The Economic Impact of Small Regional Commissions: Evidence from the Delta Regional Authority

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Evidence from the Delta Regional Authority

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Abstract

Factors such as falling U.S. migration rates and diverging regional economic fortunes have heightened interest in place-based policies. Indeed, the U.S. has had many such federal efforts including recently enacted Opportunity Zones. Historically, substantial federal funding has gone to regional economic development programs such as the Tennessee Valley Authority (TVA) and Appalachian Regional Commission (ARC). Yet, little is known about the benefits of some of the smaller place-based programs. We extend the literature on regional commissions by analyzing the economic gains to the Delta Regional Authority (DRA). The DRA was founded in 2000 to provide enhanced development aid to 252 Lower Mississippi Valley counties. Using data over the 1997 to 2016 period, we assess the DRA's impact on employment, income, migration, and poverty. One-to-one propensity score matching is used to generate a set of counterfactual counties. Due to the endogenous nature of the treatment, we instrument for counties being included in the DRA using a dummy for whether the county is within the Lower Mississippi Watershed. The ensuing results reflects an estimation of the intent-to-treat benefits of the DRA. We find that the DRA is associated with income gains and decreases in unemployment; however, no impact on poverty or migration. In sum, the DRA produces economic benefits that greatly exceed its direct costs.

1 Introduction

Factors such as falling U.S. migration rates, diverging regional economic fortunes, and stagnating rural and urban centers heightened interest in place-based policies (Austin, Glaeser, and Summers (2018), Barca, McCann, and Rodridguez (2012), and Partridge and Rickman (2006)). Indeed, the U.S. has had many such place-based federal efforts including recently enacted Opportunity Zones, as well as historic standbys such as the TVA and ARC. With the perceived success of other regional commissions, the late 1990's and early 2000's saw the expansion of these programs across the United States. From Alaska to the Great Plains, the federal government set up regional commissions to provide assistance to some of the country's poorest regions. Despite this, these economic development efforts have recently come under fire.

President Donald Trump's fiscal year 2018 budget proposed billions of dollars of cuts to many federal economic development programs, including the Delta Regional Authority (DRA) and the Appalachian Regional Commission (ARC). Even though this proposal was never enacted by Congress, it does illustrate the continuing concern that such place-based policies are wasteful in which the intended beneficiaries not receiving the bulk of the benefits. Thus, the purpose of this paper is to assess the economic impacts of DRA on counties under its authority.

The DRA is a small, ongoing Federal-State regional commission in the lower Mississippi River Valley. It was originally founded in 2000 with the intent of improving the livelihood of this historically poor region across its 252 counties [1]. The goals of the DRA are to provide funding for basic public infrastructure, transportation infrastructure, business and entrepreneurship development, and workforce development or job training.

[1] It is well known that the Deep South has long been among the poorest regions in the country. For example, Mississippi's 1929 GDP per capita income was approximately 25% of New York state's per capita income according the BEA. Yet, going back to when the first poverty rates were measured in 1959, U.S. Department of Agriculture's Economic Research Service has found that a large number of counties have persistently had poverty rates exceeding 20% for this entire time period, with a large share having rates above 20% for much of this time period.

From fiscal years 2002 to 2015, the DRA received a total of $138 million in federal funding. They claim that this money leveraged another $752 million from other public sources and $2.2 billion from private sources. The DRA has funded approximately 1000 different projects, mostly in the form of grants to private and public organizations. With this, the DRA claim to have created or helped retain 26,218 jobs, provided job training for 7,202 individuals, and positively impacted 65,831 families, well above their original projections (Masingill (2016)). However, as with all such self-assessments, one must view these assertions with some skepticism.

Despite claims of noticeable benefits from the DRA, there has been very little research on the economic benefits of the Delta Regional Authority and just as little on other small regional authorities. Much of the literature focuses on the larger ARC (Brandow, et al. (2000), Glaeser and Gottlieb (2008), and Freshwater et al. (1997)) or the Tennessee Valley Authority (TVA) (Freshwater et al. (1997) and Kline and Moretti (2014)). There are several papers examining the impact on a national scale with the Economic Development Administration (Barrows and Bromley (1975), Martin and Graham (1980), Burchell et al. (1997), and Burchell et al. (1998)). In general, these papers found either no impact on key economic indicators or small benefits. Perhaps the most knwon paper in the area is Isserman and Rephann's (1995) examination of the ARC. They used Mahalanobis Matching to find "twins" of the treated counties. They found significant gains in personal income, per capita income, and transfer payments in the region due to the ARC.

This paper also extends into the literature regarding place-based policies. The effectiveness of these targeted programs has been the center of much debate (Kline (2010), Glaeser and Gottleib (2008), Busso, Gregory, and Kline (2013), Partridge et al., (2013)). Most of the literature focuses on well-funded, urban programs like the Federal Empowerment Zones (Liebschutz (1995), Ham et al., (2011), Reynolds and Rohlin (2015)). The DRA allows for a unique look into place-based policy by examining the effectiveness of a program that is geographically large, but has relatively small funding. We ask the question as to whether such low-funded place-based efforts are worth

the effort besides being little more than a political band-aid to a stagnate region's voters. Indeed, such place-based efforts has the advantage of only being able to target the projects with the highest returns. They also have some ability to broker and build networks with affected governments and stakeholders. However, with such a lack of funding, they may lack the resources to "move the dial."

There is little research on the DRA's effectiveness. There are some papers that explore similar regions such as the Mississippi Delta (Ciscel (1999)). The main paper of note is Pender and Reeder's (2011) assessment of the initial effects of the the DRA, focusing on rural counties. Using propensity score matching with nearest neighbor matching, the authors construct a difference-in-difference estimate to the effects that the DRA produced $15 of personal income growth for every dollar spent, which seems rather optimistic at first consideration.

This paper attempts to extend the previous literature by examining smaller regional commissions that appear to have too few resources to have tangible effects. First is our use of instrumental variable (IV) estimation to correct for the selection bias for the treatment. Next is our use multiple years of pretreatment data versus only a single year to allow for a more accurate assessment of parallel trends. These techniques should result in better estimates of the economic effects of the DRA to appraise its overall effectiveness.

The rest of the paper is as follows. Section 2 describes the institutional features of the DRA. Section 3 will discuss data and empirical methodology followed by section 4's protrayal of the results. Section 5 presents robustness checks, where as Section 6 summarizes the paper's findings and discuss future research needs.

2 Delta Regional Authority

The DRA is a federal-state commission focused on the lower Mississippi River Valley; an area that consists of 10 million residents within 8 states across 252 counties and parishes (Masingill (2016)). When the DRA was created in 2000 (S. 1622), it marked

the end of a 10-year political battle to establish a regional commission in the lower Mississippi Valley. The idea of the DRA can be traced back to 1988 when Senator Dale Bumpers (D-AR) proposed the Lower Mississippi Delta Development Act (S. 2246). The proposed bill offered government assistance to 212 economically distressed counties in the Lower Mississippi Delta region. The goal was to create a similar program as the ARC and TVA. However, his bill failed to pass committee. In 1999, the Delta Regional Authority Act was again proposed. The bill was similar to Bumpers', but its the area was increased to cover 240 counties and parishes and include counties from outside the Delta in Alabama. This expansion was likely done to make it more politically attractive with a larger constituency. President Bill Clinton announced DRA funding in early 2000. Its first programs began operations in 2002. In 2008, the DRA added 10 parishes from Louisiana and two counties in Mississippi, bringing the total counties and parishes to 252. The DRA promotes economic development through various outreach programs.

