assistance … (so that) … communities within regions … that work together can produce more prosperity for all" (USDA/FSA, 2010, p. 57). In addition, the Great Regions Initiative makes use of the existing Rural Business Opportunity Grants program to target grants to intermediaries (nonprofits, tribes, etc.) that provide assistance to development strategies employing regional approaches to rural development objectives. These new regional initiatives for rural development have also been promoted in the White House's Council of Economic Advisors (CEA) report, *Strengthening the Rural Economy* (CEA, 2010).

Although it is too early to say whether this new regional policy activity will result in a major change in Federal rural development policy, it is certainly suggestive of a change in attitude toward a topic that has been largely ignored at times in the past.

Literature on Impacts of Regional Development Programs

Several studies have assessed economic impacts of ARC and EDA programs using various methods (see box, "Summary of literature on economic impacts of Appalachian Regional Commission (ARC) and Economic Development Administration (EDA)"). Using a quasi-experimental approach comparing ARC counties to similar (matched) non-ARC counties, Isserman and Rephann (1995) estimated substantially higher growth in ARC counties from 1969 to 1991 in personal income per capita; population; total personal income; earnings in total and in several industries; dividends, interest, and rent; and in transfer payments. They found similar results when focusing on Central Appalachia, the poorest subregion of Appalachia, and estimated that the $13 billion spent cumulatively by the ARC between 1965 and 1991 "meant $8.4 billion more income for Appalachia in 1991."

Isserman and Rephann (1995, pp. 362-363) indicated that the greater growth in ARC counties compared to matched non-ARC counties could not be attributed to ARC programs with certainty, acknowledging that a "leap of faith" is required in control group research. They considered one alternative explanation (not already addressed by their analysis)—differences in racial composition—stating that when racial composition was considered in the matching, the differences in growth were even stronger. They did not explicitly consider other alternative explanations, such as effects of the 1970s coal boom (as suggested by U.S. GAO (1996)), but they did investigate factors associated with differences in growth rates between ARC counties and their matched twins using multivariate regressions. These results show that coal counties grew faster than twin noncoal counties in some respects (in total and per capita income), but also that being a coal county does not explain differences in growth of other outcome variables or explain all of the differences in income growth.

An unpublished study by Freshwater et al. (1997) examined both the ARC and the TVA, examining the 1980s experience of the counties assisted by these two regional development programs. This study used a 3-equation, simultaneous equation, model explaining change in manufacturing earnings per worker, manufacturing employment, and educational attainment in 2,053 counties in the Eastern United States, with dummy variables representing

Summary of literature on economic impacts of Appalachian Regional Commission (ARC) and Economic Development Administration (EDA)

Program	Study	Outcomes measured	Methods	Key findings
ARC	Isserman and Rephann (1995)	Total personal income (PI) and per capita personal income (PCPI); population; total earnings and earnings by sector; dividends, interest and rent (DIR) ; and transfer payments (TP)	Comparison of changes in outcomes from 1969 to 1991 for ARC counties vs. matched non-ARC counties, using Mahalanobis metric (MM) matching	• ARC counties grew faster in PI, PCPI, population, DIR, TP, and earnings, with more rapid growth especially in services; finance, insurance and real estate; manufacturing and retail trade. Similar results also were found in Central Appalachia, the poorest subregion.
ARC	Brandow (2000)	Number of jobs created or retained, other public and private funds leveraged, indirect and induced economic impacts, local tax revenues, economic diversification and quality of life indicators, impact/cost ratios (e.g., cost per job created/retained)	For 99 projects that closed from 1990 to 1997: examined self-reported direct number of jobs created or retained; used input-output model (IMPLAN) to predict indirect and induced economic impacts; fiscal impacts estimated based on economic impacts; subjective qualitative data used to measure impacts on quality of life, environment, other outcomes	• Each $1 of ARC funding associated with $2.61 of other public funding and almost $100 of private investment. •$1,222 cost to ARC per direct job created; $4,574 total public cost per job created (smaller costs per total jobs created/retained). • $20 in additional annual direct wage income per $1 of ARC funds; $5.40 per $1 of total public funds (larger total income impacts per $1 spent).
ARC	BizMiner/ Brandow Co. Inc. and EDR Group (2007)	Same as Brandow (2000)	Similar to Brandow (2000), using data on 78 projects closed between 1999 and 2005	• Each $1 of ARC funding associated with $4.87 of other public funding and $75 of private investment. • $1,274 average cost to ARC per direct job created; $8,102 total public cost new job created (smaller costs per total job created/retained). • $28 in additional annual direct wage income per $1 of ARC funds; $4.40 per $1 of total public funds (larger total additional income per $1 spent).

—*continued*

Summary of literature on economic impacts of Appalachian Regional Commission (ARC) and Economic Development Administration (EDA)—Continued

Program	Study	Outcomes measured	Methods	Key findings
ARC	Glaeser and Gottlieb (2008)	Population and income per capita	Ordinary least squares (OLS) regression models explaining growth in population and income per capita in counties in ARC states from 1970 to 1980 and 1970 to 2000, using the initial value of each dependent variable as the only control variable and a dummy variable for ARC coverage	• Population growth was significantly faster in ARC counties during 1970-80, insignificant difference in growth from 1970-2000. • Income per capita growth was significantly slower in ARC counties during 1970-2000; there was an insignificant difference from 1970-1980. • Authors claim results not sensitive to inclusion of other control variables, but results for other specifications not shown.
ARC and TVA	Freshwater et al. (1997)	Manufacturing employment and earnings and educational attainment	Used 3-equation simultaneous equation model covering 2,053 counties in Eastern U.S. to estimate changes associated with ARC and TVA during 1980s. Dummy variables used to identify ARC and TVA assisted counties and effects of assistance.	• TVA had more positive effects than ARC. Rural ARC counties had significantly slower growth in manufacturing earnings and employment. Authors speculated that this could be explained, in part, by move South of manufacturing during this time period.
EDA	Barrows and Bromley (1975)	Number of jobs directly created by the EDA project	OLS regressions explaining number of jobs created (using data from a 1972 EDA evaluation of projects completed in 1967-69) on county and project characteristics; discriminant analysis of factors distinguishing projects creating no jobs and projects creating at least one job	• Firms using more unskilled labor were associated with larger employment impacts.
EDA	Martin and Graham (1980)	Personal income	OLS regressions explaining changes in growth rate of county-level personal income (growth rate from first year of aid to last year of aid minus growth rate prior to aid, and growth rate after aid ended minus growth rate before aid) on variables reflecting the amount, nature and timing of EDA assistance, and non-EDA factors affecting income growth rates	• Greater EDA funding relative to county income, greater percentage of funding for public works projects, and earlier receipt of funding are associated with greater increase in income growth during the project period. • None of the EDA variables had a significant impact on changes in income growth rates in the post-assistance period.

—continued

Summary of literature on economic impacts of Appalachian Regional Commission (ARC) and Economic Development Administration (EDA)—Continued

Program	Study	Outcomes measured	Methods	Key findings
EDA	Burchell et al. (1997)	Number of jobs created or retained, other public and private funds leveraged, indirect job impacts, local tax revenues, economic diversification, impact/cost ratios cost per job created/retained	For 203 projects that closed in 1990: examined self-reported number of direct and indirect permanent jobs created or retained and other related public or private investments; the additionality of jobs was assessed based on whether EDA assistance identified as "critical" or "essential"; tax impacts estimated based on impacts on private investment	• $3,058 median cost to EDA per direct permanent job created; $4,857 total public cost per job created. • Each $1 of EDA funds associated with $1 of other public investment and $10 of private investment. • Each $1 million in EDA funds result in 50 non-project-related direct jobs and 64 project-related indirect jobs. • Each $1 of EDA funds adds $10 to the local tax base.
EDA	Burchell et al. (1998)	Private investment leveraged and number of total jobs created or retained (direct, indirect and induced), cost per job created or retained	Input-output model (IMPLAN) used to predict indirect and induced employment impacts of direct employment impacts estimated by Burchell et al. (1997) Multiple regression models used to estimate total employment impacts controlling for other variables, using county-level annual data from 1990 to 1994; OLS and two-stage least squares (2SLS) regressions used, with 2SLS used to account for endogenous wage levels in employment regression and employment in wage regression	• Median employment multiplier (total jobs created/direct jobs created) equals 1.50; private investment multiplier (total private investment induced/direct private investment) equals 1.44 (from input-output model). • Total employment increase of 7-10 jobs per $10,000 EDA public works investment (point estimate of 9 jobs, implying cost of $1,100 per total jobs created (regression analysis). • Statistically insignificant impact of EDA spending on earnings per employee (regression analysis).
EDA	U.S. General Accounting Office (1999)	Total county employment	GAO repeated the OLS regression specification of Burchell et al. (1998), as well as alternatives including prior county employment or population as additional control variables	• The Burchell et al. (1998) finding of significant positive impact of EDA spending on county employment was not robust to including prior employment or population in the specification.

—continued

Summary of literature on economic impacts of Appalachian Regional Commission (ARC) and Economic Development Administration (EDA)—Continued

Program	Study	Outcomes measured	Methods	Key findings
EDA	Haughwout (1999)	County private nonfarm employment and farm employment, average compensation per employee	Regression model very similar to Burchell et al. (1998), except includes prior county employment (as in GAO, 1999) but not payroll per worker (which were included in both Burchell et al. and GAO studies) as controls	• Significant positive impact of EDA spending on private nonfarm employment, robust to a few alternative specifications. • Impact in preferred specification implies EDA cost per job created of $9,953. • Insignificant impact of EDA spending on compensation per employee (as in Burchell et al.) and on farm employment.
EDA	Glasmeier (2002)	Cost per job created	Using data provided by EDA on cost per job created by projects completed in 1990 (based on the Burchell et al., 1997 study) and in 1993 (based on another study using similar methods), examined how cost per job varied by year of completion, in rural vs. urban areas, and by other county characteristics	• The estimated average EDA cost per job created was higher in rural than urban areas in 1990 ($5,938 vs. $1,988), but lower in 1993 ($6,904 vs. $7,399).
EDA	Arena et al. (2008)	County employment	OLS and 2SLS regressions explaining annual county employment in 1990-2005, separately for urban (metro) vs. rural (nonmetro) counties, using a similar specification to Burchell et al. (1998), but investigating impacts of different types of EDA projects, impacts by number of years since project completion, and robustness to some alternative specifications of control variables; sample selected to avoid confounding of EDA funding in different years (included only counties with either no EDA activity or where one or more EDA projects were completed by the fourth year of a 9-year period, and with no EDA funding in remaining 5 years)	• EDA funding has a significant positive impact on employment in rural areas across OLS regression specifications; similar magnitude with 2SLS but statistically insignificant. • Impact of EDA funding in rural areas seen within one year of project completion and remains at a similar level for 5 years after project completion. • Incremental mean EDA cost per job created: $2,001-$4,611 across specifications. • EDA cost per job was smallest for business incubator projects ($144-$216 and largest for community infrastructure projects ($2,920-$6,872). • Statistically insignificant impacts of EDA funding found in urban areas.

Acronyms (in order of appearance by rows): ARC = Appalachian Regional Commission EDA = Economic Development Administration PI = Total personal income PCPI = Per capita personal income DIR = dividends, interest and rent TP = transfer payments MM = Mahalanobis metric matching estimator OLS = ordinary least squares regression TVA = Tennessee Valley Authority IMPLAN = IMpact analysis for PLANning, a commercially available input-output modeling software package GAO = U.S. General Accounting Office (renamed the U.S. Government Accountability Office in 2004) 2SLS = Two-stage least squares regression.

ARC- and TVA-assisted counties. The results were mixed but tended to show more positive impacts of the TVA than the ARC. In rural counties, TVA was associated with significantly faster rates of growth of manufacturing earnings and employment than other eastern counties, while rural counties in ARC had significantly slower growth in these two measures of manufacturing economic performance. The authors provided some possible explanations for the negative effects of the ARC on manufacturing earnings and employment, including the observation that manufacturing activity was moving southward during the 1980s, away from many ARC counties and toward TVA counties, for reasons unrelated to the two programs and unaccounted for by their impact estimation model. Hence their impact estimates may have been biased by this.

Studies by Brandow et al. (2000) and BizMiner/Brandow, Inc., and EDR Group (2007), using the reported number of jobs directly created by ARC projects combined with estimated multiplier impacts using an input-output model (IMPLAN), estimated large impacts of the ARC on employment and public revenues. For example, Brandow et al. (2000) estimated that each $1 of ARC funding leveraged $2.61 of other public funds and almost $100 of private investment, increased employment at an average cost to ARC of $1,222 per direct job created and a cost in total public funds of $4,574 per direct job created (the costs per total number of jobs created, considering multiplier impacts, were less), and generated $5.40 of additional wage income per $1 of total public funds invested. The impacts estimated by BizMiner/Brandow, Inc. and EDR Group (2007) were of similar magnitude.

Similar positive impacts of EDA's programs have been found by several evaluations. For example, Burchell et al. (1997), using self-reported impacts from project officials, estimated for all projects completed in 1990 that each $1 of EDA funds leveraged $1 of other public funds and $10 of private investment, that each $1 million of EDA funds produced 327 direct permanent jobs plus 50 nonproject-related direct jobs and 64 project-related indirect jobs, produced direct permanent jobs at a median cost to EDA of $3,058 per job and $4,857 in total public cost per job, and increased local public revenues at a rate of $10 per $1 of EDA funds. Using the IMPLAN input-output model, Burchell et al. (1998) extended these results to estimate that for each direct permanent job created by EDA projects, an additional 0.5 jobs were created through indirect and induced labor market multiplier effects[8]; and that an additional $0.44 in private investment was stimulated through multiplier effects for each $1.00 in private investments directly resulting from the projects. These estimates imply that the cost per total number of jobs created would be less than those cited above, given such multiplier effects.

Impact estimates based on self-reported figures from grant recipients and multipliers estimated by input-output models may be unreliable for several reasons. Grant recipients have incentives to overstate the positive impacts of their programs so as to help ensure continued funding, and may be unaware of (or unlikely to report) negative impacts, such as jobs that were displaced by project investments (for example, construction of a new hospital in a community may displace jobs in existing health care facilities). Multiplier estimates from input-output models account for indirect and induced impacts, but these are subject to several criticisms. They are based on static linear fixed coefficient models of production, and typically assume that local factors of

[8]Indirect multiplier effects refer to impacts resulting from interindustry linkages (e.g., employment in local industries that supply inputs to the firms directly affected by the project, employment in other local firms that provide supplies to those supplier firms, and so on). Induced multiplier effects result from the increase in demand due to increased income of residents of the region, which leads to its own chain of direct and indirect demand effects.

production respond perfectly elastically (at fixed prices) to increased demand; hence they ignore increases in costs of labor, land, or other inputs that may result from new investments (Kilkenny and Partridge, 2009). They ignore external positive and negative impacts of new developments, such as economies of agglomeration resulting from spillovers of knowledge and technology among firms, increased pollution, or congestion in use of infrastructure and public services (Edmiston, 2004). And they are often based on data estimated from national accounts rather than detailed survey data for the local economy (Rickman and Schwer, 1995). Evidence from several econometric impact studies suggests that input-output models often overestimate impacts of new industrial development on local economies (Edmiston, 2004; Fox and Murray, 2004; Kilkenny and Partridge, 2009).

To address concerns about the limitations of self-reported direct impacts and multipliers from input-output models, several studies have used econometric analysis of county-level data on employment. Burchell et al. (1998) estimated impacts of EDA spending on total employment (including direct as well as multiplier effects) using econometric regression models with county-level data for 1990 to 1994, estimating that county employment increased by nine jobs per $10,000 of EDA spending, implying a mean cost per job created of $1,100. Haughwout (1999) (one of the co-authors of the Burchell et al. (1998) study), estimated a similar econometric model to Burchell et al. (1998) but considered several alternative specifications. He found that EDA spending had a robust positive impact across the specifications considered, although the magnitude of impacts was substantially smaller than in the Burchell et al. (1998) study, implying an EDA cost of $9,953 per job created in the preferred specification. More recently, Arena et al. (2008) estimated impacts of EDA funding using county-level data for 1990 to 2005, considering several different regression specifications. They found robust positive impacts of EDA funding on employment in rural areas across specifications but insignificant impacts in urban areas; their estimates of employment impacts in rural areas imply a mean EDA cost per job created ranging from $2,001 to $4,611. They also estimated impacts by type of EDA project and found the smallest cost per job created for business incubator projects and the largest cost for community infrastructure projects. The findings from these county-level regression analyses were broadly consistent with the findings from the input-output model studies in terms of the estimated cost per job created.

Econometric impact studies of regional development programs have also not been without critics. For example, the U.S. General Accounting Office (GAO) argued that the Isserman and Rephann (1995) study did not establish a causal connection between the ARC's specific programs and specific outcomes, such as a highway construction program leading to growth in manufacturing (U.S. GAO, 1996, p. 4). GAO also argued that none of the studies of ARC and EDA available in 1996, including the Isserman and Rephann study, attempted to rule out alternative causal explanations for observed growth differences, such as the rise in coal prices that occurred in the 1970s (but see the discussion on page 8 on this point).