One of its more prominent projects is expanding access to quality healthcare. The DRA's Delta Doctors program allows foreign physicians trained in the United States to work in medically underserved communities for three years utilizing a J-1 Visa waiver. This create an opportunity for these doctors to work in these remote areas while serving DRA constituents who have little to no access to primary or emergency healthcare. Another DRA initiative is the Community Health Systems Development Program, a collaboration with Health Resources and Human Services and the U.S. Department of Health and Human Services. This program attempts to improve the infrastructure of the local hospitals, clinics, and other healthcare organizations via a technical assistance program. This includes implementing improvements to financial operations, telehealth operations, and providing social services to address challenges faced by patients like child care, housing etc.

One of the DRA's key objectives is improving the local workforce through job training programs across the region. The Re-Imagining Workforce Development program aids communities in planing and building regional development workforce systems. State-run summits strive to bring together leadership, business, and educators to

best address the changing needs of the economy and to fill the gaps in the training. In addition, the DRA runs the Delta Entrepreneurship Network to bring together small business owners to discuss, inform, and provide access to the resources needed to make small businesses thrive and to encourage their start-ups. Regarding such programs to enhance entreprenuership, they may have especially high local economic payoffs as found by Stephens and Partridge (2011) and Stephens et al. (2013).

Through the Delta Leadership Institute, the DRA links community and government leaders to address the area's direst issues. In bringing stakeholders together, the DRA can improve collaboration. The Delta Leadership Institute's Executive Academy provides training, information, and networking to facilitate bringing larger groups to address broader issues. Indeed, regional organizations such as the DRA can play a larger role as a broker that brings together the relevant players in one room. This garners a critical mass to address larger regional issues that one entity could not address alone. Similarly, the DRA can provide capacity to help identify funding sources and grant writing, as well as providing small matching funding to help achieve cost-sharing thresholds for grant applications.

Another set of programs come in the form of direct funding. In its Regional Development Plan III (Masingill et. al, (2016)), the DRA defined three project goals eligible for their direct funding: 1) "advance the productivity and economic competitiveness of the Delta workforce, 2) strengthen the Delta's physical, digital, and capital connections to the global economy, and 3) facilitate local capacity building within Delta communities, organizations, businesses, and individuals." The funding normally comes in the form of grants to any initiative that attempts to address one of these goals and the DRA can occasionally be the funding source of last resort.

3 Methodology

3.1 Instrumental Variable (2SLS)

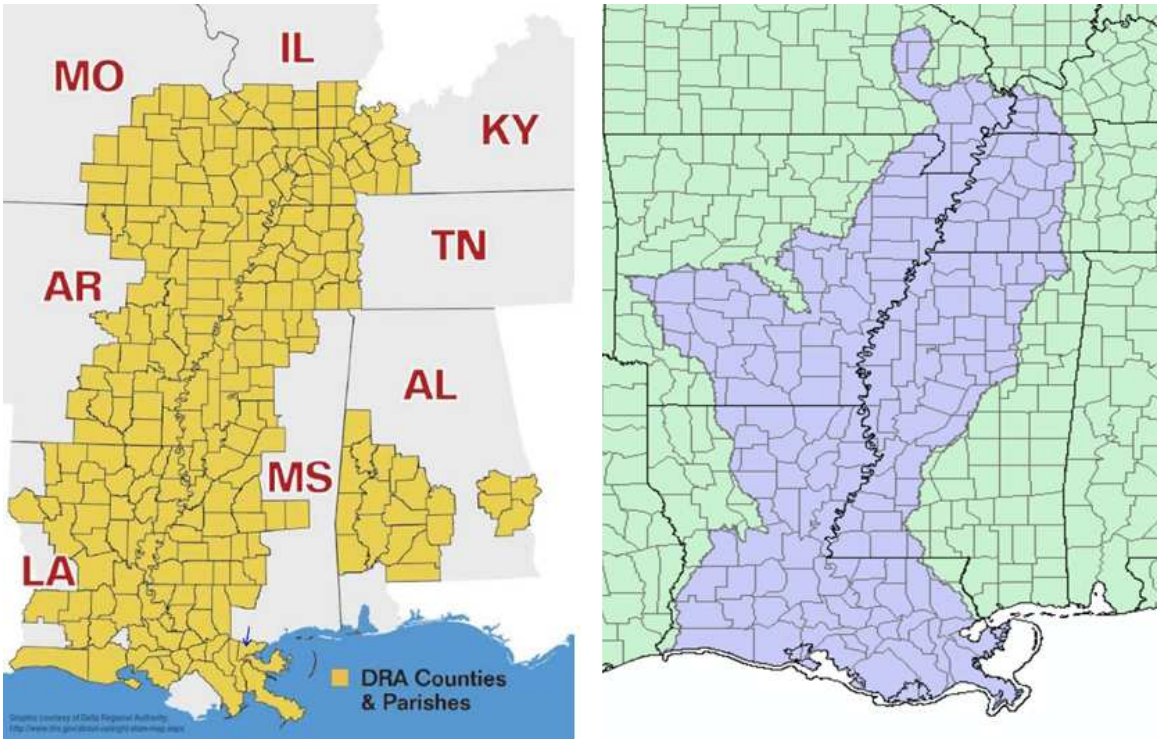
The DRA's geography, unfortunately, creates a selectivity issue for studying its effects because its counties were not randomly assigned. Rather there was an arbitrary cutoff for the program that was decided either due to the areas being sufficiently economically distressed or more due to political motives. Either way, it creates a potential endogeneity problem that requires the use of an instrumental variable to addres treatment selectivity.

Our instrument is an indicator variable for belonging to the Lower Mississippi Watershed. The watershed is a purely geographical designation defined by the U.S. Geological Society (USGS). A watershed is defined as "The divide separating one drainage basin from another and in the past has been generally used to convey this meaning.... Drainage divide, or just divide, is used to denote the boundary between one drainage area and another." (Langvein and Iseri (1995)). In this case, it is the water that flows or drains into the lower Mississippi River. Figure 1 provides a visualization of the instrument. On the left is a map of the counties of the DRA (Masingill (2016)). On the right is a map of the Lower Mississippi Watershed (Ierardi (2016)).

The strucutre of the instrument creates an issue for the difference in difference (DiD) estimate. The DiD estimator will be biased and inconsistent if it is created using one endogenous and one exogenous variable. Falling in line with Bun and Harrison (2014), we use another instrument using the interaction of the post treatment and watershed counties. Bun and Harrison (2014) show that this type of interaction, under standard IV assumptions, creates a consistent and asymptotically normal estimate such that conventional IV inference can be used.

This designation should be purely exogenous to any economic variables because the watershed's boundaries are effectively random to any individual county. They do not determine any state or county boundary, and have no economic effects by themselves, or at least conditional on the economic variables we control for such as agri-

Figure 1: DRA and Watershed



cultural intensity. Yet, due to the intent of the DRA, it should be highly predictive of treatment. The DRA is focused on the historically poorest counties near the lower Mississippi River delta.

However, the instrument may have concerns relating to how well it meets the exclusion restriction. The Lower Mississippi Watershed may have unique factors that will impact its economic outcomes. These can include soil quality, climate, etc. While we do not directly control for these things, the propensity score matched counties still should have similar characteristics. By choosing counties that control demographic, economic, and employment characteristics, it should control for any of these factors. Since both the matched and treated counties have similar characteristics, the watershed should not have unique benefits, meaning the instruments meet the exclusion restriction. In addition, as noted above, our restriction should be exogenous conditional on controlling for many of the variables that are features of the Delta including economic composition and demographics.

The instrument has the potential to act in one of two ways depending on how additional selection and exclusion was done. First, if counties are added or removed due

Table 1: Propensity Score Variables

Variable	Years	Variable	Years
Economic Variables			
% in Poverty	1997-2001	% of Children in Poverty	1997-2001
% Working for Federal Government	1997-2001	% Working for State Government	1997-2001
% Working for Local Government	1997-2001	% Working in Goods-Producing	1997-2001
% Working in Manufacturing	1997-2001	% Working in Natural Resources & Mining	1997-2001
% Working in Construction	1997-2001	% Working in Service-Providing	1997-2001
% Working in Trade, Transportation, & Utilities	1997-2001	% Working in Information	1997-2001
% Working in Professional & Business Services	1997-2001	% Working in Education & Health Services	1997-2001
% Working in Lesiure & Hospitality	1997-2001	% Working in Other	1997-2001
Wage Growth	1996-2001	% Working in Unclassified	1997-2001
ln(Median Income)	1997-2001	Education	2000
Demographic Variables			
% Male and White	1997-2001	% Female and White	1997-2001
% Male and Black	1997-2001	% Female and Black	1997-2001
% Male and Asian and Pacific Islander	1997-2001	% Female and Asian and Pacific Islander	1997-2001
% Male and Native American	1997-2001	% Female and Native American	1997-2001
Population Growth	1996-2001	Rural-Urban Continuum Codes	2003
% Net Migration	2000-2001		

to political reasons rather than poor local economies, it is likely that the 2SLS estimates of the DRA estimates would increase if these counties have healthier economies. However, if additional non-Delta counties were added based on need, using IV would decrease the point DRA estimated effects because it is of these "weaker" counties would bring down the average DRA effect.

3.2 Propensity Score Matching

In policy evaluation, a key challenge is finding a proper counterfactual. Rarely are government programs implemented using random selection with treated and control groups (i.e. Progresa in Mexico. Behrman and Hoddinott (2005) and Schultz (2003)), forcing this leads researchers to create their own counterfactual. For example, in Ham et al. (2011), they used nearby census tracts when looking at the effect of Federal Empowerment Zones. This creates a problem of possible spillovers biasing the results. Other common methodologies include propensity score matching (Rosenbaum and Rubin (1983)) and synthetic controls (Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010)).

The concept of propensity scores has been in existence for decades (Rosenbaum and Rubin (1983, 1984), Hirano, Imbens, and Ridder (2003), etc.). The idea is to find untreated regions that closely match the observational characteristics of the treated

regions. This allows for observation of regions that were similar prior to treatment to help form a credible counterfactual.

Program evaluation of this type used in this study was pioneered by Isserman and Rephann's (1995) evaluation of the ARC. They used a similar methodology called Mahalanobis Matching by considering counties that had similar demographics and economic characteristics to those included in the ARC. They then used mean testing to see how the ARC counties performed relative to their "twins".

In this paper, we use a one-to-one propensity score matching with replacement to find counterfactual counties for those not included in the DRA. Weighting the matches on their frequency should mitigate any biases the smaller sample size may present. The matching algorithm is provided by the "PSMATCH2" user code for Stata (Leuven and Sianesi (2001)) by using a standard binary probit model to generate estimated propensity scores. Many of the control variables are similar to those in Isserman and Rephann (1995). For a full list of the variables, see Table 1.

We also employ multiple pretreatment years to better establish pretreatment trends, rather than one year of pretreatment data, to help the matching algorithm find a better match. For instance, Pender and Reeder (2011) use data from 2000 as the basis for their propensity score which may be insufficient to guarantee a parallel trend for their difference-in-difference estimator. Appendix 1 provides graphical evidence regarding parallel trends for key economic indicators used in this study.

3.3 Econometric structure

Our base model is the difference-in-difference (DiD) estimation that is commonly used in policy evaluation (Baum-Snow and Ferreria (2014)):

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \beta_2 Post_{it} + \beta_3 Treat_{it} * Post_{it} + \omega X_{it} + \lambda_i + \tau_t + \epsilon_{it} \quad (1)$$

where Y_{it} represents outcome variables for count i in year t. The main outcomes we consider are median household income and poverty rates. $Treat_{it}$ is an indicator vari-

able for whether county i is included in the DRA. The comparison group for treatment will be the matched counties. $Post_{it}$ represents years starting with when the DRA became operational in 2002. Though the DRA was passed into law in 2000 (Masingill (2016)), the tangible impacts should come from funding. Given that funding levels were relatively small, we doubt there were any measuable anticipation effects; nevertheless, we will test earlier years for the $Post_{it}$ year in robustness checks. β_3 is the DiD coefficient. X_{it} is a matrix of control covariates. λ_i are state fixed effects. τ_t are year fixed effects.

We will also employ instrumental variable (IV) estimation to acount for selectivity endogeneity. As noted above, our instrument is an indicator for whether the is in county the Lower Mississippi Watershed. The two first stage equations are as follows:

$$Treat_i = \alpha_0 + \alpha_1 Watershed_i + \alpha_2 X_{it} + \lambda_i + \tau_t + \mu_{it} \tag{2}$$

$$Treat_i * Post_{it} = \gamma_0 + \gamma_1 Watershed_i + \gamma_2 Post_{it} + \gamma_3 Watershed_i * Post_{it} + \gamma_4 X_{it} + \lambda_i + \tau_t + v_{it} \tag{3}$$

The results of the first stage equations are reported in Table 1 in Section 3.1. Equation 2 describes the first stage equation for the $Treat$ variable using $Watershed$ as the instrument and the other control variables defined above. Equation 3 follows the structure recommended by Bun and Harrison (2014) for $Treat_i * Post_{it}$ variable using as instruments ($Watershed_i$), the exogenous variable ($Post_{it}$), the interaction of $Watershed_i$ and $Post_{it}$, and the control variables.

3.4 Data

The base data is a 1997 to 2016 panel dataset of U.S. counties.

The demographic data is from different Census Databases. Median houshold income and poverty estimates are from the Census Small Area Income and Poverty Es-

timates (SAIPE) database.[2] Population, net migration, and county demographics are from the Census County Population by Characteristics Database. Net migration data is only available beginning in 2000 instead of 1997. Additionally, nonemployer statistics are gathered from annual estimates provided by the Census Bureau.

Information on employment, wages, and unemployment are from the Bureau of Labor Statistics (BLS). Employment and wages by industry and wages are from the *Quarterly Census of Employment and Wages*. The county unemployment rates are from the monthly Local Area Unemployment estimates averaged over the entire year.

Geographic information are from a number of different sources. The DRA counties are from the DRA Year in Review (Masingill (2016)) and ARC are added from their website ("Counties in Appalachia"). TVA, unlike the ARC and DRA, does not have to cover entire counties. As a result, the data for the counties are taken from Kline and Moretti (2014). Shapefiles for the Lower Mississippi Watershed are provided by the USGS (Ierardi (2016)).

The watershed intrument was generated by overlaying county shapefiles on the watershed shapefile in ArcGIS. The counties were defined as being in the watershed if any part of the county was in the watershed. Therefore, if no part of the county is in the watershed, it is counted as being outside the watershed.

We omit some counties from the analysis. First, counties from Alaska and Hawaii are dropped due to their extreme remoteness. Next, independent cities are omitted because there are no independent cities within the DRA. Seven parishes from Louisiana are omitted because Hurricane Katrina meant that monthly measurements were not collected from September 2005 to June 2006, making it impossible to create annual averages for those two years. We further omit all ARC counties because, while the ARC counties might have many observable similarities to DRA counties, the ARC is considerably better funded and has been in existence for nearly 40 years longer than the

[2]One concern is that the SAIPE values are measured with measurement error. However, bear in mind that we use these variables as dependent variables, meaning that the regression coefficients should remain unbiased though the coefficient standard errors will be inflated.