Glaeser and Gottlieb (2008) also disputed the conclusions of Isserman and Rephann. Their own analysis used ordinary least squares (OLS) regressions, taking growth in county population or per capita income from 1970 to 1980 and 1970 to 2000 as dependent variables, using as the only explanatory variables a dummy variable for whether the county was an ARC county and the initial level of either population or income per capita, and focusing on counties in the ARC States (excluding counties close to the coast). They found that ARC counties had more rapid population growth than non-ARC counties from 1970 to 1980, but no significant difference in population growth from 1970 to 2000, and that ARC counties had slower growth in per capita income from 1970 to 2000. Based on this analysis, Glaeser and Gottlieb conclude that "it is unlikely that the effects of a $13-billion program spread over a giant swath of America over three decades can be accurately evaluated" (p. 200). Although their research points out how such studies are potentially vulnerable to problems related to omitted variables, Glaeser and Gottlieb's analysis and conclusions are subject to the same criticism, since they did not attempt to control for the many factors that could have affected differential growth rates between ARC and non-ARC counties, as did Isserman and Rephann.[9]

[9]Glaeser and Gottlieb (2008, p. 199) state that their results are not sensitive to including other controls, but provide no indication of what other controls they considered or evidence of the results.

In a review of the Burchell et al. (1998) study of EDA's impacts, the GAO (1999) used the same data as Burchell et al. to replicate their results and then showed how inclusion of an additional variable (either the employment level or the population level in the county in the 1980s) caused the estimated impact of EDA spending to be small and statistically insignificant. Haughwout (1999) addressed this criticism to some extent by including the 1988 county employment level as an explanatory variable in his specifications, which may explain why he found a smaller impact of EDA spending than Burchell et al. However, there were other differences in the specifications used by Haughwout (1999) compared to those of Burchell, et al. (1998) and U.S. GAO (1999). Hence, it is not possible to determine exactly why the estimates of Haughwout differed from prior studies.

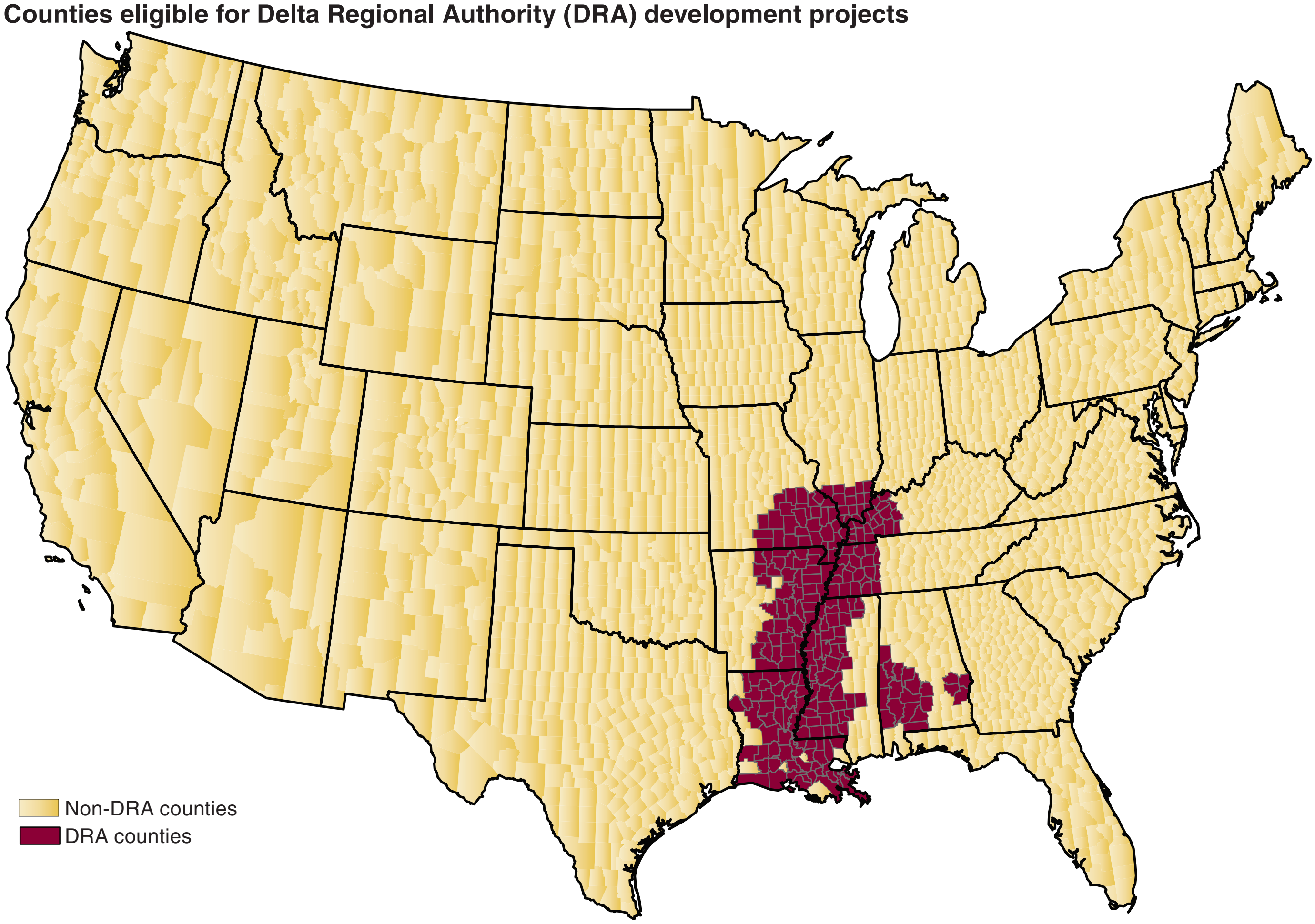
The Delta Regional Authority

The Delta Regional Authority, patterned after the ARC, is a Federal-State partnership involving eight States (Illinois, Missouri, Kentucky, Tennessee, Arkansas, Mississippi, Louisiana, and Alabama). It is led by a Federal Co-Chair and the Governors of the participating States, one of whom serves as the State Co-Chair (on a rotating basis). Within these States, 252 counties and parishes are currently eligible for the program (fig. 1). Originally 240 counties were eligible, including the 219 counties in the 7-State LMDDC region, plus 1 additional parish in Louisiana and 20 counties in Alabama. Four of the Alabama counties were added by the 2002 Farm Security and Rural Investment Act. The 2008 Food, Conservation, and Energy Act added 10 additional parishes in Louisiana and 2 counties in Mississippi.

The population of the DRA region was 9.5 million in 2000. The region is the most economically distressed large region of the country, with 250 of the DRA counties and parishes having per capita incomes below the national average, a poverty rate for the region 55 percent higher than the national average, a high school dropout rate almost 20 percent above the national

Figure 1

Counties eligible for Delta Regional Authority (DRA) development projects



Notes: The DRA operates in Alabama, Mississippi, Louisiana, Arkansas, Tennessee, Missouri, Kentucky, and Illinois. For a list of the 252 DRA-eligible counties, see http://www.dra.gov/about/maps.aspx/.

Source: Delta Regional Authority, 2010.

average, and infant mortality rates nearly 30 percent above the national average (http://www.dra.gov/delta-facts/). An Economic Research Service (ERS) analysis of the 219 counties within the original Lower Mississippi Delta region found that most were persistent poverty counties (Reeder and Calhoun, 2002). Thus this region is classified as distressed and poor, regardless of which measure is used.

The DRA Act requires that at least 75 percent of the DRA funds be used to serve the needs of distressed counties. DRA project funding has far exceeded this threshold, with 94 percent of project funding approved during 2002 to 2008 invested in distressed counties (DRA, 2009), because the vast majority of eligible DRA counties and parishes are distressed. In 2010, 223 of the 252 DRA counties and parishes were classified as distressed according to the criteria established by the EDA (DRA, 2010).[10]

[10]The DRA and EDA consider a county or parish distressed if it has (1) an unemployment rate at least 1 percentage point greater than the national average for the most recent 24-month period; or (2) per capita income that is 80 percent or less of the national average for the most recent period for which data are available; or (3) a special need arising from actual or threatened severe unemployment or economic adjustment problems due to various causes.

The DRA is authorized to provide grants to States and public and nonprofit entities for development projects, with the following order of priority: (1) basic public infrastructure in distressed or isolated areas of distress; (2) transportation infrastructure facilitating regional economic development; (3) business development, with emphasis on entrepreneurship; and (4) job training or employment-related education. At least 50 percent of project grant funds are required to be for transportation and basic public infrastructure projects. "Basic public infrastructure" is defined by the DRA as including water and wastewater facilities, electric and gas utilities, broadband delivery, and solid waste landfills (DRA, 2010). Development of Geographic Information Systems (GIS) to support such basic infrastructure is also classified as a subpart of basic public infrastructure (Ibid.). "Transportation infrastructure" means basic physical structures such as roads, bridges, rail, port facilities and airports, but local, State and Federal highway and bridge maintenance projects are not eligible for DRA funding. Often transportation projects involve an extension of a road or provision of another type of transportation structure to an industrial park or within an industrial park or port facility. From 2002 to 2008, 76 percent of DRA project funds were invested in these priority projects (DRA, 2009).

The multicounty Local Development Districts (LDDs) operating as lead organizations for this program are those already established by EDA, or if no such district exists, some other entity meeting statutory requirements concerning representation on the board. The DRA was also given authority to cover up to 80 percent of the LDD's administrative expenses.

The DRA held its first meeting in late 2001 and completed its first 5-year plan in 2002. That plan articulated four long-term goals of the DRA, including increasing income levels, reducing unemployment and underemployment, reducing dependency on Federal support and transfer payments, and providing the infrastructure necessary to support economic and domestic development (DRA, 2008b). The plan established 5-year targets for several outcomes, including income, unemployment, poverty, transfer payments, public assistance, education, single parent households, and labor participation.

In its second 5-year plan, published in 2008, the DRA built upon this framework, highlighting five key categories of investment to be funded by its competitive grants:

- Health as an economic engine
- Information technology
- Transportation
- Workforce education and leadership
- Traditional localized projects supporting basic infrastructure, transportation, workforce training, and business development

The DRA recognized from the outset that its ability to achieve improvements in outcomes would be limited by its modest budget and staff resources. The model pursued was to concentrate on developing the assets needed to sustain long-term growth in selected critical mass communities[11] by coordinating the efforts of multiple organizations and leveraging additional public and private investments. One fact that contributes to the potential leverage of DRA funds is that the DRA Act allows DRA funds to be used to supplement other Federal program funds above the maximum amounts of Federal support authorized by other applicable laws, up to 90 percent Federal support of project costs in general and up to 100 percent support of projects providing transportation or basic public services in distressed counties or isolated areas of distress (PL 106-554, Sections 382D(b) and 382F(b)).[12] In poor counties where the ability to provide local matching funds can pose a major constraint, the ability to use DRA funds beyond normal Federal statutory limits may be especially important in enabling projects to be implemented.

The allocation of DRA funds to the member States is determined by a formula that considers equity among the States (accounting for 50 percent of the score used for the allocation), the total population of the DRA counties/parishes of each State (10 percent), the distressed population of the DRA counties/parishes of each State (20 percent), and the distressed DRA county area of each State (20 percent). During the first 8 years of the program (2002-09), Louisiana received the largest share of grant funds (20.4 percent), followed by Arkansas (15.2 percent), Mississippi (14.2 percent), Missouri (11.6 percent), Tennessee (11.1 percent), Alabama (10.4 percent), Kentucky (8.6 percent), and Illinois (8.5 percent).

A joint Federal-State annual process is used by the DRA and its member States to select projects for funding (DRA, 2010). Early in the calendar year, a call for preapplications is issued and publicized by the LDDs, which also provide education and assistance to applicants.[13] By March, these preapplications are received by the LDDs and forwarded to the DRA and each State's Governor. The Federal Co-Chairman and his or her staff review the preapplications to determine whether they meet eligibility requirements established by the DRA Act and DRA's policies and clarifications. For example, projects are classified as eligible depending on whether they demonstrate that they will be sustainable, provide funding only to eligible entities (excluding private/for-profit entities and entities deemed ineligible due to poor prior grant history), and provide funds for eligible purposes. The DRA Act specifically prohibits use of DRA funds to assist businesses to relocate from one area of the Delta region to another, or to supplant existing funding streams. By April, the Federal Co-Chairman provides the list of eligible projects to the State Governors, who then select projects by June for precertification within the

[11]The DRA defines critical mass communities as "those communities in which the necessary elements exist in sufficient quantity to create and sustain a vital economy," including healthy people, an expanding population, a skilled workforce, multiple cultures, new companies, an entrepreneurial culture, and a communitywide culture of learning (DRA, 2008b).

[12]A similar but more restrictive provision applies to the ARC, which under the ARC Act can provide Federal funds to distressed counties in excess of the maximum portions authorized by other laws, but restricts the maximum Federal contribution to be not more than 80 percent (PL 89-4, Section 214(b)).

[13]The timeline described is that used in 2010. Timelines in earlier years may have been somewhat different.

context of their own DRA-approved State plans and priorities. Pre-certified entities are then invited to prepare and submit full applications, with the DRA staff playing a collaborative and collegial role in helping "these 'pre-grantees' to best insure their applications are sound and all contingencies are satisfied" (DRA, 2010, p. 84). The DRA staff review and certify the applications, which are then forwarded to the State Governors for their certification. The process is completed and applicants are notified by early in the fall.

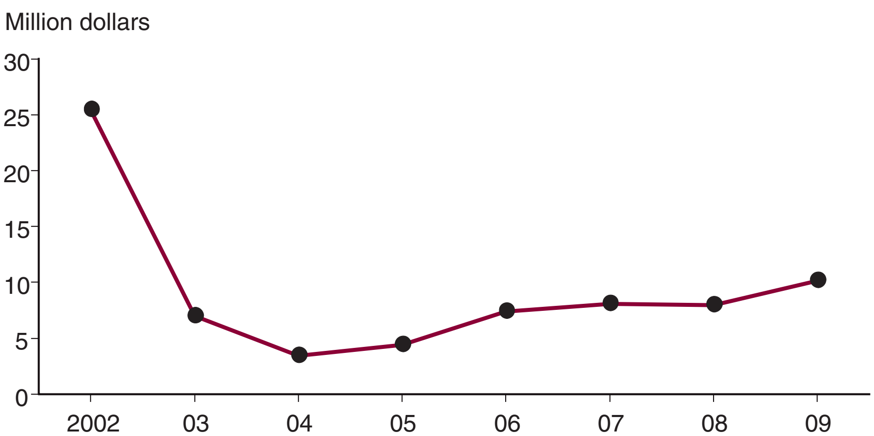
This process is fairly selective. Of the 240 counties and parishes that were eligible for DRA projects during 2002 to 2008, only 166 had projects approved (DRA, 2009). In 2009, of 261 preapplications submitted, only 88 were deemed eligible by the Federal Co-Chairman (DRA, 2010). Forty-two of the ineligible applicants appealed the decision and 12 of these were reversed, resulting in 100 eligible applicants. Of these, 69 projects were ultimately selected by the State Governors and approved.

The DRA began funding projects through its grant program in 2002. During its first 8 years of operation (2002-09), the DRA invested $74.6 million in 510 projects (DRA, 2010). Due to a relatively large initial appropriation, the number and value of projects approved was greater in 2002 than in subsequent years, although the size of the program has increased since the low point in 2004 (fig. 2). The DRA reports that this investment helped to attract $353.8 million in other public funds ($4.75 in additional public funds per $1 invested by DRA) and $1,544.4 million in private investment ($20.71 in private investment per $1 invested by DRA) (DRA, 2010).[14] The most common type of investment supported by DRA funds was investment in water and/or sewer systems; these accounted for 29 percent of DRA project funds invested during 2002 to 2008.[15] Following this were investments in roads (12 percent), industrial parks (9 percent), education and training (8 percent), port facilities (8 percent), medical facilities (7 percent), and business development (5 percent).

The amount of DRA funding per capita was small; averaging $16.75 per capita in the counties/parishes receiving DRA grant funds between 2002 and 2007 (table 2). The amounts varied significantly across and within States, with

Figure 2

Value of DRA grants approved, 2002-09



Notes: DRA = Delta Regional Authority.

Source: DRA, 2010.

[14]We are unable to determine how these estimates were collected or verify their validity. Regardless of how accurately such numbers are collected and reported, the claim that funds of one program "leveraged" funds provided by other investors is difficult to test, since one cannot readily determine whether such other investments would have occurred at the same level without those of the program. An indirect test of the leverage of the DRA funds is whether significant differences in outcomes are associated with DRA spending. Given the small size of DRA spending per capita, substantial measurable impacts on outcome measures are unlikely without the presence of leveraged funds of other programs and investors. We discuss this issue further later in the paper.

[15]The figures cited in this paragraph on allocation of project funds by project type are based on the project names and funding amounts listed in the DRA Federal Grant Profile, as more detailed descriptions of the projects were not available. Hence, there could be errors in the classification of some of these projects by type.

Table 2

Delta Regional Authority grant value per capita in recipient counties, 2002-07

State	Recipient counties	Mean grant value	Standard deviation
	Number	*Dollars per capita*	
Alabama	17	19.61	17.92
Arkansas	26	19.59	19.72
Illinois	14	32.73	45.68
Kentucky	11	16.94	19.19
Louisiana	31	13.22	24.41
Mississippi	30	13.09	15.16
Missouri	19	13.45	9.86
Tennessee	18	13.13	14.05
Delta region	166	16.75	22.08

Source: USDA, Economic Research Service, calculated from DRA data (2009) and from U.S. Department of Commerce, Bureau of Economic Analysis (for county population), 2002 to 2007.

the largest mean per capita funding in Illinois ($32.73) and the smallest in Mississippi ($13.09). These small amounts of funding per capita suggest that identifying economic impacts of DRA funding using statistical methods is likely to prove difficult. However, given the amounts of other public and private investments that DRA reports to have leveraged, the total per capita investments stimulated that were stimulated by per capita DRA investment could have been substantially greater (on the order of 25 times larger according to the leverage ratios reported by DRA). Hence, the prospect of measuring impacts of the program is not as remote as it may appear, if the program was effective in catalyzing other investment funds.

Initial Economic Outcomes of the DRA

In this section we assess the initial economic outcomes of the DRA's grant program using a combination of quasi-experimental matching methods and multivariate regression analysis. We focus on the period 2002 to 2007 because the program began to be implemented in 2002 and because the data used for the analysis were available up to 2007 at the time the analysis was completed.

Estimation approach

The basic problem in assessing the impacts of any program on program participants is that we don't observe the counterfactual situation (i.e., what would have happened to program participants in the absence of the program). In situations where random assignment of participants is possible, this ensures that the mean outcome observed for the nonparticipant group should be the same as the counterfactual mean outcome that would have been observed for the participants had they not been affected by the program, as long as the program does not affect the outcomes of the nonparticipants.[16] When such conditions are satisfied, an experimental approach can provide a reliable estimate of program impacts. This is why use of randomized experiments is the preferred method for assessing impacts of social programs when this is possible (Heckman et al., 1998). This approach has been used to identify the impacts of job training and social welfare programs in the United States, for example (Ibid.; Moffitt, 2004).

[16]If, to the contrary, a program has "spillover effects" on nonparticipants, then the difference between mean outcomes for participants and nonparticipants does not fully reflect the mean impacts of the program, even if these two groups are randomly selected.

Unfortunately, randomized evaluations are often not possible. This is certainly the case for assessing impacts of programs that are already operating and for which the selection of beneficiaries was not random, such as the DRA. Without random assignment, some method of estimating the counterfactual outcomes is necessary. Often evaluators estimate both the factual and counterfactual outcomes using a predictive model, such as in the evaluations of ARC by Brandow et al. (2000) and BizMiner/Brandow Co. Inc. and EDR Group (2007). The validity of such model-based evaluations hinges on the validity of the model assumptions, which are often questionable, and the empirical validity of the model predictions is often difficult to gauge. One benefit of using empirical ex post evaluation methods is that they can provide a test of the predictions of model-based ex ante methods, as demonstrated by Burchell et al. (1998) in their use of both approaches to evaluate the impacts of EDA programs.

The most commonly used methods of ex post economic impact evaluation of programs include multiple regression analysis, double-difference (DD) estimation, and quasi-experimental matching methods (Ravallion, 2008). Multiple regression analysis usually specifies a parametric statistical model for how the program and other observed confounding factors affect the outcomes of interest, estimating the impact of the program conditional upon the levels of those other factors. DD estimation computes the impact of the program by estimating the mean difference in outcomes for participant and nonparticipant groups during or after the program, and subtracting the mean difference in outcomes between the two groups before the program. Matching methods compare mean outcomes between groups of participants and nonparticipants selected to be similar in observed preprogram characteristics that are thought to jointly affect program participation and outcomes. Since matching tries to mimic the randomized experimental approach in selecting "treatment" and "control" groups that are similar, this approach is referred to as a quasi-experimental design (Cook and Campbell, 1979).

These approaches have different strengths and weaknesses. Parametric regression models provide the most efficient estimator (i.e., the smallest degree of uncertainty) if the parametric assumptions are correct; and a large variety of methods have been developed to test and correct for violations of the basic assumptions. However, these models can give biased results if the parametric assumptions are violated or if factors that affect both program participation (or other explanatory variables) and outcomes are excluded from the model. Matching methods avoid dependence of the results on parametric assumptions about how the program and other factors affect outcomes; as long as there are sufficient numbers of good matches in the nonparticipant group for each observation in the participant group, matching can produce valid impact estimates regardless of the true relationship between observed variables and outcomes. However, like regression methods, matching methods are sensitive to omission of relevant factors that jointly affect participation and outcomes. DD estimation addresses the problem of unobserved confounding factors by subtracting out initial mean differences between the participant and nonparticipant groups. This approach is effective if the confounding factors are fixed over time and have the same additive impact on both groups, since the effects of these factors will be subtracted out (whether or not such confounding factors are observed). However, DD estimation does not address differences that may arise if the two groups had different trends in outcome variables even before the program.

Using combinations of these methods has the potential to address the limitations of individual methods, resulting in more robust conclusions (Ravallion, 2008). For example, use of matching in combination with DD estimation can help to reduce the potential that the two groups were on different trajectories prior to the program (since they will be matched in terms of preprogram characteristics, including possibly their preprogram outcome trajectories), while the DD estimator subtracts out the effects of fixed factors that could cause a simple matched comparison to yield biased conclusions. Similarly, combining DD estimation with regression reduces the confounding effects of fixed or commonly changing factors, and enables controlling for factors that may differ between the groups using the regression model. Finally, combining matching with regression analysis can help reduce the dependence of the regression results on parametric assumptions by assuring that program participants are compared to nonparticipants having similar levels of the explanatory variables.

For these reasons, we use a combination of these methods in our analysis. First, we estimate the impacts of the DRA on changes in outcome variables from 2002 to 2007, comparing DRA-recipient and matched non-DRA counties. This combines DD estimation with matching and is similar to the approach used by Isserman and Rephann (1995) to evaluate the impacts of the ARC, as well as several studies of impacts of enterprise zones and Empowerment Zones.[17] The difference between our approach to matching and that of Isserman and Rephann is that we try several different matching

[17]Recent studies by ERS researchers have also used this approach, including studies assessing the economic impacts of the Conservation Reserve Program (Sullivan et al., 2004) and access to rural broadband (Stenberg et al., 2009).

estimators (including variants of the Mahalanobis metric (MM) estimator used by Isserman and Rephann and other regional scientists, and variants of propensity score matching (PSM), which is commonly used by labor economists) because of strengths and weaknesses of each approach. For simplicity of exposition, in our presentation of results we focus on the results using PSM with kernel matching (PSM-KM), because this estimator provided the best matches in our data.[18] Although there are some differences in our results across estimators, our main conclusions are robust to the choice of matching estimator.

[18]The theoretical strengths and weaknesses of different matching approaches and the results of using different approaches are discussed in appendix A.

To test whether our results using matching and DD estimation are biased by unobserved confounding factors, we use a test suggested by Imbens and Wooldridge (2009): test for significant differences in outcomes using preprogram data. If there are significant differences in preprogram trends in outcome variables between participants and nonparticipants, it contradicts the assumption that the differences in trends observed during the program period are due to the program. As we shall see, we find few such differences in prior trends across different outcome variables. We also compare preprogram and post-program trends in selected outcome variables between the matched groups using graphs, helping to identify whether differences observed during the program period could be explained by preprogram differences in trends.

We also combine matching and DD estimation with regression analysis to correct for errors caused by imperfect matching and to investigate the impacts of the level of DRA program spending. Studies that have used only matching approaches, such as Isserman and Rephann (1995), fail to correct their estimates for the fact that the matches found are imperfect (i.e., there exist differences in the mean values of the covariates between the matched groups, even though the matching generally reduces these differences). Regressions can be used to correct for the effects of these differences. Furthermore, matching approaches fail to account for different levels of program intensity, as reflected by differences in the level of program spending per capita. By using regression analysis on the matched samples, we are able to address both of these limitations. Furthermore, we allow the regression coefficients to differ between the participant and nonparticipant groups (called a “switching regression”), which allows us to test and account for heterogeneous program impacts (i.e., impacts that differ according to different levels of the explanatory variables (Crump et al., 2008)).[19] Our statistical tests strongly supported the model with heterogeneous impacts.

[19]The switching regression model is defined in detail in appendix B.

Study Population

The population and units of observation for this study include nonmetropolitan DRA-recipient counties and other nonmetropolitan counties in the eight DRA States and in three additional States of the Southeastern United States—Georgia, South Carolina and North Carolina. We focus on nonmetropolitan counties because we seek to understand the impacts of the program on rural development.[20] The Southeastern States were included to identify possible matched counties to compare to DRA-recipient counties because of their similarities to the Delta region (especially the southern Delta region) in terms of outcome indicators such as income per capita and poverty, in their economic structure, and in their broad historical context. Within the DRA region, DRA-eligible counties that did not receive DRA program funding

[20]Although “nonmetropolitan” is not a perfect proxy for rural (see http://www.ers.usda.gov/Briefing/Rurality/WhatIsRural/), we use this classification because we are using county-level data, limiting our ability to focus specifically on rural areas within metropolitan or nonmetropolitan counties. An alternative approach could be to focus on nonmetro counties that do not include any urban areas, but this would limit the analysis to 123 counties, including only 36 fully rural DRA recipient counties.

during the time period studied (2002 to 2007) were not included as possible comparison counties because of concerns about spillover impacts of DRA projects to nearby DRA-eligible but nonrecipient counties, which could make such counties a poor choice to represent the counterfactual nonprogram situation. Concerns about selection bias are also greater in comparing DRA-recipient to eligible nonrecipient counties, since eligible nonrecipient counties may be different from recipients in important but unobserved ways, such as in their ability to organize to obtain and manage project funding. We also excluded ARC counties as either DRA-treated counties or as possible controls, to avoid confounding the impacts of the DRA with the impacts of the ARC.[21]

In total, there are 196 nonmetro counties among the 252 DRA-eligible counties. Of these, 133 received DRA funds during 2002 to 2007. Two of these counties are also part of the ARC, so were excluded, as were 131 non-DRA nonmetropolitan ARC counties from the pool of potential control counties. The resulting population included 131 DRA-recipient counties and 330 non-DRA-eligible counties in the 11 States included in the study. The common support requirement[22] used in the matching eliminated 28 of the DRA-recipient counties, leaving 103 DRA-recipient counties in the study sample. The DRA-recipient counties that failed to meet the common support requirement were mainly counties having significant cotton and rice harvested areas, although several other differences exist between these counties and the other DRA-recipient counties.[23] The per capita areas harvested of cotton and rice were included because of strong trends in commodity prices during the study period that could have affected relative changes in farm earnings in DRA vs. non-DRA counties, especially for rice. In initial analysis of the data, these variables were not included among the covariates and much more rapid growth in average farm earnings was found in DRA counties, suggesting unrealistically large positive impacts of the DRA on earnings and income. Inclusion of cotton and rice area in the matching procedures substantially reduced these differences. Rice production was very limited in the study counties outside of the DRA region, so it was not possible to find good matches for major rice-producing counties. Hence, our findings cannot be interpreted as applying to all DRA counties, but rather are limited to nonmetropolitan DRA counties without significant rice area.

Outcome and Control Variables

The outcome variables investigated include county-level personal income per capita and its components (net earnings; dividends, interest, and rent; and personal transfer payments); employment per capita; and population. We also investigated impacts on earnings and employment per capita by major industry classification for the seven largest industries in nonmetropolitan counties of the Delta Region (construction, manufacturing, retail, education, health care and social services, farming, and government). More than 70 percent of the adult working population in the nonmetropolitan DRA-recipient counties was employed in these industries in 2000, with more than 5 percent of adults employed in each. In almost all cases, the earnings and employment impacts by industry were statistically insignificant, so we do not report these in general. We report the impacts on earnings from the regression analysis for only one industry—health care and social services—

[21]In initial analysis of the data, we included DRA-eligible nonrecipient counties, ARC counties, and counties in West Virginia and Virginia as possible controls. The results were qualitatively similar, with significantly faster growth in income and transfer payments per capita in DRA-recipient counties than in matched nonrecipient counties. (These results are available upon request). We excluded counties in West Virginia in the final analysis because these were mostly counties in the ARC. Very few counties in Virginia were selected as matched controls, so Virginia was dropped to simplify the analysis.

[22]See appendix A for an explanation of the common support requirement.

[23]The mean 2002 harvested cotton and rice areas per capita in the 28 counties that failed the common support requirement were 2.45 acres of cotton and 2.57 acres of rice, compared to 0.54 acres of cotton and 0.04 acres of rice for the other nonmetro DRA-recipient counties. These differences are highly statistically significant (p-level less than 0.01 percent). Other statistically significant differences between the DRA counties that failed the common support requirement and other DRA-recipient counties are also evident. In general, the dropped DRA counties have many characteristics associated with greater poverty.

because this is the only industry in which statistically significant impacts were found.[24] We also investigated and report the impacts on different types of transfer payments.

The control variables ("covariates") included in the analysis include many of the same variables used in other studies of impacts of rural interventions on rural economic growth (e.g., Isserman and Rephann (1995), Stenberg et al. (2009)), including indicators of prior outcomes (personal income per capita in 2000, the poverty rate in 2000, shares of personal income from asset returns and from transfer payments in 2001, population in 2000), economic structure (share of adults in 2000 employed in the seven largest industries), and spatial structure (distances to the nearest urban center of different sizes in 1980 (25,000 or more; 100,000 or more; 250,000 or more; 500,000 or more; 1 million or more) and population density in 1990).

Additional covariates not included in Isserman and Rephann (1995) were included in the analysis because these were judged to possibly differ between DRA-recipient counties and non-DRA counties and to potentially affect changes in outcomes. These covariates included indicators of the demographic and educational structure of the population in 2000 (rural share, farm household share, African American share, share age 17 or less and share age 65 or more, share of adults with more than a high school diploma), employment conditions in 1999 (share of men and share of women working full time all year), cotton and rice areas harvested per capita in 2002, Federal economic development grant funds received per capita during 2000-01, and whether the county was in a Gulf Opportunity Zone. Federal economic development funding is a potentially important confounding factor, since such funds may augment or displace funds provided by the DRA. Failure to account for this (and other) confounding factors could have biased the conclusions of prior studies of the impacts of particular economic development interventions. The variable for Gulf Opportunity Zone counties was included to account for potential impacts of Hurricanes Katrina and Rita, and the effects of the Katrina Emergency Tax Relief Act of 2005 (KETRA) and the Gulf Opportunity Zone (GO Zone) Act of 2005. Of the twelve counties most affected by flooding resulting from Hurricane Katrina, only one—Tangipahoa Parish in Louisiana—is a nonmetropolitan county. Excluding this parish from the analysis had little impact on the results.

The data sources used for these variables are summarized in appendix C.

[24]These results are available in Pender and Reeder (2010).

Differences in Outcomes Between DRA Counties and Matched Non-DRA Counties

The results of the estimation using the DD estimator with PSM-KM matching are reported in the second column of table 3. We find that growth in per capita personal income from 2002 to 2007 was greater in the DRA counties than in the matched non-DRA counties, with the difference statistically significant at the 90-percent confidence level. The mean income growth was about $600 per capita higher in DRA counties than in the matched non-DRA counties. The magnitude of this difference was similar using all of the variants of the matching estimators—between $500 and $600, and was statistically significant in most cases (see appendix A). Looking at the major components of personal income, we find a statistically significant difference only in growth of personal transfer payments (growth in transfer payments

Table 3

Mean changes in outcomes, Delta Regional Authority minus matching counties (standard errors in parentheses)[1]

Dependent variable	2002-2007 (DD estimator)	2000-2002 (preprogram DD)
Personal income per capita	597.0*	19.61
	(333.5)	(138.1)
Major components of personal income		
Net earnings per capita	240.2	123.8
	(229.1)	(141.8)
Dividends, interest and rent per capita	164.6	-33.2
	(127.9)	(33.6)
Personal transfer payments per capita	192.1**	-3.3
	(81.5)	(41.0)
Employment per capita	-0.0028	0.0036
	(0.0053)	(0.0043)
Population	-447.5	-132.2
	(543.0)	(88.9)
Transfer payments by type		
Retirement and disability	16.4	-8.4
	(26.8)	(15.4)
Medical	120.5**	-2.4
	(59.2)	(35.7)
Income maintenance	35.3**	19.9***
	(16.0)	(7.1)
Unemployment insurance	6.3	-12.4
	(10.1)	(8.4)
Veterans benefits	9.6	-2.4
	(6.4)	(2.6)
Federal education and training assistance	2.1	-1.3
	(8.1)	(6.2)

*, **, *** Difference statistically significant at 10-percent, 5-percent, and 1-percent levels, respectively. DD=Double difference; see page 21, paragraph 4 for explanation.

[1]Based on 103 DRA-recipient counties and 330 non-DRA counties. Using propensity score—kernel matching (PSM-KM) (standard errors in parentheses).

Source: USDA, Economic Research Service analysis.

was almost $200 per capita higher in DRA counties than in the matched non-DRA counties), although there was a positive but not statistically significant difference in growth of net earnings per capita and in dividends, interest, and rent per capita. The positive and statistically significant impact on transfer payments was robust across most matching estimators, as was the positive but statistically insignificant impact on earnings as well as on dividends, interest, and rent (appendix A).[25] We find no statistically significant difference in growth of employment per capita or in population growth.

Since we found a robust positive association between DRA counties and growth in personal transfer payments, we investigated differences for specific types of transfer payments to better illuminate the nature of the impacts of the DRA. We found significant differences for two types of transfer payments—medical transfer payments (primarily Medicare and Medicaid) and income maintenance program payments (mainly Supplemental Security Income, Temporary Assistance for Needy Families, and the Supplemental Nutrition Assistance Program), with $120 per capita higher growth in medical transfer payments in DRA counties and $35 per capita higher growth in income maintenance program payments. These results were robust across most matching estimators (appendix A).

These differences in growth of medical and income maintenance transfer payments might be due to differences in demographic changes between DRA and non-DRA counties. For example, if the share of the elderly population grew more in DRA counties, this could explain differences in growth of Medicare payments. Similarly, differences in growth of different racial groups could cause differences in use of income maintenance programs, to the extent that participation in these programs differs across racial groups.[26] However, we found no statistically significant differences between DRA and matched non-DRA counties in terms of changes in the age composition of their populations using any estimator, or in the racial composition of their populations using the preferred PSM-KM matching estimator.[27] Thus the evidence does not support these alternative explanations.