Table 2: Summary Statistics Before Treatment

	DRA Counties	PSM Counties	Other US Counties	Difference in Levels	Difference in Growth Rate
Median HH Income	27757.3	29286.7	35824.7	***	
	(5216.5)	(4928.7)	(8784.6)		
% in Poverty	0.198	0.181	0.132	***	
	(0.0587)	(0.0694)	(0.0559)		
% Children in Poverty	0.254	0.235	0.171	***	
	(0.0722)	(0.0854)	(0.0781)		
Unemployment Rate	0.0684	0.0686	0.0467	***	
	(0.0249)	(0.0450)	(0.0239)		
Nonemployer	1735.2	2025.461	5807.16		
	(3525.095)	(2206.594)	(20289.1)		
% Net Migration	-0.00325	-0.000275	-0.000565	***	***
	(0.00835)	(0.00733)	(0.0114)		
Number of Counties	236	33	2407		

Statistical tests run on DRA counties against the matches.
*** p<0.01, ** p<0.05, * p<0.1

DRA. ARC counties will be included in robustness checks. Finally, the counties that are part of both the ARC and the DRA have been dropped.

Table 2 shows the summary statistics for pretreatment data between the different groups in this study. The first group is the "treated" DRA counties containing all DRA counties except those that were omitted as perviously discussed. The Propensity Score Matched (PSM) counties appear to create a reasonable counterfactual. The matched counties are fairly close in income, poverty, and unemployment measures in terms of the matched counties are not statistically different from the DRA counties, while the rest of the country's corresponding variables are statistically different from the DRA counties. This illustrates that the matched counties are a much better counterfactual than the rest of the U.S. Appendix 1 shows the pre- and post-treatment trends for the dependent variables and illustrates that the treatment and control counties have parallel trends.

4 Results

Turning to the regression results, we first discuss the first-stage results and then the second-stage IV results.

Table 3: First Stage Results

	(1) Treat	(2) Treat*Post
Watershed	0.409***	0.108***
	(0.0106)	(0.0161)
Post		0.292***
		(0.0155)
Watershed * Post		0.294***
		(0.0182)
Observations	9440	9440
R^2	0.392	0.491
F	144.2	206.1

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

4.1 First Stage Results

Table 3 reports the first stage results for the two different specifications of the instrument. Recall $Watershed$ is a dummy variable that denotes whether a county is within the Lower Mississippi Watershed. $Post$ is a dummy if the year of the panel is before or after the DRA begins its funding programs, which is 2002 or there after. $Watershed * Post$ is the interaction following Bun and Harrison's (2014) correction. $Treat$ is a dummy for any DRA county. Column 1 depicts the first-stage for just the treatment, in which the F-statistic value for the strength of the instrument being 144. Column 2 reports the first-stage results for the interaction instrument. In this case, the F-statistic regarding the strength of the instrument equals 206. In both cases, the first-stage results indicate a strong instrument for the second-stage.

4.2 Income

We first consider average county median household income as a welfare measure of the middle class by providing multiple OLS and IV models to show robustness. Our preferred results are in columns 3 and 7 that include both year and state fixed effects and columns 4 and 8 that replace state fixed effects with county fixed effects. The tradeoffs are that the county fixed effect models are economically "stronger" as they remove county-specific time-invariant omitted effects, not just state omitted effects. However, this comes at the costs of removing all persistent cross-sectional effects, including

Table 4: Median Household Income

	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.0531***	-0.0344***	-0.0367***		0.0645**	0.0420*	0.0304	0.418***
	(0.00500)	(0.00397)	(0.00397)		(0.0270)	(0.0227)	(0.0193)	(0.0247)
Post 2002	0.106***	0.380***	0.383***	0.375***	0.137***	0.377***	0.374***	0.370***
	(0.00450)	(0.00712)	(0.00697)	(0.00418)	(0.0162)	(0.0161)	(0.0144)	(0.00600)
Treat*Post	0.0308***	0.0228***	0.0258***	0.00923***	-0.0195	0.0650**	0.0818***	0.0170**
	(0.00566)	(0.00448)	(0.00430)	(0.00190)	(0.0315)	(0.0263)	(0.0229)	(0.00720)
Observations	9440	9440	9440	9440	9440	9440	9440	9440
R^2	0.656	0.785	0.805	0.919	0.621	0.726	0.759	0.967
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No		Yes	
County Fixed Effects	No	No	No	Yes	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

those of the DRA, because such effects are picked up by the county fixed effects. This would suggest that the models with the state fixed effects may be more valid. Table 3 shows that the DRA had a (nominal) average median household income of $27,757 in 2000 while the rest of the country's average median household income was considerably higher at $35,824, or 29.1% above the DRA.

Using the natural log of the county's median household income as the dependent variable, Table 4 presents the regression results. The first four columns report the OLS estimates followed by the 2SLS estimates in columns 5 to 8.

With only demographic controls in column 1, the OLS regression results suggest that DRA counties are negatively selected with approximately 5.31% lower average median household income prior to the DRA's operation than the matched results (or in actuality .0531 log points). Regarding the DRA's influence, the results indicate that the average treatment effect of the DRA on average county median household income is approximately a statistically significant 3.08% increase. Column 2 adds year fixed effects, which reduces the DRA's influence to approximately 2.28%. After including state and year fixed effects in column 3, the DRA's average treatment effect increases to approcimately 2.58% compared to matched counties. For comparison, using the mean values from Table 2, the estiamted postive effects equals $716 in 2000 dollars or $1,051.38 in 2018 dollars after adjusting by the Consumer Price Index (CPI). The

Table 5: Percent of People in Poverty

	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.00852***	0.00933***	0.00920***		0.00704	0.0105	-0.00248	-0.250***
	(0.00137)	(0.00129)	(0.00126)		(0.00719)	(0.00662)	(0.00563)	(0.0113)
Post 2002	0.0110***	0.00104	0.00387*	0.0193***	0.0174***	0.00686	0.00338	0.00588**
	(0.00123)	(0.00231)	(0.00221)	(0.00185)	(0.00431)	(0.00468)	(0.00419)	(0.00275)
Treat*Post	-0.00653***	-0.00610***	-0.00734***	-0.00414***	-0.0214**	-0.0206***	-0.0120*	0.0175***
	(0.00155)	(0.00145)	(0.00136)	(0.000840)	(0.00837)	(0.00766)	(0.00667)	(0.00330)
Observations	9440	9440	9440	9440	9440	9440	9440	9440
R^2	0.654	0.691	0.735	0.630	0.654	0.690	0.732	0.928
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No	No	Yes	No
County Fixed Effects	No	No	No	Yes	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

DRA has spent about $13.90 per person in direct spending over the program's lifetime or $350 per person when including leveraged money, suggesting that DRA expenditures have remarkable returns.

The 2SLS estimates likely provide the better estimates given the endogenous selection. Jumping to column 7 when state and year fixed effects are included, the estimated DRA treatment is now a considerably larger 8.18% and statistically significant. Even when controlling for county fixed effects in column 8, there is still a positive and statistically significant treatment effect of 1.70%. What this further indicates is that the DRA negatively selected counties in that they were more slowly growing than one would otherwise expect.

4.3 Poverty

Poverty is clearly one of biggest challenges facing DRA counties. Despite strong national economic growth during the latter-half of the 1990s for most of the country, the DRA region's total poverty rate was about 50% greater than the national average. In addition, the DRA child poverty rate was also considerably higher (25.4% compared to 17.1%).