Differences in Prior Trends in Outcomes

As noted earlier, if there were differences in prior (and continuing) trends in outcomes between the DRA and matched non-DRA counties, this could bias the results of our analysis. The third column of table 3 presents our analysis of this issue. We find statistically insignificant differences between the matched groups in the growth rates from 2000 to 2002 of all but one of the outcome variables. For those variables, we therefore have no evidence of a difference in prior trends affecting our results. The one outcome variable for which we did find a significant difference in prior trends was income maintenance transfer payments per capita. We do not know why such payments were growing faster in DRA counties prior to implementation of the DRA, but it is unlikely that the DRA was responsible for this difference. Hence, we suspect that the difference in growth of these payments from 2002 to 2007 is also not due to the DRA.

In addition to testing for differences in prior trends, it is useful to visualize these trends. Figure 3 shows the trends in mean personal income per capita in the DRA-recipient counties and matched non-DRA counties (using the

[25]The greater statistical significance for transfer payments is in part because the variance in estimated transfer payments is smaller. This may be because data on transfer payments are based on administrative records and hence may be more reliable than estimates of earnings or dividends, interest, and rent. There also may simply be more actual variation in earnings and dividends, interest, and rent per capita. Statistically insignificant results for these outcomes do not prove that there was no effect on these outcomes, only that such impacts are hard to detect given the sample size available and the large variance of these outcomes.

[26]Recall that our matching estimators matched the DRA and non-DRA counties according to their composition by age and racial groups in 2000, and the resulting counties matched well. Hence we are looking for differences in change of these age and race groups during the study period as a possible explanation for observed changes in transfer payments and other outcomes.

[27]We did find more rapid growth in the African American share of the population in DRA counties according to two of the less-preferred matching estimators (i.e., those that didn't match as well as PSM-KM) (see appendix A). If such a difference truly exists, this could account for more rapid growth in income maintenance payments in DRA counties.

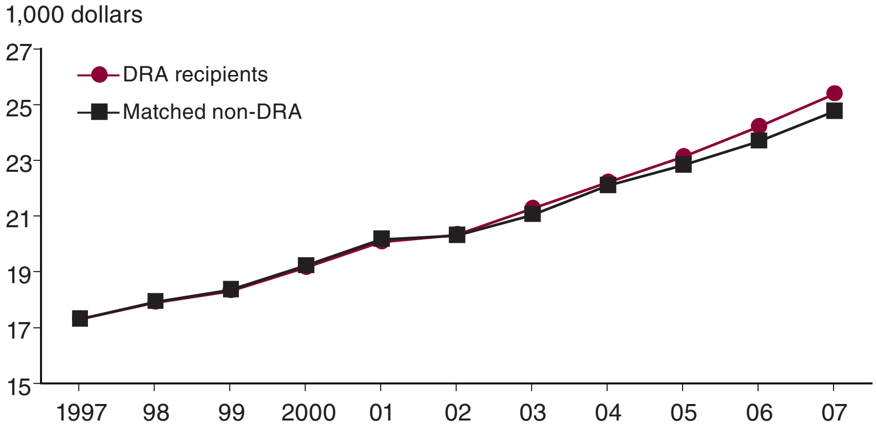
PSM-KM estimator) during 1997 to 2007. The figure shows that the trends in personal income per capita were very similar up to 2002, and then income per capita began to grow more rapidly in the DRA-recipient counties, especially in 2005 to 2007.[28] This figure demonstrates that the differential growth rates in income per capita in DRA and matched non-DRA counties were not the result of a difference in prior trends.

In contrast to the great similarity between the two groups of counties in their prior levels and trends of personal income per capita, there were differences between these groups in their prior levels and trends of mean population (although these differences were not statistically significant) (fig. 4). Thus the negative (though statistically insignificant) value for the difference in

[28]A figure comparing mean personal income per capita in the two groups from 1990 to 2007 shows the same result, with the trends for the two groups almost identical from 1990 to 2002. We present the figure for the shorter time series to better illustrate the divergence that begins after 2002.

Figure 3

Mean annual per capita personal income in DRA-recipient and matched nonrecipient counties (using PSM-KM)[1]

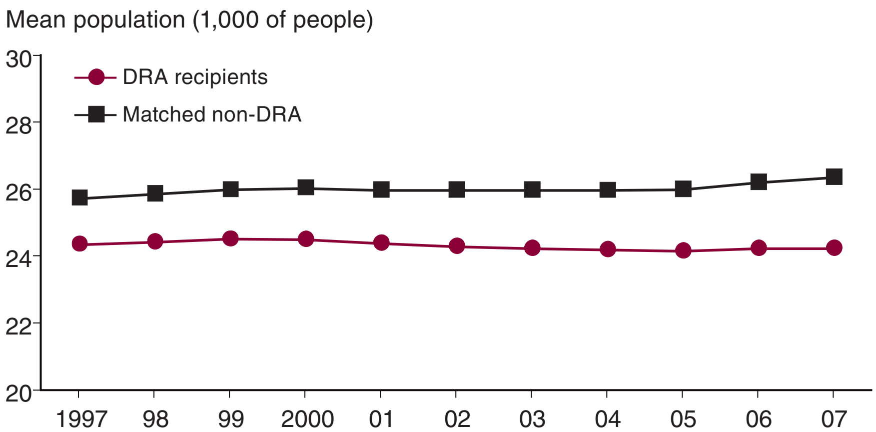


[1]PSM-KM = Propensity score matching with kernel matching.
DRA = Delta Regional Authority.

Source: USDA, Economic Research Service analysis.

Figure 4

Mean population, DRA-recipient and matched nonrecipient counties (using PSM-KM)[1]



[1]PSM-KM = Propensity score matching with kernel matching.
DRA = Delta Regional Authority.

Source: USDA, Economic Research Service analysis.

population growth reported in table 3 reflects a trend of declining population in DRA-recipient counties prior to initiation of the DRA.

Effects of the Level of DRA Spending Per Capita

Table 4 presents the key results of the switching regression analyses conducted for each outcome variable.[29] The magnitudes of the estimated average program effect (the coefficient of the DRA-recipient variable) are similar in most cases to those estimated by the matching estimator, though some of the results are more statistically significant given the smaller standard errors in the regression results. For example, the estimated average program effect on change in personal income per capita is $512, somewhat smaller than the estimate in table 3 ($597), but the regression results are more statistically significant. The estimated mean impact on net earnings per capita is also similar, though somewhat smaller, in table 4 ($223 rather than $240), but this effect is statistically significant at the 10-percent level in the regression. Similarly, the estimated average effects on change in transfer payments per capita and change in medical transfer payments per capita are somewhat smaller than the estimates in table 3, but the results are more statistically significant. For several other outcomes, results that had statistically insignificant impacts in the matching analysis are statistically significant and of the same sign and order of magnitude in the regression results (e.g., retirement and disability payments and veterans' benefits per capita, and population). As discussed in the preceding section, the negative trend of population in DRA= recipient counties began prior to initiation of the DRA, so this result appears to be the result of a prior difference in trends. For one outcome—income maintenance transfer payments—the estimated average effect of the DRA was statistically insignificant in the regression but statistically significant in the matching result.

Table 4 also reports the estimated marginal effects of DRA program spending (the coefficient of the level of DRA spending per capita). We find that several outcomes are associated with greater DRA spending. Personal income per capita grew significantly more in counties with more DRA spending per capita, with each $1 of additional DRA spending per capita associated with $15 of additional growth in personal income per capita. This suggests that DRA spending is having a strong impact on personal income growth, well beyond the simple amount of funds transferred. It seems unlikely that all of this increase can be attributed solely to DRA spending (which would imply an unbelievably large marginal benefit-cost ratio). This suggests that other public or private funds are being leveraged by DRA spending, as claimed by the DRA, although we cannot verify the specific magnitude of other funds leveraged.

The largest component of the estimated increase in personal income per capita is in net earnings (almost $8 of additional net earnings growth per $1 of DRA spending), although this estimate is less statistically significant than the estimated impact on transfer payments (about $5 of additional transfer payments per $1 of DRA spending).[30] This suggests that DRA spending (combined with other funds that this spending may be leveraging) is having a noticeable marginal impact on earnings growth, and is not only having an effect by increasing transfer payments.

[29]Table 4 reports only the coefficients of the dummy variable for whether the county is a DRA-recipient county (the average program effect) and the level of DRA spending per capita (the marginal effect of additional program spending). The full regression results for changes in personal income per capita are provided in appendix B, table B1. Other regression results are available upon request.

[30]Note that the coefficients of DRA spending for the three sources of personal income in table 4 (net income; dividends, interest, rent; and transfer payments) add up to the total marginal impact of DRA spending on personal income. Thus the estimated increase in dividends, interest, and rent is about $2 per $1 of DRA spending, though this coefficient is statistically insignificant.

Table 4

Average and marginal effects of DRA spending based on switching regressions for changes in outcomes, DRA and matching counties, 2002-07 (standard errors in parentheses)[1]

Dependent variable	DRA recipient	DRA funds per capita
Personal income per capita ($)	512.1*** (174.5)	15.32** (6.34)
Major components of personal income		
Net earnings per capita ($)	222.8* (129.7)	7.88* (4.44)
Net earnings per capita from health care and social services ($) (N=52)[2]	321.4 (210.2)	8.21** (3.89)
Dividends, interest and rent per capita ($)	122.7 (81.7)	2.32 (3.52)
Personal transfer payments per capita ($)	166.4*** (45.1)	5.12*** (1.34)
Employment per capita	-0.0035 (0.0037)	0.00001 (0.00013)
Transfer payments by type		
Retirement and disability ($)	31.9** (15.3)	1.67*** (0.50)
Medical ($)	115.5*** (36.1)	2.49** (1.02)
Income maintenance ($)	6.9 (7.9)	0.36 (0.280
Unemployment insurance ($)	-2.4 (5.4)	0.08 (0.130
Veterans benefits ($)	9.2** (4.7)	0.34** (0.13)
Federal education and training assistance ($) (N=194)	-0.5 (5.0)	0.14 (0.17)
Population	-363.4** (187.2)	1.071 (3.020)

*, **, *** Coefficient statistically significant at 10-percent, 5-percent, and 1-percent levels, respectively. DRA=Delta Regional Authority.

[1]Using PSM-NN without replacement to select sample. PSM-NN= propensity score – nearest neighbor matching (see text for explanation of matching approaches).

[2]Regressions for earnings in health care and social services assume equal coefficients of covariates in treated and control samples with an intercept shift; full switching regressions were not possible due to the small number of observations with unsuppressed data.
N=206, except where noted.

Source: USDA, Economic Research Service analysis.

Across major industries, we find a significant impact of additional DRA spending only for the health care and social services sector, with each $1 of additional spending associated with about $8 of additional earnings.[31] These impacts were apparently masked in the matching analysis, which failed to account for differences in the level of DRA spending.

Although the impacts of the DRA apparently go beyond transfer payments, DRA spending is also having a noticeable marginal impact on transfer

[31]The results for the health and social services sector are based on a constrained regression (with equal coefficients of the covariates for both DRA and non-DRA counties), because of a small sample size due to data suppression for earnings in this sector. Given the small sample size, we have less confidence in the results for this sector than for others.

payments. The additional transfer payments are mainly medical transfers ($2.49 per $1 of additional DRA spending) and retirement and disability benefits ($1.67 per $1 of additional DRA spending). These findings indicate that DRA spending leverages other forms of government spending. In the case of medical transfer payments, this may be partly due to DRA investments in medical facilities, though this is not the only potential mechanism of impact.

The DRA seeks to stimulate the supply and demand for medical services in other ways besides its investments in medical facilities. The DRA is increasing the supply of medical services by bringing foreign doctors to the Delta region through its J-1 visa waiver ("Delta Doctors") program. The DRA is one of the few Government agencies that are allowed to recommend such waivers to the State Department (as is the ARC) and has assisted in the placement of more than 100 physicians in the region through the Delta Doctors program (http://www.dra.gov/programs/doctors/). The DRA is also stimulating demand for medical services through its Healthy Delta Initiative, which includes a campaign to address major health problems in the region, especially diabetes (http://www.dra.gov/programs/health-improvement/).

The positive impacts of DRA spending on medical transfer payments and on net earnings per capita are consistent with the fact that the health care sector is the only one found to have greater earnings as a result of greater DRA spending, which suggests that a major near-term impact of the DRA has been to promote health sector earnings and medical transfer payments.

Robustness of the Results to Alternative Specifications

In addition to investigating the robustness of our findings to alternative matching and regression methods, we also investigated the effects of a large number of alternative scenarios, including:

- alternative sets of covariates in the analysis (e.g., with or without controlling for the area of cotton and rice, distance to urban centers, population density, GO Zones, or demographic variables)
- including additional covariates to control for possible additional confounding factors, including the presence of a Critical Access Hospital and the number of Federally Qualified Health Centers, the preprogram level of total transfer payments or medical transfer payments per capita, and the rate of population growth in the 1990s
- alternative functional forms for the variables in the analysis (i.e., using logarithmic transformations of the continuous variables vs. untransformed variables)
- alternative definitions of the potential population of controls (i.e., including counties in the Appalachian region in early specifications, including only counties in DRA States in the analysis, using nonrecipient DRA-eligible counties as possible controls, dropping from the analysis counties that suffered large losses in Hurricane Katrina, dropping counties that are affected by the Delta Health Alliance, and dropping counties that are part of a Federal Empowerment Zone, Enterprise Community, or Renewal Community)

- alternative specifications of the base and ending years in the analysis (i.e., considering 2001 rather than 2002 as the base year and 2005 or 2006 as ending years)

Many of these scenarios are discussed in greater detail, and selected results for them are presented in appendix D.[32] Our main findings—that growth in personal income and transfer payments, especially medical transfer payments, was more rapid in DRA-recipient counties than matching counties and that greater growth was associated with greater DRA spending—were generally robust to these alternative specifications.

[32]The full results using alternative specifications are available from the authors upon request. Some of these results are discussed in more detail in Pender and Reeder (2010).

Conclusions

The results of this analysis suggest that even though the DRA is a relatively small program and its impacts could only be investigated during its first 6 years of implementation, the program is associated with measurable positive impacts on some outcomes, including per capita personal income, net earnings, and transfer payments. These impacts were larger in counties where DRA spending per capita was greater, with each $1 of additional DRA spending per capita associated with an additional $15 in growth of personal income per capita, including $8 in additional earnings, primarily in the health care and social service sector, and $5 in additional transfer payments, mainly due to additional medical transfer payments. The effect of higher DRA spending on health sector earnings and medical transfer payments is consistent with the fact that spending on medical facilities is one of the priority areas of DRA spending. It is also consistent with the DRA's efforts to increase the supply of doctors and promote improved health awareness in the Delta Region.

The fact that we do not find measurable impacts of the DRA so far on other outcome indicators, such as on earnings per capita in most industries or on employment per capita, does not mean that there have not been any such impacts or will not be in the future. Given the relatively small amount spent by the DRA in DRA-recipient counties so far and the time required for investments in infrastructure to affect economic growth, it is not surprising that it is difficult to detect impacts on more fundamental indicators, such as jobs and broadbased earnings, after only 6 years of program implementation. Furthermore, many of the largest investments made by the DRA have been in community facilities such as improved water and sewer systems, which improve the quality of life but may have little direct near-term impact on employment or income, although they may promote community economic development in the longer term by attracting new residents and industries and reducing outmigration.

It is perhaps more surprising to find such large incremental impacts of DRA spending on personal income, earnings, and transfer payments. The results suggest that these impacts are not simply the direct result of DRA funds circulating in the local economies of the Delta Region, since the multipliers are far larger than those typically estimated for spending in rural areas. Rather, it appears that DRA programs related to improving the supply of health facilities and doctors and improving health awareness of Delta residents may be leveraging additional resources through medical programs such as Medicare and Medicaid, and that these are contributing to increased earnings in the health care and social service sector. This suggests that the DRA and associated investments by other agencies are addressing supply constraints for health services that limit the ability of rural people in the region to use the medical transfer payments that they are entitled to, and it highlights the importance of increasing the supply of health services in rural areas as well as providing support to payments for health care.

The finding that each $1 of DRA spending is associated with $15 of additional growth in personal income suggests that the program has been successful in leveraging other public and possibly private funds. We see this directly with the estimated impact on transfer payments, although this is not the kind of leverage that is claimed by the program. The DRA may also be leveraging other economic development program funding, as intended, although we have not investigated this leverage impact directly in this analysis. This would be a worthwhile topic of future research. To the extent that other public funds (including transfer payments) are being leveraged, one cannot claim the estimated impact on personal income or earnings as the return to DRA investments exclusively, since other public funds were also used to achieve this impact.

The estimated impacts of the DRA on income in the health care sector raise the general question of the potential for investments in that sector to contribute to economic development in rural areas. Ours is not the first study to notice the potential economic impacts of health sector investments in rural areas of the United States. There is a growing body of literature on such impacts, led by researchers at the National Center for Rural Health Works at Oklahoma State University and other research centers (e.g., Doeksen et al.,1998; Doeksen and Schott, 2003; St. Clair, Doeksen, and Schott, 2007; St. Clair and Doeksen, 2009). However, most of that literature estimates impacts of health sector investments using an economic input-output model, without being validated by empirical ex post estimates of the impacts of actual investments. The present study is the only one that we are aware of that estimates such impacts using quasi-experimental and other econometric methods with county-level income data. Although we did not start out specifically hypothesizing impacts of the DRA on income in the health sector, the fact that we found evidence of such impacts suggests that further empirical research on the impacts of this type of investment in rural areas could prove fruitful.

It would also be useful to investigate impacts of the DRA on other outcomes beyond the economic ones considered in this study. Given the impacts on medical transfer payments and health sector earnings that we have observed, assessing impacts on health outcomes would also be valuable. Furthermore, since many of the investments by the DRA focus on improving the quality of life and not necessarily economic improvements (in the short term), it would be very useful to identify and assess impacts on other outcome indicators more directly affected by these investments, such as improvements in drinking water quality and sanitation, and health and environmental indicators related to these improvements. As many of these impacts may take a longer time to become evident, longer term assessments are likely to be needed.