Keeping with the structure of Table 4, Table 5 reports the DiD results for the total poverty rate. With only demographic controls in column 1, the OLS results suggest

that the DRA is associateed with a statistically significant decrease in the poverty rate of .653 percentage points, which seems to be similar to the results in column 2 when adding the year fixed effects. When controlling for year and state fixed effects, the poverty reducing effect rises to .734 percentage points. As before, the 2SLS results suggest that the DRA are negatively selected with larger poverty rate reductions that the corresponding OLS results (columns 1-3 versus columns 5-7). Focusing on when state and year fixed effects are included (column 7), the 2SLS shows that the DRA was associated with a statistically significant 1.2 percentage point lower poverty rate(though at the 10% level). The effect changes to an increase in poverty when controlling for county fixed effects. Though the effect is statistically significant, it is likely due to the decreased variation from removing all persistent cross-sectional effects.

Table 6 shows the results for the child poverty rate. When including all controls in column 3, there seems to be again a small significant decrease in child poverty. The point estimate is about -0.76 percentage points that is statistically significant. The 2SLS estimates again suggest that the DRA counties are negatively selected with relative stable point estimates that are negative and greater in magnitude than the the OLS. However, only the results without year and state fixed effects (column 5) and with year and county fixed effects (column 8) are statistically significant. These estimates show the DRA program is associated with about 2 percentage points lower county child poverty rates. These results show that the DRA has mixed impacts on total and child poverty, though most of the estimates suggest that it has a negative effects. It should be noted that the results are not as precisely estimated as the the median household income results, but in general, suggest postive effects for the DRA.

4.4 Unemployment

One of the important goals of the DRA was to promote employment opportunities. To apparise this, we first consider county unemployment rates. Pretreatment, the average 2000 DRA county unemployment rate was 46.5% higher compared to the rest of the

Table 6: Percent of Children in Poverty

	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.0115***	0.0133***	0.0116***		-0.0129	-0.00981	-0.0191**	-0.337***
	(0.00197)	(0.00185)	(0.00179)		(0.0108)	(0.00987)	(0.00827)	(0.0164)
Post 2002	0.0179***	0.0194***	0.0258***	0.0468***	0.0228***	0.0198***	0.0226***	0.0308***
	(0.00178)	(0.00332)	(0.00314)	(0.00271)	(0.00647)	(0.00698)	(0.00616)	(0.00398)
Treat*Post	-0.00627***	-0.00617***	-0.00756***	-0.00547***	-0.0218*	-0.0180	-0.0148	0.0204***
	(0.00223)	(0.00208)	(0.00193)	(0.00123)	(0.0126)	(0.0114)	(0.00981)	(0.00479)
Observations	9440	9440	9440	9440	9440	9440	9440	9440
R^2	0.627	0.671	0.715	0.633	0.622	0.666	0.701	0.905
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No	No	Yes	No
County Fixed Effects	No	No	No	Yes	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

country, or the corresponding 2000 unemployment rate levels are 6.84% in the DRA compared to 4.67% in the rest of the country.

The unemployment rate results are shown in Table 7. Unemployment rates appears to decline more in the treated DRA counties relative to their matched counties, though again in the OLS estimates, the results are not precisely estimated. With the state and year fixed effects (column 3), the DRA is associated with about a statistically significant 0.14 percentage point lower unemployment rate. The 2SLS estimates continue to tell a negative selectivity story with negative estimates in the negative one percentage point range. When examining the model with state and year fixed effects (column 7), the unemployment rate in the DRA counties appears to be about 1.25 percentage points lower compared to the matched counties. The effect with county and year fixed effects (column 8) shows similar results, though the magnitude of the effect is reduced by about half.

The decrease in the unemployment rate actually falls in line with many of the assertions the DRA makes. The DRA has claimed over its existence that it has helped create or helped retained 26,218 jobs (Masingill (2016)). While self-reported evaluations should always be treated skeptically, the evidence is consistent with the DRA improving the local labor market. Consequently, if at least the direction of the DRA's self-reported estimate is accurate, this change in unemployment is expected.

Table 7: Unemployment Rate

	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.00216**	0.00378***	0.00255***		0.00942**	0.00722**	0.0106***	-0.0714***
	(0.000893)	(0.000675)	(0.000656)		(0.00466)	(0.00349)	(0.00291)	(0.00756)
Post 2002	0.0101***	-0.00942***	-0.00802***	-0.0118***	0.0170***	-0.00466*	-0.00219	-0.00874***
	(0.000803)	(0.00121)	(0.00115)	(0.00128)	(0.00280)	(0.00247)	(0.00216)	(0.00184)
Treat*Post	-0.000976	-0.00111	-0.00140**	-0.000763	-0.0155***	-0.0118***	-0.0125***	-0.00571***
	(0.00101)	(0.000762)	(0.000710)	(0.000580)	(0.00543)	(0.00403)	(0.00345)	(0.00221)
Observations	9440	9440	9440	9440	9440	9440	9440	9440
R^2	0.407	0.652	0.688	0.684	0.406	0.649	0.684	0.825
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No	No	Yes	No
County Fixed Effects	No	No	No	Yes	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

4.5 Self Employment

One of the largest programs the DRA provides is to promote and develop entrepreneurship (Masingill (2016)). To explore this, we use rate of change of the Census Bureau's Nonemployer Statistics. While this is only a proxy for entrepreneurship, it covers most of the self-employment and new businesses in the region, which is a worthy goal in itself. There is also a substantial amount of evidence supporting the benefits self employment can have (i.e. Rupasingha and Goetz (2013)). The regression results are reported in Table 8.

In the OLS model, the point estimates are actually slightly negative and statistically significant. However, the 2SLS results results show the positive and significant results, suggesting favorable DRA effects. In column 7, for instance, there is a 2.22 percentage point increase in the number of new nonemployers being formed over the matched counties. All of the 2SLS results show similar point estimates, with the exception of those in column 8. This is quite notable given the national rate of nonemployer increase over the same time period, averaged 1.55%. These results suggest that the DRA is meeting its entrepreneurship goals. The counties are experiencing a boom in new small firms over their matched counties, consistent with success in the Delta Entrepreneurship Network.

Table 8: Nonemployer

	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.0135***	0.0110***	0.00823***		-0.0465***	-0.0424***	-0.0192*	-0.0928***
	(0.00222)	(0.00205)	(0.00213)		(0.0134)	(0.0122)	(0.0105)	(0.0319)
Post 2002	0.0122***	-0.00127	-0.00573*	-0.0289***	-0.0196**	-0.0300***	-0.0218***	-0.0396***
	(0.00192)	(0.00338)	(0.00343)	(0.00495)	(0.00771)	(0.00797)	(0.00724)	(0.00769)
Treat*Post	-0.00926***	-0.00851***	-0.00661***	-0.00854***	0.0540***	0.0425***	0.0222*	0.00856
	(0.00246)	(0.00227)	(0.00226)	(0.00236)	(0.0152)	(0.0137)	(0.0121)	(0.00971)
Observations	8968	8968	8968	8968	8968	8968	8968	8968
R^2	0.0793	0.220	0.237	0.201	0.00178	0.161	0.221	0.988
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No	No	Yes	No
County Fixed Effects	No	No	No	Yes	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

Table 9: Net Migration

	OLS				2SLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.000106	0.000330	0.000309		-0.0000806	-0.00105	-0.000772	0.0322***
	(0.000303)	(0.000279)	(0.000282)		(0.00277)	(0.00248)	(0.00213)	(0.00415)
Post 2002	0.0000902	0.00371***	0.00357***	0.00322***	0.000867	0.00360***	0.00362***	0.00483***
	(0.000237)	(0.000346)	(0.000349)	(0.000446)	(0.00146)	(0.00134)	(0.00120)	(0.00101)
Treat*Post	-0.000271	-0.000396	-0.000533*	-0.000307	-0.00202	-0.000538	-0.00114	-0.00301*
	(0.000320)	(0.000293)	(0.000290)	(0.000271)	(0.00297)	(0.00266)	(0.00231)	(0.00156)
Observations	8024	8024	8024	8024	8024	8024	8024	8024
R^2	0.824	0.833	0.843	0.819	0.824	0.833	0.843	0.797
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No	No	Yes	No
County Fixed Effects	No	No	No	Yes	No	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

4.6 Net Migration

The previous results generally show positive economic effects. There are positive gains in income, decreases in unemployment and poverty, and generally positive effects on business start-ups. One possible reason for this is an influx of migrants, especially high-skill migrants. For example, programs like Delta Doctors aim to attract high-skill health-care workers into the DRA to address current shortages. Likewise, we may expect inward net migration if the DRA generally increases employment opportunities.

Table 9 reports corresponding results using annual net migration as a percentage of total county population from 2000 to 2015 as the dependent variable. Table's 9 re-

sults suggest weakly negative effects from the DRA's efforts. That is the point estimates are negative, but only in the OLS models with state and year fixed effects (column 3) and the IV results with county fixed effects (column 8) are statistically significant. Overall, it is only possible to say that the DRA was not increasing net migration (though it may have been negative), which may be desired to ensure that the original low-income residents are the ones benefiting from DRA policy. A typical positive criteria of place-based policy is imperfect labor force mobility (Bartik (1991), Partridge and Rickman (2015a, 2015b). To be sure, it is still possible that high-skilled migrants are in-migrating on net, but this overwhelmed by net migration of low-skilled workers.

4.7 DRA Dynamic Effects Over Time

A possible weakness is that the previous models assume that the DRA's treatment effect begins immediately and remains unchanged thereafter. Possibly, a more likely dynamic pattern, is that the DRA's treatment effect initially rises as it begins to operate and then levels off over time because it takes time for the DRA to affect outcomes. Thus, we explored how the DRA's treatment effects varied over time. Our regression model is the exact same as Equations 1 -3) from Section 3.3, however $Treatment * Post$ was replaced by $Treatment * Year$ in which $Year$ is a dummy variable for each year from 2002 onwards. The IV correction is applied for each $Treatment * Year$ variable. The results are reported in Appendix 2 and only show one OLS and 2SLS regression with demographic controls and state and year fixed effects.

First, we consider the median income change models. Both OLS and IV regression results show nearly identical results in terms of magnitude and significance. In the OLS result, it takes until 2006 for positive growth to occur; however, sustained positive effects over the matched counties are not noticeable until 2009. Post 2009, there is a consistent higher level of income hovering around 3%. The 2SLS results show positive sustained growth starting in 2007 instead of 2009 with a larger magnitude (somewhere normally between 3 and 4%).These results suggest that by assuming an average treatment effect that does not vary time, our previous median household income results

appear to underestimated the contemporaneous effects of the DRA after its influence more strongly took hold.

The poverty trend results are less unclear. The total poverty results show no clear or statistically significant impact. The results for child poverty are a bit more complex. The OLS results provide some evidence of a growing impact on child poverty, with a decrease at about 1.5%. The 2SLS results paint a less clear picture with little trend. There is no consistent change in child poverty rate. Despite this, the point estimates are mostly negative. This would suggest there is some negative trend on poverty.

The OLS unemployment results suggest the DRA's decreasing effects do not kick in until 2006. The IV results indicate less of clear trend, though during the 2009-2010 period, the effects are negative and stastically significant. This provides evidence that during the Great Recession and its immediate aftermath, the DRA provided needed help in reducing unemployment.

Finally, both the IV and OLS net migration results continue to show no consistent DRA pattern over time.

5 Robustness Checks

This section presents some robustness checks of the results in Section 4.

5.1 ARC Included

As noted above, we did not include the ARC counties as possible candidates in the pool of possible match counties, though this came at the cost of losing many counties that would have provided closer matches, providing a better counterfactual. They also increase the number of matched counties to 90 from 33 in the counterfactual. Generally, we would expect that if the much better funded and more established ARC was successful in improving its counties' outcomes, that would reduce the estimated effects of the DRA. The corresponding results are available upon request. Perhaps surprisingly, most of the conclusions do not tangibly change. All estimates remain the same

sign and generally the same statistical significance, only the point estimates slightly change, however they continue to support a general theme that the DRA has modest postive effects.

5.2 Different Treatment Year

As described above, the DRA was became law in 2000, but did not become operational until 2002. If there were any tangible anticipation effects, it would make sense to start the treatment effects in 2000. As a result, we assess the robustness of the results using 2000 as the beginning of the DRA's treatment. Net-migration results are not reported as there would be no pretreatment years due to data availability.

The results from these regressions are available upon request. There are some differences from using 2002 as treatment compared to 2000. First, median income has considerably smaller impacts. Median income goes up by about 3% compared to about 8% before. Next, unemployment rate estimates show smaller impacts, dropping by .612 percentage points compared to 1.25. Finally, poverty rate shows no significant impacts. One reason for the smaller results could reflect from the negative selection of the DRA counties—i.e., they were doing worse than their matched counterparts prior to the DRA.

5.3 Adjusting the Matching Algorithm

Another possible concern is our use of one-to-one matching with replacement for the propensity score matched counties. This results in only 33 counties being used when matched. To address this, we change the matching algorithm by using nearest neighbor matching with replacement and three matches. This is a difference from one-to-one by the algorithm finds three untreated counties that matches closest on observable characteristics instead of one. This method is fairly common in the literature and should add more control counties to compare against. Updated summary statistics and the corresponding regression results are available upon request.

The results suggest that three nearest-neighbor matching seems inferior in identifying close matches to the treatment counties. Before treatment begins, the nearest neighbor matches have higher incomes, lower poverty rates, and lower unemployment. The outcome variable means for the matched counties are farther from the DRA county means than when one-to-one matching is used. Thus even though the nearest neighbor has a larger sample of control counties (61 compared to 33), it may come at the cost of accurate controls.

Turning to these regression results, the main results are fairly robust. For median income, the results from the three nearest neighbor matches seem to be of slightly smaller magnitude than the base results, whereas statistical significance is also little changed. These results are consistent across the other economic measures in terms of magnitude of the coefficients and their statistical significance.

6 Conclusion

Given the growing interest in place-base policies, this paper assesses a relatively small policy intervention, the Delta Regional Authority. We ask whether quite small place-based programs like the DRA have any measurable efforts and in turn, whether they are even worth the effort beyond a political desire to appear to be trying something. This paper explores the economic impact of the DRA on its member counties, representing the first paper to examine its longer-run outcomes. We use the Lower Mississippi Watershed as a unique instrumental variable to correct for selection bias into the DRA. Using this correction along with propensity score matching, we assess the effectiveness of the DRA.

Using both OLS and IV difference in difference estimation, we find a number of significant economic benefit to the DRA. There are positive benefits to unemployment and income growth and it appears to decrease child poverty. However, it had no measurable impact on migration. Overall the program increases annual median household income by $1051.38 assuming its benefits are uniform across the income distribution.

Given its annual budget is quiet small ($25.6 million in FY 2018), the program appears to easily pass cost-benefit analysis. One possible reason for its relative success is that the relative small nature of the DRA allows it to pick only the most worthy initiatives with the highest rate of return. While at the margin, this does suggest that expanding the DRA would be beneficial, it is not guaranteed if the program was greatly expanded, whether we would continue to observe such a high payoff. Indeed this federal-state commission can bring all the relative federal, state, and local governments and stake holders together to cooperate. Nonetheless, the DRA appears to be benefiting one of the poorest regions in the country at very little cost.