As we have noted, this study is not intended to be a formal or thorough evaluation of all of the impacts of the DRA, but rather is a test of whether any initial economic impacts could be detected using publicly available data and the best available assessment methods. We have contributed to the literature evaluating impacts of regional rural development programs by focusing on an important regional program that has not yet been assessed in any published reports, by using a variety of statistical methods and specifications to control for confounding influences and to check the robustness of our results, by controlling for several factors that have not been adequately accounted for in prior assessments of economic development programs, and by assessing impacts of the level of program spending per capita as well as the presence of the program. Our findings do not replicate the findings of several other influential studies of impacts of rural development programs, since we do not find significant impacts of the DRA on employment. This is perhaps not too surprising given the small size of the program. However, our findings suggest that economic impacts can occur through mechanisms other than by increasing employment, such as by leveraging increased transfer payments. This suggests additional avenues that could be fruitful to pursue in future research on the impacts of regional economic development programs.

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Appendix A—Matching and Double Difference (DD) Estimation of Impacts of DRA

Quasi-experimental matching approaches seek to mimic random assignment by selecting nonparticipants that are similar to participants in terms of selected observable characteristics. This matching method can reduce biases caused by differences between program participants and nonparticipants (or "treated" vs. "controls") in these observable characteristics (i.e., this addresses the problem of "selection on observables") (Heckman et al., 1998). However, this does not assure that differences between the treated and controls in unobserved characteristics are negligible, and to the extent that such unobserved differences contribute to differences in outcomes, this could still bias the results of the analysis (i.e., the problem of "selection on unobservables").

We address these problems by combining the use of matching methods with difference-in-differences (DD) estimation. The DD estimator estimates the average impact of a program on the participants (or "average impact of the treatment on the treated" (ATT)) as the difference between the mean outcomes for the treated and control groups after the program is implemented, minus the difference in outcomes before the program is implemented; that is, $(EY_{T1} - EY_{C1}) - (EY_{T0} - EY_{C0})$, where EY_{T1} and EY_{T0} are the mean outcomes for the treated group in period 1 and 0, respectively (where period 1 is during or after program implementation and period 0 is before implementation), and EY_{C1} and EY_{C0} are the mean outcomes for the control group. This is equivalent to the change in mean outcome for the treatment group minus the change in mean outcome for the control group $((EY_{T1} - EY_{T0}) - (EY_{C1} - EY_{C0}))$. This estimator subtracts out the effects of any time invariant additive factors that differ between the treated and control groups and any common trends affecting both groups. Thus, as long as the effects of differences between the two groups are additive and time invariant, this method eliminates bias due to selection on unobservables or observables (Imbens and Wooldridge, 2009).

Unfortunately, the assumption that differences between the two groups are time invariant may fail to hold in practice. For example, development programs may be attracted to locations where incomes are rising more rapidly (or more slowly) for reasons other than the program. One way to address this potential problem is to use the DD estimator for matched treatment and control groups, in which the variables used for matching are those that are expected to differ between the groups and to influence changes in outcomes over time (Ravallion, 2008). This approach is similar to the conditional difference-in-differences estimator proposed by Heckman et al. (1998), which they found to be a promising method to address selection bias in evaluating a job training program. Smith and Todd (2005) also found that this approach substantially reduced the bias in evaluating a job training program caused by time invariant sources of cross sectional variation, and that the advantages were robust across a range of matching methods and model specifications using different subsamples of the data and different survey instruments. Isserman and Rephann (1995) used this approach to assess the impacts of the ARC, combining Mahalanobis metric matching with DD estimation of differences in growth rates of income, population and earnings between ARC and matched non-ARC counties. Ravallion and Chen (2005) also used this approach, using propensity score matching to reduce observable preproject differences between participants and nonparticipants in a development project in China, and then DD estimation for the matched sample.

We use several alternative matching estimators combined with DD estimation. Propensity score matching (PSM) has been used in many studies of impacts of social programs. PSM matches participants and nonparticipants according to the probability of program participation (or "propensity score", denoted P(X), where X includes the observable characteristics used to predict participation). Rosenbaum and Rubin (1983) proved under the assumption of "unconfoundedness" that Y_o is also independent of treatment status conditional upon P(X), provided that $0 < P(X) < 1$.[33] Under this assumption, matching on the propensity score is sufficient to ensure that the outcomes for the matched nonparticipant group are statistically indistinguishable from the outcomes that the participants would have experienced in the absence of the program.

We use PSM nearest neighbor matching (PSM-NN), with and without replacement. Matching with replacement allows control observations to be used as the best match for more than one treated observation; hence it tends to obtain better matches with less potential bias resulting from imperfect matches. However, use of fewer control observations results in larger standard errors and in many cases a larger mean squared error, despite less bias (Zhao, 2004; Smith and Todd, 2005). We also use PSM with kernel matching (PSM-KM), which estimates matching observations based on a weighted average of observations from the nonparticipant pool, with the weights a declining function of the distance of each observation (in terms of its propensity score) from the observation in the treatment group to be matched (Heckman et al., 1998). Kernel PSM is able to obtain lower standard errors than NN matching, since it uses more information to construct the counterfactual observations, but this may be at a cost of increased bias (Caliendo and Kopeinig, 2005). We use the Epanechnikov kernel function and a bandwidth of 0.06, which are the default options in the Stata procedure used for PSM (Leuven and Sianesi, 2003). In general, results of PSM-KM are not very sensitive to the choice of the kernel function, as with nonparametric regression approaches (Caliendo and Kopeinig, 2005; DiNardo and Tobias, 2001). The choice of the bandwidth parameter appears to be more important, but involves a tradeoff between bias and variance—i.e., a high bandwidth yields a smoother density function estimation and reduced variance, but may be more biased by smoothing out underlying features of the actual function (Caliendo and Kopeinig, 2005).

To avoid observations with very high (P(X) near 1) or low (P(X) near 0) propensity scores, which will have poor matches, we impose a condition of "common support", which drops treatment observations whose estimated propensity score is higher than the maximum or less than the minimum estimated propensity score of the control group (Leuven and Sianesi, 2003).

We also use the Mahalanobis metric (MM) matching estimator. The MM estimator minimizes the distance function $d_{TC} = (X_T - X_C)' \Sigma^{-1} (X_T - X_C)$, where X_T and X_C are vectors of matching variables for the treatment and potential control observations (considering all possible controls, and not only matched ones), and Σ is the variance-covariance matrix of X_C.

[33]The assumption of unconfoundedness is the assumption that the outcome that would have occurred without the treatment (denoted as Y_o) is independent of treatment status (D), conditional upon X. The assumption that $0 < P(X) < 1$ ensures that there are members of the comparison group for both treated and untreated units of observation. That is, if $P(X) = 0$ there are no treated observations for this value of X, and if $P(X) = 1$, there are no control observations. To estimate the average effect of the treatment on the treated (ATT), the assumption that $P(X) > 0$ is not necessary, since the requirement is only to find matches for each treated observation (the requirement $P(X) < 1$ is necessary in this case). If instead of ATT, the average treatment effect on the population (ATE) is to be estimated (including the potential impact of the treatment on controls), then the assumption $P(X) > 0$ is also necessary.

There is no theorem comparable to that of Rosenbaum and Rubin (1983) providing a theoretical justification for the MM method, and it often is more biased (in terms of differences in mean values of X_T and X_C in matched samples) than PSM, especially when a large number of covariates are involved (Gu and Rosenbaum, 1993; Zhao, 2004). Intuitively, PSM achieves balance by implicitly giving greatest weight to matching on the variables that have significant association with the treatment assignment. MM matching attempts to achieve balance in all covariates, weighted by the inverse variance matrix of the covariates, and so may overweight variables that have little association with the treatment assignment (and hence are of little concern regarding bias), especially with a large number of covariates. Nevertheless, the MM estimator often has lower standard errors than the PSM estimator and in many cases lower mean squared error, despite being more biased (Zhao, 2004).

Another advantage of the MM estimator relative to PSM is that the estimated standard errors for MM are asymptotically consistent, provided that the bias resulting from imperfect matching on covariates is corrected (Abadie and Imbens, 2006).[34] To address the bias, we use the MM version of the matching estimator developed by Abadie et al. (2004), which corrects the bias in estimating the ATT using a linear least squares regression of the outcome on the covariates for the matched control observations.[35] Abadie and Imbens (2007) showed, using Monte Carlo simulations, that their bias corrected estimator substantially reduces bias and mean squared error compared to matching without bias adjustment and to linear and quadratic regression models. This estimator is available only for nearest neighbor matching with replacement, so we implement it for that case only.

For PSM, the estimated standard errors are not valid, both because of imperfect matching and because the estimated standard errors do not account for the fact that the propensity scores are estimated in a first stage estimation. We address the bias in one version of the PSM model (nearest neighbor with replacement) using the bias corrected estimator of Abadie et al. (2004). In this case, we use the estimated propensity score from a first stage probit model as the single covariate in the covariate matching algorithm.[36] This reproduces the ATT estimated by the standard PSM model when no bias correction is used, although the estimated standard error is different. With the bias correction, this estimator corrects for the effects of differences in propensity scores (but not in the individual covariates) between the treated and matched control observations on the estimated counterfactual outcome.

We use bootstrapping to estimate the standard errors for all PSM estimators used (PSM-NN with replacement, with or without bias correction; PSM-NN without replacement, PSM-KM). This is standard practice among researchers to account for the fact that the propensity scores are estimated in a first stage estimation, but it doesn't address the error caused by imperfect matches. Abadie and Imbens (2008) proved that the use of bootstrapping is not generally valid for matching estimators, and demonstrated the inconsistency of the bootstrap estimator for a specific case of nearest neighbor covariate matching (for a scalar covariate) with replacement. They argue that bootstrapping may be valid with kernel PSM estimation because the number of matches increases with sample size, but do not prove this. Despite this problem, we

[34]Abadie and Imbens (2006) proved the consistency and asymptotic normality of a class of bias-corrected covariate matching estimators that includes the Mahalanobis metric as a special case (Ibid., footnote 4, p. 239).

[35]Formally, Abadie et al. (2004) estimate the counterfactual outcome for each treated observation i (Y_{oi}) as: $Y_{oi} = (1/\#m(i)) \Sigma_{k \in m(i)}\{Y_{ok} + \mu_o(X_i) - \mu_o(X_k)\}$, where m(i) is the set of matched control observations to treated observation i, #m(i) is the number of matched observations in this set, Y_{ok} is the outcome of matched control observation k (within m(i)), and $\mu_o(X)$ is the estimated linear regression function of the outcome on the covariates within the matched control group. The terms $\mu_o(X_i) - \mu_o(X_k)$ correct the estimated counterfactual outcome for differences resulting from differences in the values of the covariates between the treated (X_i) and matched control observations (X_k).

[36]We use a probit model to estimate propensity scores. Other parametric probability models, such as a logit or linear probability model, are also commonly used, as well as nonparametric probability models. Results of propensity score estimation with a binary treatment are generally not highly sensitive to the choice of probability model (Zhao, 2004; Caliendo and Kopeinig, 2005).

use bootstrapping to estimate the standard errors for our PSM models due to lack of a suitable alternative.[37]

As we have seen, no matching method is clearly superior to all others in terms of both bias reduction and efficiency. Furthermore, PSM models suffer from inconsistent estimation of the standard errors. Although the MM estimator with bias correction has the advantages of being bias corrected and using asymptotically valid estimates of the standard errors, it generally has to correct for larger biases than PSM estimates, and thus can be greatly affected by the linear regression model used to correct for bias. This is an important drawback, since one of the advantages of matching methods over parametric regression methods is that they seek to avoid dependence on parametric assumptions about the relationships between the covariates and the outcome variable. Given these tradeoffs, we investigate the robustness of our conclusions to these different matching methods. To investigate how much difference is made by the bias correction, we report the results of the MM estimator and the PSM-NN estimator (without replacement in both cases) both with and without the bias correction.

Comparisons between the covariates in the unmatched and matched samples are shown in tables A-1 and A-2. The mean values of many of the covariates differ between DRA counties and non-DRA counties in the unmatched samples. In general, these comparisons indicate that DRA-recipient counties were poorer and more dependent upon Federal spending than non-DRA counties in the Delta and Southeast States, with a smaller share of the adult population employed and greater dependence on service occupations. Such initial differences may affect differences in outcomes during the study period, and therefore need to be controlled for using econometric methods.

Table A-1 indicates that most of these mean differences in characteristics are much smaller in the matched samples using the propensity score–nearest neighbor matching method (PSM-NN) with replacement. Statistically significant differences remain in the matched samples for only a few variables: the share of adults employed in manufacturing (less in DRA counties), cotton harvested area per capita (more in DRA counties), and the elderly share of the population (less in DRA counties). In all of these cases, the statistical significance is weak (between the 5-percent and 10-percent level) and the mean differences are relatively small. Across all covariates, the maximum absolute standardized bias is reduced from over 100 percent to 27 percent.[38] The pseudo R^2 of the probit model is much lower in the matched sample, and a likelihood ratio (LR) test of overall balance in the matched sample indicates that differences in the covariates are statistically insignificant, with a p value of 0.103.[39] Hence, this matching method performs well to reduce, if not eliminate, all differences between the DRA-recipient counties and the matched non-DRA counties in their pre-DRA characteristics.

Table A-2 provides similar comparisons between the matched samples using the other matching methods investigated.[40] Figure A-1 shows the DRA-recipient nonmetro counties (with common support) and the matched nonmetro non-DRA counties, using the PSM-NN estimator without replacement. This matching estimator results in larger biases for some variables (with a maximum absolute bias of nearly 36 percent) and more statistically significant differences (compared to matching with replacement), because the constraint

[37]In a recent unpublished working paper, Abadie and Imbens (2009) derive the asymptotic standard error for the PSM estimator of the average treatment effect (considering nearest M neighbor matching with replacement), taking into account the fact that the propensity scores are estimated. Remarkably, they find that the standard error is less when the propensity score is estimated, indicating that use of uncorrected standard errors will lead to conservative inferences when rejecting the null hypothesis (i.e., the true probability of falsely rejecting the null hypothesis will be less than the p-value of the test). However, this result is only for the population average treatment effect and need not apply to the variance of the ATT (op cit., p. 8), which is what we are interested in estimating.

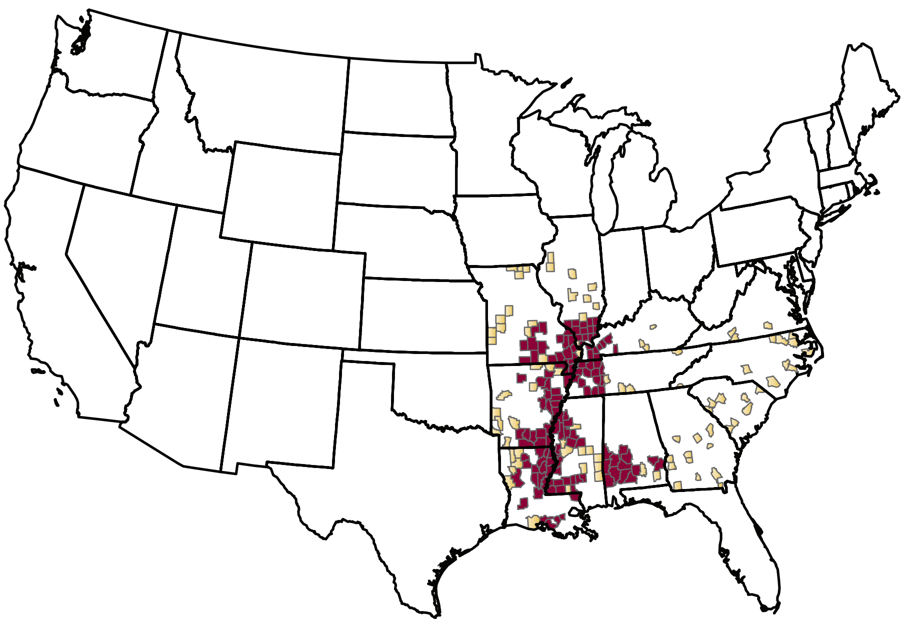
[38]The sample standardized bias for covariate X is defined as $(m(X_t)-m(X_c))/\text{square root}(s_t^2+s_{cr}^2)$, where $m(X_t)$ and $m(X_c)$ are the sample means for the treated and control groups (whether matched or unmatched), respectively; and s_t^2 and s_{cr}^2 are the sample variances for the treated group and control reservoir (unmatched controls), respectively (Rosenbaum and Rubin, 1985). The standardized bias is divided by this denominator (rather than the variance of the difference in means, as in a t statistic) so that the measure is not affected by sample size and is comparable between different matching methods.

[39]The overall balance test is a likelihood ratio test of the joint statistical significance of all covariates in a probit model for program participation in the matched sample. If the samples are well matched, the covariates should have a statistically insignificant impact in this model.

[40]The comparisons between unmatched samples do not vary across the matching methods, so these comparisons are not shown again in table A-2. The mean levels of all covariates for the DRA counties are the same for all matching methods, so these are reported only once in table A-2 for comparison purposes. The difference between these matching methods is in their choice of matched non-DRA counties.

Figure A-1

Matched DRA-recipient nonmetro counties and non-DRA nonmetro counties[1]



Matched non-DRA nonmetro counties

DRA-recipient nonmetro counties

[1]Using PSM-NN without replacement. PSM-NN without replacement=propensity score nearest neighbor matching, without replacement.
DRA=Delta Regional Authority. The DRA operates in Louisiana, Alabama, Mississippi, Arkansas, Tennessee, Missouri, Kentucky, and Illinois.
For a list of the 252 DRA-eligible counties, see http://www.dra.gov/about/maps.aspx/.