There is much work that can be done to further research in this field. First, using synthetic control method has the potential to create a better counterfactual with a more varied donor pool. In this study, there were very few counties that matched to the treated counties. Using synthetic control method, the mix of a number of counties would allow a researcher to exploit additional variation amongst the control and treated groups. We also did not consider many potential economic indicators that should be explored, including but not limited to health outcomes (given the DRA's healthcare focus) and happiness indicators. Another example is conducting a more detailed sectoral analysis of the DRA's effects to assess which industries are being helped or harmed by its programming.

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Appendix 1: Parallel Trend Graphs

Figure 2: Income Trends

Figure 3: Poverty Trends

Figure 4: Child Poverty Trends

Figure 5: Unemployment Trends

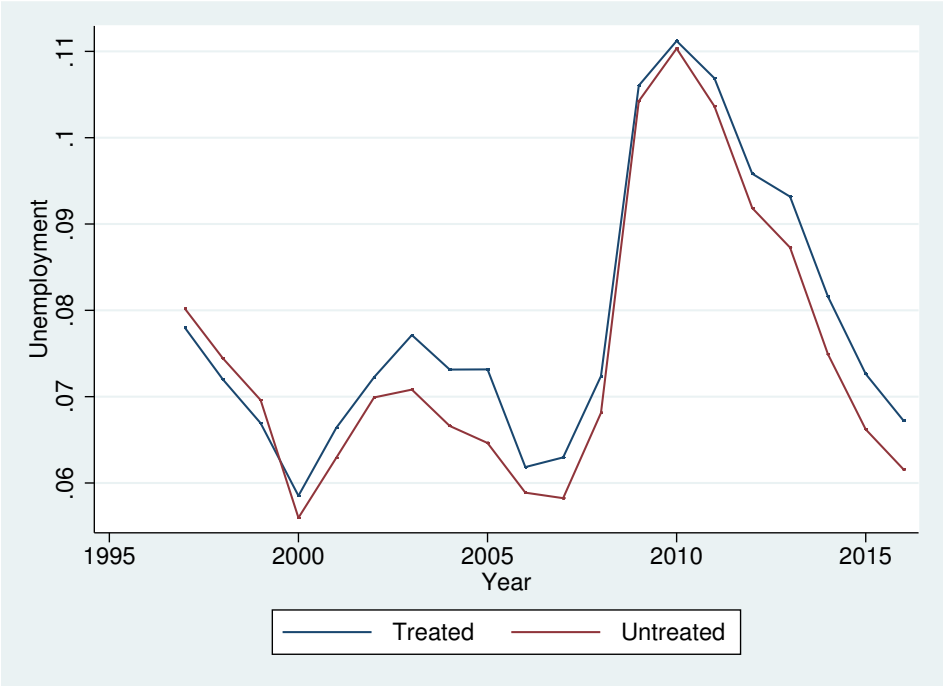


Figure 6: Nonemployer

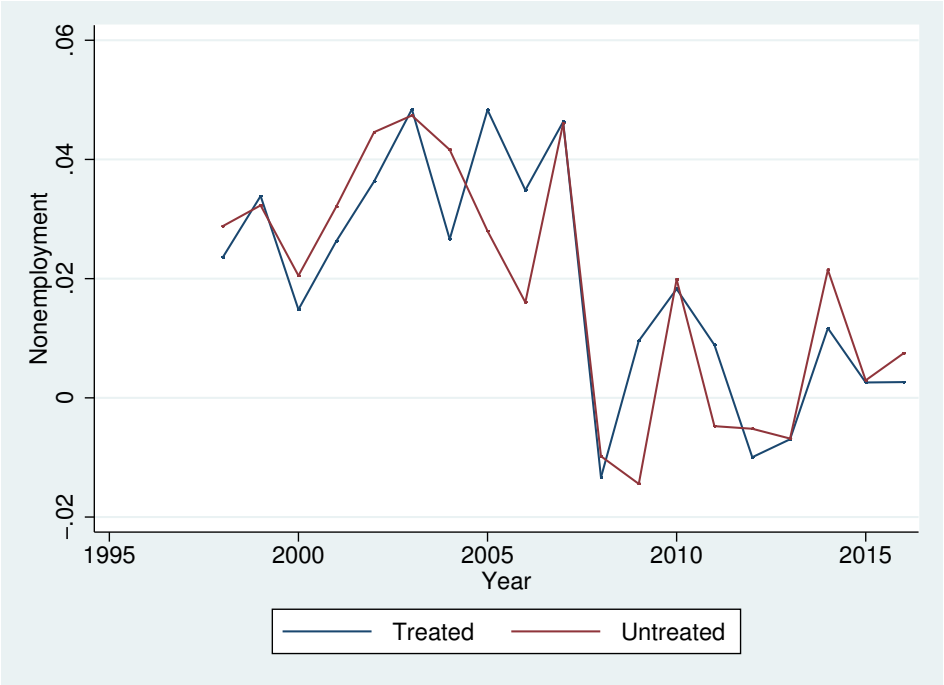
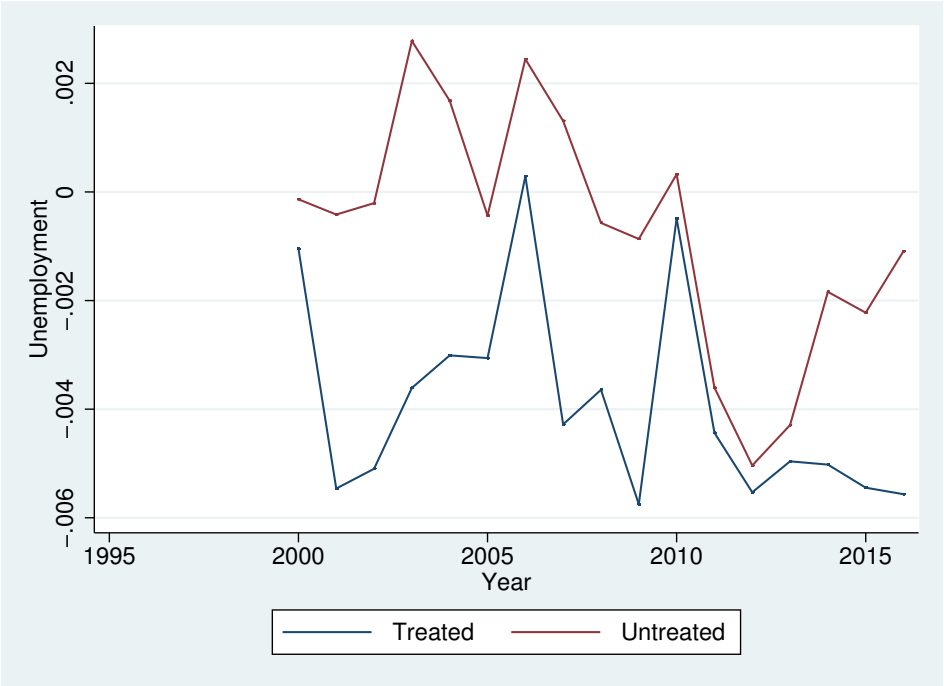