Source: USDA, Economic Research Service analysis, 2010.

of nonreplacement limits the ability to use the best matching counties more than once. With this estimator, there are statistically significant differences between the DRA and matching non-DRA counties in terms of the poverty rate (greater in DRA counties), the share of adults employed in manufacturing (less), whether the county is in a GO Zone (more likely), rice-harvested area per capita (greater), the farm share of the population (less), the child share of the population (greater), and the share of women working full time all year (less). Despite having larger biases and more significant differences for several individual covariates, the PSM-NN estimator without replacement has a lower overall measure of bias, with a smaller pseudo R^2 and smaller LR test statistic than the PSM-NN estimator with replacement. Hence it is not clear whether the PSM model with or without replacement is preferable.

The PSM kernel matching (PSM-KM) estimator performs the best, with no statistically significant mean differences for any covariates, the smallest maximum bias (24 percent), the smallest pseudo R^2, and the smallest LR test statistic. The Mahalanobis metric (MM) estimator performs the poorest in terms of bias, with significant differences remaining between the DRA and matched samples for 12 of the covariates, the largest maximum bias (nearly 62 percent), and the largest pseudo R^2 and LR test statistic (indicating statistically significant difference overall between the matched samples).

Table A-1

Comparison of characteristics of unmatched and matched Delta Regional Authority and comparison samples[1]

Variable	Sample	Mean: Treated	Mean: Control	Percent bias	p>|t|
Personal income per capita, 2000 ($)	Unmatched	18,755	20,703	-70.3	0.000***
	Matched	19,147	19,139	0.3	0.981
Population, 2000	Unmatched	23,876	26,265	-12.4	0.273
	Matched	24,483	26,102	-8.4	0.559
Poverty rate, 2000 (percent)	Unmatched	19.96	15.63	81.8	0.000***
	Matched	19.02	18.20	15.6	0.212
Share of personal income from personal transfer payments, 2001	Unmatched	0.2681	0.2277	83.3	0.000***
	Matched	0.2616	0.2620	-0.9	0.951
Share of personal income from dividends, interest and rent, 2001	Unmatched	0.1659	0.1839	-51.9	0.000***
	Matched	0.1702	0.1674	8.2	0.458
Share of adults employed in agriculture, forestry, fishing or hunting, 2000	Unmatched	0.0568	0.0540	7.8	0.446
	Matched	0.0484	0.0481	0.8	0.943
Share of adults employed in construction, 2000	Unmatched	0.0720	0.0775	-26.7	0.011**
	Matched	0.0750	0.0726	11.5	0.408
Share of adults employed in manufacturing, 2000	Unmatched	0.1978	0.2225	-33.0	0.001***
	Matched	0.1990	0.2180	-25.4	0.069*
Share of adults employed in retail trade, 2000	Unmatched	0.1141	0.1136	3.3	0.757
	Matched	0.1149	0.1152	-1.4	0.925
Share of adults employed in public administration, 2000	Unmatched	0.0559	0.0527	13.4	0.200
	Matched	0.0542	0.0496	19.3	0.120
Share of adults employed in educational services, 2000	Unmatched	0.0926	0.0835	33.2	0.001***
	Matched	0.0927	0.0891	12.9	0.386
Share of adults employed in health care or social services, 2000	Unmatched	0.1156	0.1077	31.5	0.002***
	Matched	0.1167	0.1165	0.9	0.953
Federal economic development grant funds per capita, 2000-01 ($)	Unmatched	367.21	285.78	17.6	0.097*
	Matched	336.88	304.91	6.9	0.538
Gulf Opportunity Zone counties (share of counties)	Unmatched	0.1832	0.0394	46.8	0.000***
	Matched	0.2233	0.3010	-25.3	0.207
Cotton-harvested acres per capita, 2002	Unmatched	0.9494	0.3330	43.1	0.000***
	Matched	0.5409	0.2451	20.7	0.079*
Rice-harvested acres per capita, 2002	Unmatched	0.5834	0.0017	57.8	0.000***
	Matched	0.0430	0.0282	1.5	0.395
Distance to the nearest urban center of 25,000 or more, 1980 (miles)	Unmatched	37.28	35.03	8.4	0.424
	Matched	37.04	38.22	-4.4	0.737
Distance to the nearest urban center of 100,000 or more, 1980 (miles)	Unmatched	85.43	82.53	5.3	0.611
	Matched	85.72	86.25	-1.0	0.943
Distance to the nearest urban center of 250,000 or more, 1980 (miles)	Unmatched	149.04	139.37	11.1	0.290
	Matched	146.89	157.16	-11.8	0.380
Distance to the nearest urban center of 500,000 or more, 1980 (miles)	Unmatched	236.17	225.97	7.1	0.503
	Matched	235.90	244.16	-5.8	0.682
Distance to the nearest urban center of 1,000,000 or more, 1980 (miles)	Unmatched	377.79	397.67	-9.9	0.358
	Matched	371.46	395.03	-11.8	0.398

—continued

Table A-1
Comparison of characteristics of unmatched and matched Delta Regional Authority and comparison samples[1]—Continued

Variable	Sample	Mean: Treated	Mean: Control	Percent bias	p>|t|
Population density, 1990 (persons/square mile)	Unmatched	40.29	46.06	-17.3	0.103
	Matched	42.61	41.19	4.3	0.738
Rural share of population, 2000	Unmatched	0.6785	0.7066	-12.3	0.222
	Matched	0.7070	0.7452	-16.7	0.256
Farm share of population, 2000	Unmatched	0.0317	0.0501	-54.1	0.000***
	Matched	0.0341	0.0381	-11.7	0.296
Black share of population, 2000	Unmatched	0.2805	0.1868	43.3	0.000***
	Matched	0.2590	0.2524	3.1	0.828
Share of population age 17 or less, 2000	Unmatched	0.2583	0.2505	29.3	0.002***
	Matched	0.2546	0.2511	13.0	0.345
Share of population age 65 or more, 2000	Unmatched	0.1495	0.1520	-9.2	0.398
	Matched	0.1504	0.1577	-27.4	0.053*
Share of adults with more than a high school education, 2000	Unmatched	0.3269	0.3490	-30.6	0.004***
	Matched	0.3370	0.3301	9.5	0.508
Share of men working full time all year, 2000	Unmatched	0.5732	0.6144	-71.4	0.000***
	Matched	0.5764	0.5778	-2.5	0.860
Share of women working full time all year, 2000	Unmatched	0.3937	0.4308	-102.7	0.000***
	Matched	0.3915	0.3912	0.8	0.957

Overall balance tests		Pseudo R^2	LR chi^2	$p>chi^2$
	Unmatched	0.429	236.26	0.000***
	Matched	0.140	40.12	0.103

*, **, *** Difference statistically significant at 10-percent, 5-percent, and 1-percent levels, respectively.
[1]Using PSM-NN with replacement. PSM-NN=propensity score nearest neighbor matching. DRA=Delta Regional Authority.
Source: USDA, Economic Research Service analysis.

These results are consistent with results of other studies that compare different matching methods (Gu and Rosenbaum, 1993; Zhao, 2004), and demonstrate that no matching method is clearly superior in terms of both bias reduction and efficiency. Hence, as noted earlier, we report the results of several methods and investigate the robustness of our conclusions to the method.

DD Estimates With Matching Methods

The results of the estimation using the DD estimator for changes in the outcome measures using the different matching methods are reported in Table A-3. We find that growth in per capita personal income from 2002 to 2007 was greater in the DRA counties than in the matched non-DRA counties, with the difference statistically significant (at the 10-percent level or less) for four of the six matching estimators. In all cases, the mean difference in the growth of annual per capita income from 2002 to 2007 was in the range of $500 to $660 per capita, a fairly large difference. This difference was not statistically significant using either bias corrected estimator, however. This is due mainly to larger standard errors of the bias corrected estimators.

Table A-2

Comparison of characteristics of matched Delta Regional Authority and comparison samples[1]

		PSM-NN without replacement			PSM-KM			Mahalanobis		
Variable	Mean treated	Control	Percent bias	p>ltl	Control	Percent bias	p>ltl	Control	Percent bias	p>ltl
Personal income per capita, 2000 ($)	19,147	19,385	-8.6	0.512	19,205	-2.1	0.876	19,865	-25.9	0.036**
Population, 2000	24,483	23,345	5.9	0.655	26,026	-8.0	0.542	28,348	-20.0	0.069*
Poverty rate, 2000 (percent)	19.02	17.56	27.6	0.035**	18.07	18.0	0.160	16.46	48.3	0.000***
Share of personal income from personal transfer payments, 2001	0.2616	0.2536	16.5	0.250	0.2585	6.5	0.650	0.2372	50.3	0.000***
Share of personal income from dividends, interest and rent, 2001	0.1702	0.1708	-1.7	0.884	0.1673	8.4	0.473	0.1683	5.5	0.629
Share of adults employed in agriculture, forestry, fishing or hunting, 2000	0.0484	0.0496	-3.4	0.757	0.0456	7.9	0.462	0.0464	5.6	0.589
Share of adults employed in construction, 2000	0.0750	0.0752	-1.1	0.937	0.0763	-6.2	0.657	0.0762	-6.2	0.628
Share of adults employed in manufacturing, 2000	0.1990	0.2202	-28.3	0.040**	0.2113	-16.4	0.235	0.2388	-53.1	0.000***
Share of adults employed in retail trade, 2000	0.1149	0.1134	9.2	0.523	0.1139	6.3	0.671	0.1145	2.3	0.843
Share of adults employed in public administration, 2000	0.0542	0.0518	10.4	0.433	0.0523	8.3	0.523	0.0460	34.4	0.004***
Share of adults employed in educational services, 2000	0.0927	0.0901	9.5	0.557	0.0888	14.1	0.329	0.0865	22.2	0.140
Share of adults employed in health care or social services, 2000	0.1167	0.1139	11.4	0.414	0.1171	-1.6	0.911	0.1058	43.6	0.001***
Federal economic development grant funds per capita, 2000-01 ($)	336.88	298.61	8.3	0.479	281.99	11.9	0.313	287.84	10.6	0.275
Gulf Opportunity Zone counties	0.2233	0.1262	31.6	0.067*	0.2976	-24.2	0.226	0.2039	6.3	0.735
Cotton harvested acres per capita, 2002	0.5409	0.3899	10.5	0.406	0.3181	15.6	0.197	0.2558	19.9	0.116
Rice harvested acres per capita, 2002	0.0430	0.0048	3.8	0.007***	0.0239	1.9	0.257	0.0236	1.9	0.249
Distance to the nearest urban center of 25,000 or more, 1980 (miles)	37.04	34.62	9.0	0.502	36.98	0.2	0.986	35.84	4.4	0.730
Distance to the nearest urban center of 100,000 or more, 1980 (miles)	85.72	76.31	17.2	0.209	87.50	-3.3	0.814	84.62	2.0	0.878
Distance to the nearest urban center of 250,000 or more, 1980 (miles)	146.89	147.20	-0.4	0.980	154.10	-8.3	0.557	136.83	11.6	0.361
Distance to the nearest urban center of 500,000 or more, 1980 (miles)	235.90	220.83	10.5	0.447	238.68	-1.9	0.892	210.46	17.8	0.202
Distance to the nearest urban center of 1,000,000 or more, 1980 (miles)	371.46	362.64	4.4	0.745	383.39	-6.0	0.666	316.47	27.5	0.020**
Population density, 1990 (persons/ sq. mile)	42.61	38.48	12.4	0.295	41.83	2.4	0.854	43.48	-2.6	0.838
Rural share of population, 2000	0.7070	0.7187	-5.2	0.721	0.7372	-13.2	0.356	0.6974	4.2	0.753
Farm share of population, 2000	0.0341	0.0416	-22.1	0.047**	0.0336	1.5	0.882	0.0399	-17.0	0.067*
Black share of population, 2000	0.2590	0.2118	21.9	0.132	0.2301	13.4	0.338	0.1995	27.5	0.039**

—continued

Table A-2

Comparison of characteristics of matched Delta Regional Authority and comparison samples[1]—Continued

Variable	Mean treated	PSM-NN without replacement: Control	Percent bias	p>|t|	PSM-KM: Control	Percent bias	p>|t|	Mahalanobis: Control	Percent bias	p>|t|
Share of population age 17 or less, 2000	0.2546	0.2484	23.5	0.097*	0.2523	8.5	0.547	0.2525	7.7	0.584
Share of population age 65 or more, 2000	0.1504	0.1549	-16.9	0.210	0.1531	-10.0	0.464	0.1485	7.4	0.524
Share of adults with more than a high school education, 2000	0.3370	0.3343	3.7	0.798	0.3383	-1.8	0.897	0.3389	-2.8	0.836
Share of men working full time all year, 2000	0.5764	0.5870	-18.3	0.198	0.5827	-10.9	0.444	0.6112	-60.3	0.000***
Share of women working full time all year, 2000	0.3915	0.4044	-35.8	0.010***	0.3922	-2.1	0.886	0.4137	-61.7	0.000***
		Pseudo R^2	LR chi^2	$p>chi^2$	Pseudo R^2	LR chi^2	$p>chi^2$	Pseudo R^2	LR chi^2	$p>chi^2$
Overall balance tests – matched samples		0.097	27.65	0.589	0.047	13.33	0.996	0.218	62.29	0.000***

*, **, *** Difference statistically significant at 10-percent, 5-percent, and 1-percent levels, respectively.

[1]Using PSM-NN matching without replacement, PSM-KM and Mahalanobis nearest neighbor matching. PSM-NN=propensity score nearest neighbor matching; PSM-KM = propensity score kernel matching (see text for explanations of these matching methods).

Source: USDA, Economic Research Service analysis.

Changes in personal income per capita for the DRA-recipient counties (with common support) and the matched non-DRA nonmetro counties are shown in figures A-2 and A-3 (using PSM-NN without replacement). No strong geographical pattern of changes in personal income is evident for either group. Comparing the cumulative distribution of changes in per capita personal income for DRA-recipient and non-DRA counties indicates that the distribution of changes in income per capita of DRA-recipient counties stochastically dominates that of matched non-DRA counties (fig. A-4). Thus, it is evident that the mean difference in income growth per capita is not driven by outliers in these distributions.

Among the major components of personal income (net earnings; dividends, interest, and rent; and transfer payments), transfer payments grew statistically significantly more rapidly in the DRA counties, using four of the six estimators. The difference in growth in transfer payments according to the PSM-NN estimator with replacement was not significant, in part because the standard errors tend to be larger for this estimator, as discussed earlier. For all major income components, the predicted sign of the difference was positive (i.e., greater growth in DRA counties), although the differences were not statistically significant except for transfer payments.

Among the different types of transfer payments, the difference between DRA counties and matched non-DRA counties was largest and most robust for medical transfer payments. The estimated mean differences in growth in medical transfer payments were positive and statistically significant for all estimators except PSM-NN with replacement. Growth in income mainte-

Table A-3

Mean changes in outcomes, Delta Regional Authority minus matching counties 2002-07 (using DD estimator, standard errors in parentheses)

Dependent variable	PSM-NN with replacement: Not bias corrected	PSM-NN with replacement: Bias corrected	PSM-NN without replacement: Not bias corrected	PSM-KM with replacement: Not bias corrected	MM-NN with replacement: Not bias corrected	MM-NN with replacement: Bias corrected
Personal income per capita	604.7*	539.8	660.3***	597.0*	498.9*	619.0
	(331.2)	(386.4)	(248.4)	(333.5)	(281.5)	(448.7)
Major components of personal income						
Net earnings per capita	278.5	225.5	324.7	240.2	192.9	175.9
	(283.1)	(316.4)	(249.9)	(229.1)	(210.6)	(329.0)
Dividends, interest and rent per capita	170.8	213.0	166.0	164.6	76.6	82.5
	(176.9)	(142.0)	(108.8)	(127.9)	(130.6)	(173.9)
Personal transfer payments per capita	155.3	101.0	169.5**	192.1**	229.4***	360.5***
	(120.7)	(85.4)	(71.1)	(81.5)	(68.3)	(93.7)
Employment per capita	-0.0022	-0.0060	-0.0024	-0.0028	0.0020	-0.0039
	(0.0072)	(0.0064)	(0.0052)	(0.0053)	(0.0055)	(0.0075)
Transfer payments by type						
Retirement and disability	9.3	25.7	20.0	16.4	38.9*	78.4**
	(33.4)	(26.3)	(24.8)	(26.8)	(22.4)	(32.2)
Medical	93.0	69.6	111.3**	120.5**	116.5**	258.5***
	(77.4)	(61.9)	(45.6)	(59.2)	(52.3)	(71.3)
Income maintenance	37.6*	23.7	25.8*	35.3**	47.0***	18.2
	(20.6)	(18.1)	(13.6)	(16.0)	(16.2)	(13.0)
Unemployment insurance	7.5	-34.2	2.7	6.3	14.2	-9.1
	(14.6)	(24.1)	(7.2)	(10.1)	(8.7)	(10.9)
Veterans benefits	5.3	7.0	6.1	9.6	13.7**	12.7
	(10.7)	(7.7)	(5.3)	(6.4)	(6.9)	(8.4)
Federal education and training assistance	5.4	8.5	1.1	2.1	0.9	-1.9
	(11.5)	(17.0)	(6.0)	(8.1)	(6.6)	(7.7)
Population	-253.3	-7.6	-449.9**	-447.5	-548.2***	-590.4**
	(406.1)	(431.0)	(213.0)	(543.0)	(208.9)	(243.5)

, **, *** Difference statistically significant at 10-percent, 5-percent, and 1-percent levels, respectively.

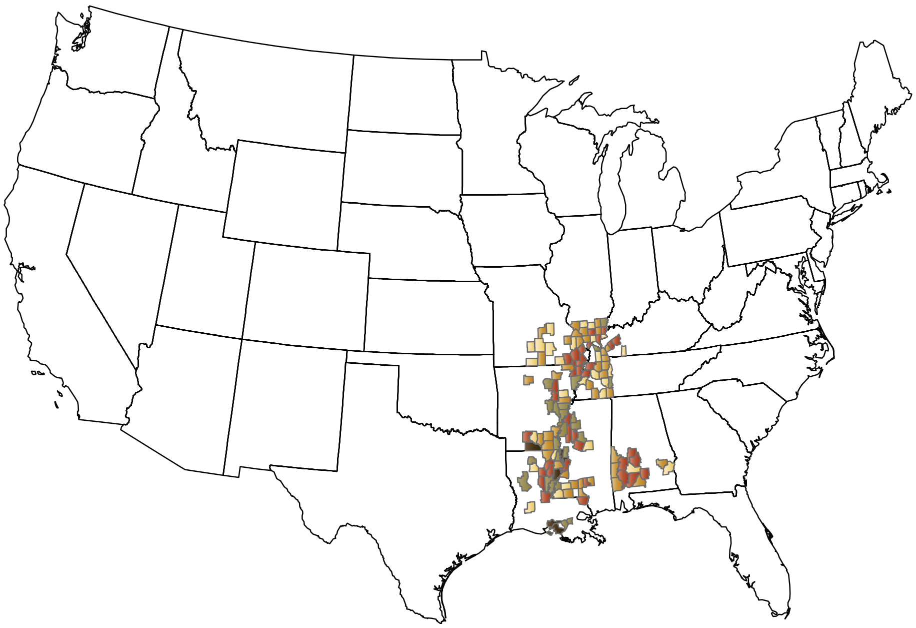
DD estimator= difference-in-difference estimator. PSM-NN=propensity score nearest neighbor matching ; PSM-KM = propensity score kernel matching ; MM-NN = Mahalanobis metric matching.

Source: USDA, Economic Research Service analysis.

nance program payments was greater in DRA counties according to most of the estimators.

Population growth was less (or population decline was greater) in DRA counties according to some estimators (PSM-NN without replacement, MM-NN with and without bias correction). There was no statistically significant difference between DRA counties and matched non-DRA counties in the change in the share of the population that is elderly, according to any of the estimators. Hence, the changes in population growth or difference in growth of Medicare transfer payments in DRA counties do not appear to be driven by differences in growth of the elderly population. We find more growth in the share of the population that is African American in DRA counties using two of the estimators (PSM-NN without replacement in MM-NN without bias correc-

Figure A-2
Change in annual per capita personal income in DRA-recipient nonmetro counties, 2002-07



Change in per capita personal income
Dollars
717 - 3,853
3,854 - 5,109
5,110 - 6,365
6,366 - 8,161
8,162 - 13,715

DRA=Delta Regional Authority. The DRA operates in Louisiana, Alabama, Mississippi, Arkansas, Tennessee, Missouri, Kentucky, and Illinois.
For a list of the 252 DRA-eligible counties, see http://www.dra.gov/about/maps.aspx/.

Source: USDA, Economic Research Service analysis, 2010.

tion). This could be related to the greater decline in population observed in DRA counties using those same estimators (i.e., greater decline in the White population), and could be related to differences in growth in income maintenance payments per capita, to the extent that African Americans are poorer and more likely to use such programs in the region studied. These are not necessarily effects of the DRA, however, although these tendencies are more apparent in DRA-recipient counties.

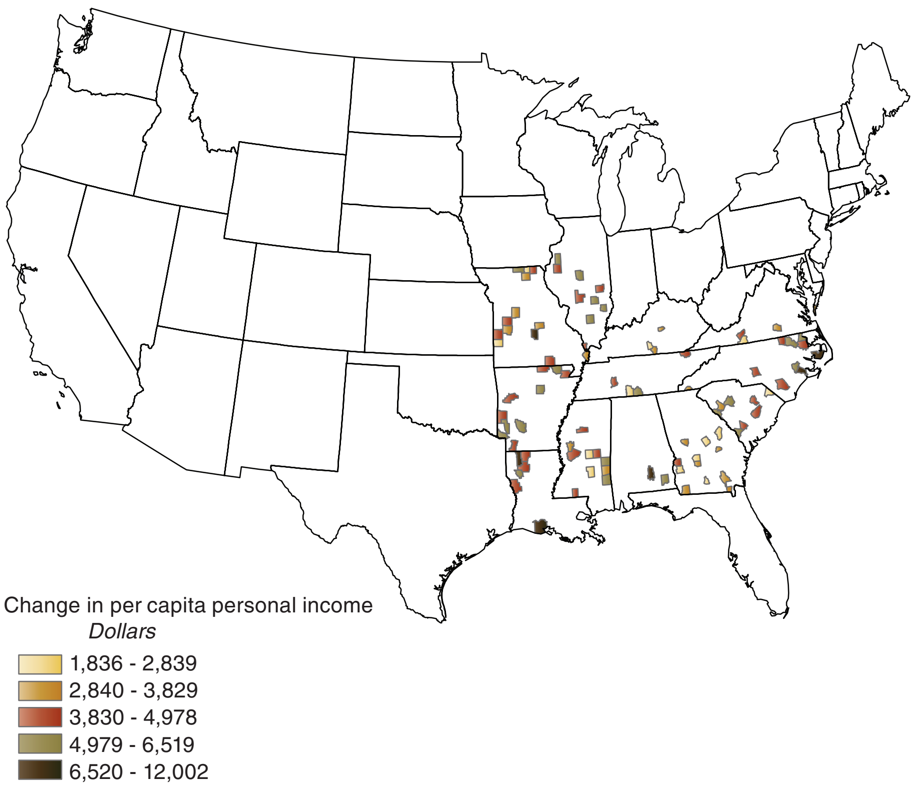
Pre-DRA Differences in Outcome Trends

Table A-4 provides estimates of the differences between DRA-recipient counties and matching non-DRA counties in their pre-2002 outcome trends. For most outcome variables and most matching estimators, there were not statistically significant differences in these pre-2002 outcome trends. Here we comment on outcome variables for which there was a significant difference using at least one of the matching estimators.

Pre-2002 growth in per capita personal income, net earnings, transfer payments, employment, and medical transfer payments was more rapid in the DRA-recipient counties than matched non-DRA counties, according to the

Figure A-3

Change in annual per capita personal income in matching non-DRA counties, 2002-07

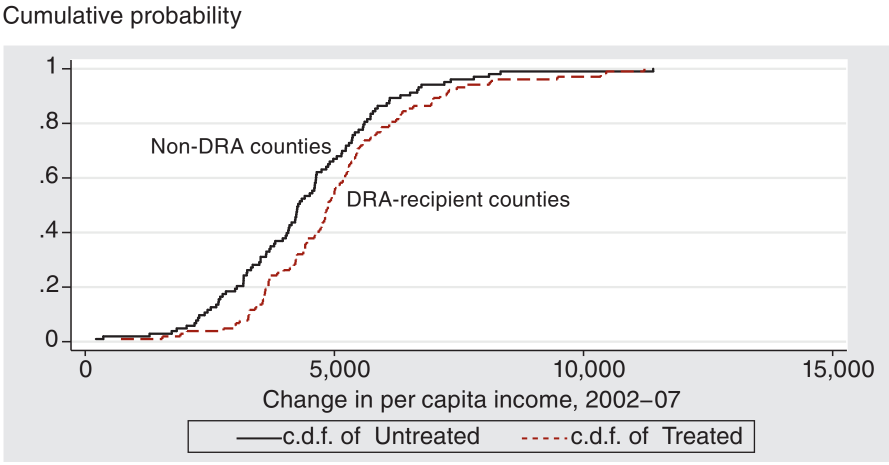


DRA=Delta Regional Authority. The DRA operates in Louisiana, Alabama, Mississippi, Arkansas, Tennessee, Missouri, Kentucky, and Illinois.
For a list of the 252 DRA-eligible counties, see http://www.dra.gov/about/maps.aspx/.

Source: USDA, Economic Research Service analysis, 2010.

Figure A-4

Cumulative density functions of change in personal income per capita, 2002-07[1]



[1]Matched DRA-recipient counties and non-DRA counties, using PSM-NN without replacement.
PSM-NN without replacement = Propensity score nearest neighbor matching.
DRA = Delta Regional Authority.

Source: USDA, Economic Research Service analysis.

Table A-4

Mean changes in outcomes, Delta Regional Authority minus matching counties, 2000-02

Dependent variable	PSM-NN with replacement		PSM-NN without replacement	PSM-KM with replacement	MM-NN with replacement	
	Not bias corrected	Bias corrected	Not bias corrected	Not bias corrected	Not bias corrected	Bias corrected
Personal income per capita	2.2 (195.9)	219.3 (270.4)	243.7 (152.8)	87.1 (138.1)	506.0*** (169.6)	-53.6 (244.7)
Major components of personal income						
Net earnings per capita	84.5 (171.9)	220.8 (218.9)	182.8 (121.0)	123.8 (141.8)	404.5*** (151.4)	22.6 (204.2)
Dividends, interest and rent per capita	-55.6 (56.4)	-34.0 (53.3)	6.8 (35.4)	-33.2 (33.6)	14.9 (47.2)	-40.1 (65.4)
Personal transfer payments per capita	-26.6 (56.6)	32.6 (50.5)	54.2 (47.8)	-3.3 (41.0)	86.8** (36.2)	-35.9 (43.5)
Employment per capita	0.0035 (0.0048)	0.0021 (0.0060)	0.0035 (0.0039)	0.0036 (0.0043)	0.0098** (0.0044)	-0.0052 (0.0058)
Transfer payments by type						
Retirement and disability	-17.7 (17.4)	-17.1 (14.7)	7.2 (9.3)	-8.4 (15.4)	-1.5 (12.3)	-1.7 (14.3)
Medical	-7.4 (40.6)	11.3 (38.9)	34.6 (36.2)	-2.4 (35.7)	73.7** (33.2)	-43.6 (35.7)
Income maintenance	16.3* (9.8)	13.1** (6.4)	23.9*** (5.3)	19.9*** (7.1)	27.9*** (5.8)	17.6*** (5.3)
Unemployment insurance	-17.7 (13.1)	14.0 (18.1)	-14.7* (7.9)	-12.4 (8.4)	-17.1* (8.8)	-1.9 (10.6)
Veterans benefits	-1.5 (2.7)	-2.6 (2.6)	-1.6 (1.9)	-2.4 (2.6)	-0.4 (2.4)	0.3 (3.5)
Federal education and training assistance	-3.7 (9.5)	9.1 (8.6)	-1.4 (4.8)	-1.3 (6.2)	-2.5 (6.3)	-6.8 (6.9)
Population	-95.2 (129.5)	-46.6 (141.7)	-205.2*** (74.9)	-132.2 (88.9)	-260.9*** (79.8)	-76.0 (92.0)

*, **, *** Difference statistically significant at 10-percent, 5-percent, and 1-percent levels, respectively.

PSM-NN=propensity score nearest neighbor matching ; PSM-KM=propensity score kernel matching ; MM-NN=Mahalanobis metric matching.

Source: USDA, Economic Research Service analysis.

uncorrected MM estimator. However, none of these differences was significant using any of the other estimators. Given the large biases noted earlier for the MM estimator and the lack of robustness of these results, these results are not substantial evidence of a difference in these trends prior to 2002.

Growth in income assistance program payments was significantly greater in DRA-recipient counties using all matching estimators. Unemployment insurance payments grew less rapidly in DRA-recipient counties (weakly significant) according to two of the estimators (PSM-NN without replacement and MM-uncorrected).

Population grew less rapidly (or declined more rapidly) in DRA-recipient counties from 2000 to 2002 according to the same two estimators (PSM-NN without replacement and MM—uncorrected).

These results indicate that there may have been differences between DRA-recipient counties and matched non-DRA counties in outcome trends prior to implementation of the DRA for some outcome variables. However, few of these differences are robust to the choice of estimator, with many of these seen only with the uncorrected MM estimator or the PSM-NN estimator without replacement, both of which were more biased than other estimators.

Appendix B—Regression Model To Estimate Impacts of DRA

One important drawback of quasi-experimental methods is that they only estimate mean differences in outcomes between program participants and nonparticipants, as if all participants received the same program funding. Presumably, the impacts of a program are likely to be larger for participants that received more funding. We investigated this issue using switching regression models for matched DRA and non-DRA counties, which also address the bias caused by imperfect matching and allow for heterogeneous impacts of DRA spending depending on the levels of other covariates.[41] The matching counties used in the switching regressions were based on the PSM-NN model without replacement.

[41]The switching regression model was also used to test for the significance of such heterogeneous impacts, using a Chow test for differences in coefficients of the covariates in the regressions for DRA vs. non-DRA counties (Crump et al., 2008). In almost all cases, this test strongly rejected the null hypothesis of homogeneous impacts; so the heterogeneous switching regression model was used.

The switching regression models have the following form:

(1) $\Delta Y_{Ti} = \alpha_T + \beta_T (P_{Ti} - \mu_p) + \gamma_T (X_{Ti} - \mu_x) + \varepsilon_{Ti}$ for program participants (T), and

(2) $\Delta Y_{Cj} = \alpha_C + \gamma_C (X_{Cj} - \mu_x) + \varepsilon_{Cj}$ for nonparticipants (C).

Note that equations (1) and (2) do not specify that the changes in outcomes (ΔY) are functions of the changes in covariates (ΔX), as in Wooldridge (2002, p. 284), but rather as functions of preprogram values of the covariates (X). The reason for this specification is the endogeneity of ΔX; i.e., changes in values of covariates, such as changes in population and in the economic and demographic structure of the counties studied, could be affected by the DRA program, potentially biasing the estimation results. Furthermore, ΔX is not observed for all relevant covariates, many of which are observed only during decennial census years. Equations (1) and (2) represent a reduced form specification in which the ΔX are derived as linear functions of their preprogram values X and the effects of the program (i.e., ΔX = f(X, P)), and these linear functions substituted into the structural linear model of ΔY (ΔY=g(ΔX,P)=g(f(X,P),P)=h(X,P)). It is not possible to identify the parameters of the structural model g(ΔX,P) (and in particular the structural model impact dg/dp) based on estimation of h(X,P) without restrictive assumptions. Nevertheless, the impact of P on ΔY estimated using h(X,P) (i.e., dh/dP) is of interest in its own right, as the impact of the program conditional on initial conditions (but not conditional on the contemporaneous values of the covariates). Our specification of equations (1) and (2) is similar to the form specified by Abadie (2005, equation (8)).

The switching regression is implemented using the following pooled regression of all matched observations:

(3) $\Delta Y_i = \alpha_C + (\alpha_T - \alpha_C)\, DRA_i + \beta_T DRA_i (P_{Ti} - \mu_p) + \gamma_C (X_i - \mu_x) + (\gamma_T - \gamma_C)\, DRA_i (X_i - \mu_x) + \varepsilon_{Ci} + DRA_i (\varepsilon_{Ti} - \varepsilon_{Ci})$, where $DRA_i = 1$ for DRA-recipient counties and = 0 for matched non-DRA counties. The coefficients of the interactions between DRA_i and $X_i - \mu_x$ (which equal $\gamma_T - \gamma_C$) can be interpreted as measuring the effect of variations in X_i on the impact of the DRA (i.e., program effect heterogeneity). This form of the pooled switching regression model is

similar to that given in Wooldridge (2002), p. 613 (equation 18.16); except that we include the demeaned variable for the level of program spending.

ΔY_{Ti} is the change in per capita outcome Y from before to during the program for program participant i (i.e., $Y_{T1i} - Y_{T0i}$, using the notation for periods used earlier); ΔY_{Cj} is the change in per capita outcome Y from before to during the program for program nonparticipant j; P_{Ti} is the level of program investment per capita during the program period for program participant i; μ_p is the mean level of program investment per capita in the population of treated units; X_{Ti} is a vector of preprogram characteristics of program participant i that influence ΔY_{Ti}; X_{Cj} is a vector of preprogram characteristics of program nonparticipant j that influence ΔY_{Cj}; μ_x is the mean of X in the matched populations; α_T, α_C, β_T, γ_T, and γ_C are parameters to be estimated; and ε_{Ti} and ε_{Cj} are error terms with $E(\varepsilon_{Ti})=0$ and $E(\varepsilon_{Cj})=0$. Although linear functional form restrictions are imposed in this model (unlike the simple DD-matching estimator model, which imposes no restrictions on the relationship between ΔY and X), these regression functions allow for heterogeneous impacts of the covariates X on outcomes (i.e., γ_T and γ_C are not necessarily equal). Subtracting the means of P and X ensures that the difference between the intercept terms in regressions (1) and (2) ($\alpha_T - \alpha_C$) estimates the average treatment effect of the program (Wooldridge, 2002, p. 613).[42] In estimating these regressions, the population means of P and X are replaced by the sample means.[43]

Partial results of the switching regressions for various outcome variables (showing only the estimated values of α_T-α_C and β_T) are reported in table 4. The full results of the switching regression model for changes in personal income per capita are reported in table B-1. These results show that the DRA had a positive average effect on income growth and that greater DRA spending was associated with larger impacts on income growth. They also show that some of the covariates associated with industrial structure – including the shares of employment in manufacturing, retail trade and public administration – were associated with heterogeneous impacts of the DRA. In particular, the higher the share of employment in each of these sectors, the smaller the impact of the DRA. This suggests that DRA actions and investments were less beneficial to these sectors than to other industrial sectors.

[42]The population average treatment effect (ATE) is not the same as what is estimated by the matching - DD models, which were used to estimate the average effect of the treatment on the treated (ATT). However, since the switching regression models were run for matched samples, the ATE and ATT are likely to be similar. Estimation of the ATT using switching regression models requires additional calculations (Wooldridge, 2002).

[43]Formally, the use of sample means rather than population means affects the standard errors of the estimates, although this typically has a minor effect on the estimated standard errors (Wooldridge, 2002, p. 613). We do not correct our standard errors for this additional source of error.

Table B-1
Full switching regression results for change in personal income per capita

Variable	Non-DRA counties Coefficient	Non-DRA counties Standard error	DRA counties Coefficient	DRA counties Standard error
Mean change (α)	4,373.8***	124.3	4,885.9***	122.5
DRA spending per capita ($)	NA	NA	15.324**	6.337
Per capita income in 2000 ($)	0.378***	0.118	0.121	0.108
Population in 2000	0.004	0.010	0.021	0.019
Poverty rate in 2000 (percent)	-70.34	88.61	110.90	89.00
Share of income from transfer payments in 2001	5,537.5	7,773.0	-10,530.2	6,922.9
Share of income from dividends, int, rent in 2001	-11,437.3	7,137.9	1,241.9	7,733.2
Share of adults employed in agriculture, forestry, fishing, or hunting in 2000	9,530.5	11,246.1	-5,161.5	8,840.7
Share of adults employed in construction in 2000	6,116.0	7,881.3	-4,880.4	9,227.5
Share of adults employed in manufacturing in 2000	4,727.0	3,813.1	-10,386.8***	3,700.5
Share of adults employed in retail trade in 2000	12,519.5	9,740.5	-34,266.0***	11,579.6
Share of adults employed in public admin in 2000	14,537.8	9,595.0	-19,031.2**	9,080.7
Share of adults employed in education in 2000	2356.1	7,674.4	-3,364.6	7,833.6
Share of adults employed in health & social services in 2000	-10,654.4	7,838.2	-10,372.8	7,718.2
Federal economic development grants per capita in 2000 ($)	-0.074	0.312	0.379	0.388
Gulf Opportunity Zone counties	-744.8	506.0	497.2	432.7
Cotton harvested area per capita in 2002	-533.8***	173.5	235.2*	137.9
Rice harvested area per capita in 2002	-2175.5	2,524.4	139.8	1,132.5
Distance to nearest city of 25,000 or more (miles)	-15.474***	5.628	-6.306	5.586
Distance to nearest city of 100,000 or more (miles)	5.640*	2.985	-0.936	3.091
Distance to nearest city of 250,000 or more (miles)	-0.153	1.765	2.091	1.951
Distance to nearest city of 500,000 or more (miles)	0.019	1.334	0.976	1.309
Distance to nearest city of 1,000,000 or more (miles)	0.727	0.770	-0.938	0.931
Population density in 1990 (persons/square mile)	-6.823	10.589	-0.134	7.408
Rural share of population in 2000	1,753.8*	968.5	-791.9	956.3
Farm share of population in 2000	1,972.8	6,366.7	-13,806.4	8,952.9
Black share of population in 2000	1,508.7	1,283.2	-1,766.2	1637.1
Share of population age 17 or less in 2000	12,354.3	9441.5	11,152.0	8,820.0
Share of population age 65 or more in 2000	-13,076.5	11,511.5	11,713.6	13,405.8
Share of adults with more than high school in 2000	16,313.8***	3,607.4	-3,611.2	3,703.0
Share of men working full time all year in 2000	-1,333.0	4,467.4	-937.6	5,113.2
Share of women working full time all year in 2000	-15,026.7***	5,587.6	-3,355.4	5,983.3

*, **, *** Coefficient statistically significant at 10-percent, 5-percent, and 1-percent levels, respectively.

Source: USDA, Economic Research Service analysis.

Appendix C—Data sources

The data on personal income and employment and their components and on population by county were taken from the Regional Economic Information System (REIS) of the U.S. Department of Commerce, Bureau of Economic Analysis (BEA) (http://www.bea.gov/regional/reis/). The estimates of personal income and employment are based on administrative records, censuses, and surveys, and are designed to be consistent with State and national levels of personal income reported the National Income and Product Accounts.[44] For total personal income and employment and major components of personal income and employment, the data are available by county from 1969 to 2007. For earnings and employment by industry, the data are only available from 2001 to 2007.

The data on poverty rate and demographic and education characteristics of counties in 2000 and employment conditions in 1999 were taken from the 2000 Census of Population (http://www.census.gov/main/www/cen2000.html/). The data on areas of cotton and rice harvested in 2002 were taken from the 2002 Census of Agriculture (http://www.agcensus.usda.gov/Publications/2002/).

The data on distances to urban centers of different sizes and population density were provided by Peter Stenberg, and were based on geographic information systems analysis conducted by researchers of the Economic Research Service as part of a study of broadband Internet in rural areas (Stenberg et al., 2009).

The data on economic development grant spending in 2000 and 2001 were taken from the Consolidated Federal Funds Report (CFFR) (http://www.census.gov/govs/cffr/). Classification of specific Federal programs as rural economic development programs used the classification developed by the U.S. Government Accountability Office (GAO) in a report on Federal rural economic development programs (U.S. GAO, 2006). This GAO report notes several problems with the CFFR data, but this is the only comprehensive source available for these programs.

The data on DRA spending by county were taken from the DRA's Federal Grant Program Profile (DRA, 2009), which lists all DRA projects funded from 2002 to 2008 by year, project name, location and approved funding amount. Since the approved amounts of funding may not be spent in the same year that approval occurred, the amount of funds actually spent in each county during 2002 to 2007 may be less than amounts approved during this period. Despite this, these data were judged to be more reliable than the amounts reported as DRA outlays in the CFFR.

The list of GO Zone counties is taken from the Gulf Opportunity Zone Act of 2005.

[44]See http://www.bea.gov/regional/pdf/lapi2007/lapi2007.pdf/ for details on the methodology used to produce the local area personal income and employment estimates.

Appendix D—Sensitivity Analysis

Scenarios

We investigated the sensitivity of our matching and regression results to a series of alternative scenarios considering hypotheses about factors that may have confounded our results. The factors investigated included the following:[45]

- *Losses from Hurricane Katrina.* Counties that experienced large losses from Hurricane Katrina may have suffered unusually large declines in population and income. On the other hand, insurance payments and Government programs intended to promote recovery after Hurricane Katrina may have led to higher income per capita in some affected counties, at least temporarily. As noted in the text, we sought to control for these concerns by including whether a county was in the Gulf Opportunity Zone (GO Zone) as one of the covariates in the matching estimation. However, this may not adequately reflect differences in damages and payments resulting from Hurricane Katrina. To address this, we used estimates of asset loss ratios (the ratio of losses to asset values) from the Federal Emergency Management Agency's (FEMA) HAZUS model (http://www.fema.gov/plan/prevent/hazus/). In two scenarios, we dropped counties that suffered loss ratios greater than 1 percent or greater than 0.5 percent.

- *Drop counties from States other than the DRA States.* It may be that counties in States outside of the DRA States (GA, NC, and SC) are different from DRA counties in important unmeasured ways that could influence the differences in outcomes. These other States are different from the DRA States in some ways in their history and culture, although there are many commonalities as part of the Southern United States. Furthermore, the use of the double-difference estimator seeks to subtract out the effects of such fixed or slowly changing factors, and dropping these States inhibits the ability to find counties that are good matches in terms of the observed covariates. Nevertheless, we investigate the robustness of our results to dropping counties in these other States from the analysis.

- *Keep DRA counties that have not received DRA grants as possible matches.* Some of these nearby counties may be the best matches for DRA-recipient counties in terms of the observed covariates. These counties were dropped from the baseline analysis because of the concern that unobserved factors likely account for why these counties did not receive DRA grant funding (e.g., they may have less effective leadership), and these unobserved factors could be associated with differences in outcomes (i.e., the problem of "selection on unobservables" is likely to be greater when comparing eligible recipients and nonrecipients). These counties also may be more likely to be affected by spillover effects (either positive or negative) of the DRA program investments and activities. Nevertheless, we investigate the robustness of our findings to including these counties as possible controls.

[45]We are grateful to two reviewers of this paper—Mark Partridge and Matt Fannin—for suggesting several of these sensitivity analyses. We are also grateful to Beau Beaulieu and other seminar participants at Mississippi State University for suggesting investigating the effects of the Delta Health Alliance on the results.

- *Drop counties that are in the Delta Health Alliance.* The Delta Health Alliance (DHA) is a nonprofit organization established in 2004 to improve the health of people in 18 counties in the Yazoo-Mississippi Delta region of the State of Mississippi (http://www.deltahealthalliance.com/). Although it was established more recently and focuses on a fairly small subset of the DRA counties, the activities of this organization could have influenced outcomes in the DRA region compared to non-DRA counties, especially those related to health sector services. Thus, we investigate how dropping these counties from our analysis affects the results.

- *Drop counties that are part of a Federal Empowerment Zone (EZ), Enterprise Community (EC), or Renewal Community (RC).* Several rural EZs, ECs and RCs have been established in rural parts of the Mississippi Delta region. The impacts of these programs therefore could be confounded with the impacts of the DRA in our analysis. Hence, we investigate how dropping counties that are part of an EZ, EC or RC affects our results.

- *Include as covariates whether the county or a neighboring county had a Critical Access Hospital in 2002 and the change in this status between 2002 and 2007.* The Critical Access Hospital (CAH) program was launched in 1999 to assist rural hospitals, many of which closed in the 1980s and 1990s due to rising costs and payment restrictions imposed by Medicare's prospective payment system, which paid less to rural hospitals than urban ones (McNamara, 2009). The program allowed small rural hospitals (25 beds or less) in remote rural areas to become CAHs, which allowed them to receive cost-based reimbursement from Medicare. Rapid expansion of the program occurred from 1999 through 2005, and various studies have shown that cost-based reimbursement significantly improved the financial situation of rural hospitals, in many cases enabling increased investments by the CAHs in staff, training and equipment (*Ibid.*). If DRA-recipient counties or their neighbor counties experienced more growth in CAH facilities than matched non-DRA counties during our study period, the impacts of the CAH program could be confounded with the impacts of the DRA, especially impacts on medical transfer payments and earnings in the health sector. To address this concern, we include as covariates in one scenario variables representing (1) the presence of a CAH in the county in 2002, (2) the presence of a CAH in a neighboring county in 2002, (3) change in the presence of a CAH in the county between 2002 and 2007, and (4) change in the presence of a CAH in a neighboring county between 2002 and 2007. The data on CAHs were taken from the CAH Flex Monitoring Team's website: http://www.flex-monitoring.org/cahlistRA.cgi/.

- *Include as covariates the number of Federally Qualified Health Centers in the county in 2002 and the change in number from 2002 to 2007.* Federally Qualified Health Centers (FQHCs) are located in medically underserved areas and provide patients access to care regardless of their insurance status or ability to pay. Like CAHs, FQHCs receive cost-based reimbursement from Medicare. As with CAHs, if the number of FQHCs grew more rapidly in DRA-recipient counties than in matched non-DRA counties, this may have affected our results. Hence, we include the number of FQHCs in each county in 2002 and the change the number from 2002 to 2007 as covariates in one scenario (the same scenario in which we include indicators for CAHs). The data on FQHCs was taken from the Area Resource File maintained by the U.S. Department of Health and Human Services (http://arf.hrsa.gov/purchase.htm/).

- *Include medical transfer payments per capita or total transfer payments per capita in 2002 as a covariate.* Given the robust finding that medical transfer payments and total transfer payments per capita grew more rapidly in DRA-recipient counties than in matched non-DRA counties, we include the initial levels of these variables in two separate scenarios.
- *Include lagged population growth as a covariate.* In one scenario, we include the ratio of county population in 2000 to county population in 1990 as a covariate to account for possible persistent effects of differences in population growth rates. Given evidence that there are differences in prior trends in population levels and growth in the matched samples in our baseline analysis (see table 3 and figure 4), this scenario helps to address possible biases caused by these differences.
- *Use different starting and ending years in the analysis.* To see whether our results may be an artifact of the particular starting or ending year, we investigate impacts using alternative starting and ending years.

Results

The results of the sensitivity analysis for estimated impacts on per capita personal income and transfer payments are shown in table D-1.[46] The results are quite robust to almost all of the scenarios, especially the regression results. The matching results are not statistically significant in some scenarios, although the magnitude of the estimated average treatment effects are in a similar range ($350 to $710 for personal income per capita, $160 to $240 per for transfer payments per capita) for all but one scenario—the scenario in which counties outside of the DRA States are dropped. For that scenario, the estimated average treatment effect is much smaller and statistically insignificant for both personal income and transfer payments per capita, using both the matching estimator and the regression estimator. However, the estimated marginal impacts of DRA spending are still statistically significant and fairly similar in magnitude ($12.35 increased growth in personal income and $5.51 increased growth in transfer payments per $1 spent by DRA) to the baseline analysis for this scenario.

[46]Results for other outcome variables are available upon request.

One possible explanation for why the estimated average treatment effect is smaller and not statistically significant for this scenario is that this results from poorer matching results when dropping counties in GA, NC, and SC from the analysis. For example, the pseudo-R^2 of the probit model distinguishing the two matched groups was 0.047 in the baseline analysis (see table A-2) but increases to 0.079 in this scenario, indicating poorer matching. A second possible explanation is that the effects of the DRA spilled over to non-DRA counties within the DRA States, so that the impacts of the DRA cannot be measured by comparing counties within the same States. Such spillovers could result from the ability of non-DRA counties to access the benefits of investments made in DRA counties, or from displacement effects whereby DRA States shift development funds to non-DRA counties in response to the availability of additional funds for DRA counties. A third possibility is that the DRA truly had no mean impact. However, it is difficult to understand how the DRA could have a positive marginal impact of funds spent, as we still find in this scenario, but a zero mean impact. Furthermore, this would not explain why a positive mean impact is found for the DRA in so many alternative scenarios, which rule out many possible alternative explanations for the findings in the baseline analysis.

Although these results are not fully conclusive, the preponderance of the evidence supports the conclusion that the DRA has had a positive impact on personal income and transfer payments per capita. The positive marginal impacts of DRA spending are particularly robust.

Table D-1
Selected results under alternative matching scenarios

Scenario	Total no. of obs.	No. of matched DRA counties	Change in per capita personal income			Change in per capita transfer payments		
			PSM-KM	Switching regression		PSM-KM	Switching regression	
				α_T-α_C	β_T		α_T-α_C	β_T
Baseline	461	103	597.0* (333.5)	512.1*** (174.5)	15.32*** (6.34)	192.1** (81.5)	166.4*** (45.1)	5.12*** (1.34)
Drop counties with loss ratios > 1 percent from Hurricane Katrina	457	100	541.8* (312.3)	519.6*** (176.8)	15.13** (6.48)	195.5** (98.3)	148.4*** (45.9)	5.04*** (1.37)
Drop counties with loss ratios > 0.5 percent from Hurricane Katrina	452	98	429.6 (332.5)	446.2** (176.7)	16.21** (6.67)	162.3* (84.7)	147.8*** (48.1)	4.52*** (1.39)
Drop counties outside of DRA States (i.e., in GA, NC, SC)	319	76	30.8 (369.6)	-124.1 (179.3)	12.35* (7.03)	51.9 (126.3)	34.6 (45.2)	5.51*** (1.44)
Keep DRA counties that are nonrecipients as possible matches	516	121	354.2* (211.7)	271.5* (159.9)	12.34** (5.97)	160.3** (76.0)	186.4*** (42.1)	5.29*** (1.43)
Drop counties in the Delta Health Alliance	450	99	587.3** (272.4)	459.4*** (173.7)	14.69** (6.38)	203.6*** (77.5)	168.4*** (45.8)	4.74*** (1.34)
Drop counties that are part of a Federal Empowerment Zone, Enterprise Community or Renewal Community	415	78	983.9*** (307.7)	682.6*** (183.2)	15.62 (9.86)	222.0** (93.1)	189.6*** (47.0)	5.85*** (1.90)
Include as covariates: i) presence of a Critical Access Hospital (CAH) in 2002 in county or neighboring counties; ii) change in presence of CAH in county or neighboring counties, 2002-07; iii) number of Federally Qualified Health Centers (FQHC) in county in 2002; and iv) change in number of FQHCs in county, 2002-07	461	100	575.0 (473.1)	454.3** (177.7)	16.88** (6.80)	211.4** (91.5)	177.1*** (46.7)	5.48*** (1.41)
Include lagged medical transfer payments as a covariate	461	103	539.1* (319.1)	450.8*** (171.3)	14.89** (6.26)	196.2** (84.1)	156.0*** (44.5)	5.12*** (1.36)
Include lagged total transfer payments as a covariate	461	100	604.0* (361.0)	529.5*** (173.5)	14.76** (6.45)	238.8*** (73.8)	197.2*** (45.9)	4.76*** (1.28)
Include lagged population ratio (2000/1990) as a covariate	461	103	372.4 (374.6)	382.2** (167.7)	14.87** (6.23)	172.2** (82.2)	158.4*** (44.0)	5.01*** (1.29)
Investigate changes from 2002-06 instead of from 2002-07	461	103	487.9** (240.2)	458.0*** (147.7)	11.58** (5.38)	158.1*** (55.0)	139.6*** (34.8)	3.66*** (1.03)
Investigate changes from 2001-07 instead of from 2002-07	461	103	710.6** (282.6)	618.3*** (175.6)	13.62** (6.20)	157.7* (86.6)	126.5*** (46.1)	5.39*** (1.38)

DRA=Delta Regional Authority ; PSM-KM=propensity score kernel matching.

Source: USDA, Economic Research Service analysis.