Figure 7: Net Migration



Appendix 2: DRA Dynamic Effects Over Time Tables

Table 10: Median Household Income

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Treatment	-0.00476		0.155***	-0.285***
	(0.00392)		(0.0132)	(0.0219)
Treatment*2002	0.0110	0.000632	-0.118***	0.000632
	(0.00928)	(0.00365)	(0.0143)	(0.00365)
Treatment*2003	0.00838	0.0102***	-0.121***	0.0102***
	(0.00926)	(0.00364)	(0.0143)	(0.00364)
Treatment*2004	0.00689	0.00657*	-0.123***	0.00657*
	(0.00925)	(0.00363)	(0.0143)	(0.00363)
Treatment*2005	0.0119	0.0114***	-0.119***	0.0114***
	(0.00926)	(0.00365)	(0.0143)	(0.00365)
Treatment*2006	0.0239***	0.0234***	-0.106***	0.0234***
	(0.00926)	(0.00365)	(0.0143)	(0.00365)
Treatment*2007	0.00864	0.00233	-0.123***	0.00233
	(0.00928)	(0.00365)	(0.0144)	(0.00365)
Treatment*2008	0.00521	-0.00341	-0.127***	-0.00341
	(0.00930)	(0.00367)	(0.0145)	(0.00367)
Treatment*2009	0.0326***	0.0233***	-0.1000***	0.0233***
	(0.00931)	(0.00367)	(0.0145)	(0.00367)
Treatment*2010	0.0304***	0.0253***	-0.103***	0.0253***
	(0.00930)	(0.00367)	(0.0145)	(0.00367)
Treatment*2011	0.0237**	0.0140***	-0.110***	0.0140***
	(0.00931)	(0.00367)	(0.0145)	(0.00367)
Treatment*2012	0.0281***	0.0225***	-0.104***	0.0225***
	(0.00930)	(0.00367)	(0.0144)	(0.00367)
Treatment*2013	0.0344***	0.0229***	-0.0989***	0.0229***
	(0.00931)	(0.00367)	(0.0145)	(0.00367)
Treatment*2014	0.0159*	0.00599	-0.118***	0.00599
	(0.00930)	(0.00367)	(0.0145)	(0.00367)
Treatment*2015	0.0253***	0.0207***	-0.108***	0.0207***
	(0.00931)	(0.00367)	(0.0145)	(0.00367)
Observations	9800	9800	9800	9800
^{2}	0.770	0.906	0.730	0.966
Demographic Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

Table 11: Percent of People in Poverty

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Treatment	0.00132		-0.0209***	0.0748***
	(0.00127)		(0.00401)	(0.00954)
Treatment*2002	-0.00559*	-0.00175	0.0124***	-0.00175
	(0.00300)	(0.00159)	(0.00433)	(0.00159)
Treatment*2003	-0.00672**	-0.00558***	0.0113***	-0.00558***
	(0.00299)	(0.00159)	(0.00433)	(0.00159)
Treatment*2004	-0.00474	-0.00367**	0.0134***	-0.00367**
	(0.00299)	(0.00159)	(0.00434)	(0.00159)
Treatment*2005	0.00201	0.00383**	0.0202***	0.00383**
	(0.00300)	(0.00159)	(0.00435)	(0.00159)
Treatment*2006	-0.00741**	-0.00534***	0.0107**	-0.00534***
	(0.00300)	(0.00159)	(0.00434)	(0.00159)
Treatment*2007	-0.00419	-0.00157	0.0141***	-0.00157
	(0.00300)	(0.00159)	(0.00436)	(0.00159)
Treatment*2008	-0.00478	-0.00236	0.0137***	-0.00236
	(0.00301)	(0.00160)	(0.00439)	(0.00160)
Treatment*2009	-0.0111***	-0.00961***	0.00738*	-0.00961***
	(0.00301)	(0.00160)	(0.00439)	(0.00160)
Treatment*2010	-0.0179***	-0.0171***	0.000680	-0.0171***
	(0.00301)	(0.00160)	(0.00440)	(0.00160)
Treatment*2011	-0.00832***	-0.00607***	0.0103**	-0.00607***
	(0.00301)	(0.00160)	(0.00441)	(0.00160)
Treatment*2012	-0.00496*	-0.00339**	0.0135***	-0.00339**
	(0.00301)	(0.00160)	(0.00439)	(0.00160)
Treatment*2013	-0.0105***	-0.00733***	0.00807*	-0.00733***
	(0.00301)	(0.00160)	(0.00441)	(0.00160)
Treatment*2014	-0.00780***	-0.00528***	0.0108**	-0.00528***
	(0.00301)	(0.00160)	(0.00441)	(0.00160)
Treatment*2015	-0.00564*	-0.00417***	0.0129***	-0.00417***
	(0.00301)	(0.00160)	(0.00441)	(0.00160)
Observations	9800	9800	9800	9800
R^2	0.736	0.635	0.728	0.929
Demographic Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

Table 12: Percent of Children in Poverty

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Treatment	0.00190		-0.0570***	0.0590***
	(0.00180)		(0.00591)	(0.0150)
Treatment*2002	-0.00611	-0.000735	0.0416***	-0.000735
	(0.00427)	(0.00250)	(0.00639)	(0.00250)
Treatment*2003	-0.00638	-0.00534**	0.0415***	-0.00534**
	(0.00426)	(0.00250)	(0.00639)	(0.00250)
Treatment*2004	-0.0106**	-0.00945***	0.0374***	-0.00945***
	(0.00425)	(0.00249)	(0.00639)	(0.00249)
Treatment*2005	0.00335	0.00486*	0.0516***	0.00486*
	(0.00426)	(0.00250)	(0.00642)	(0.00250)
Treatment*2006	-0.00188	-0.000485	0.0462***	-0.000485
	(0.00426)	(0.00250)	(0.00641)	(0.00250)
Treatment*2007	-0.00196	0.00148	0.0464***	0.00148
	(0.00427)	(0.00251)	(0.00643)	(0.00251)
Treatment*2008	-0.00859**	-0.00519**	0.0403***	-0.00519**
	(0.00428)	(0.00252)	(0.00647)	(0.00252)
Treatment*2009	-0.0167***	-0.0135***	0.0322***	-0.0135***
	(0.00428)	(0.00252)	(0.00648)	(0.00252)
Treatment*2010	-0.0146***	-0.0122***	0.0345***	-0.0122***
	(0.00428)	(0.00252)	(0.00649)	(0.00252)
Treatment*2011	-0.0179***	-0.0133***	0.0314***	-0.0133***
	(0.00428)	(0.00252)	(0.00650)	(0.00252)
Treatment*2012	-0.00488	-0.00120	0.0439***	-0.00120
	(0.00428)	(0.00252)	(0.00647)	(0.00252)
Treatment*2013	-0.0209***	-0.0155***	0.0283***	-0.0155***
	(0.00428)	(0.00252)	(0.00650)	(0.00252)
Treatment*2014	-0.0166***	-0.0128***	0.0327***	-0.0128***
	(0.00428)	(0.00252)	(0.00651)	(0.00252)
Treatment*2015	-0.00808*	-0.00620**	0.0411***	-0.00620**
	(0.00428)	(0.00252)	(0.00650)	(0.00252)
Observations	9800	9800	9800	9800
R^2	0.716	0.637	0.685	0.906
Demographic Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

Table 13: Unemployment Rate

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Treatment	0.00408***		-0.00613*** 0.0236***	
	(0.000664)		(0.00209)	(0.00715)
Treatment*2002	-0.00712***	-0.00634***	0.00115	-0.00634***
	(0.00157)	(0.00119)	(0.00226)	(0.00119)
Treatment*2003	-0.000516	0.0000518	0.00778***	0.0000518
	(0.00157)	(0.00119)	(0.00226)	(0.00119)
Treatment*2004	0.000129	0.00100	0.00844***	0.00100
	(0.00157)	(0.00119)	(0.00226)	(0.00119)
Treatment*2005	-0.00261*	-0.001000	0.00575**	-0.001000
	(0.00157)	(0.00119)	(0.00227)	(0.00119)
Treatment*2006	-0.00867***	-0.00710***	-0.000337	-0.00710***
	(0.00157)	(0.00119)	(0.00227)	(0.00119)
Treatment*2007	-0.0172***	-0.0147***	-0.00880***	-0.0147***
	(0.00157)	(0.00119)	(0.00228)	(0.00119)
Treatment*2008	-0.00948***	-0.00652***	-0.00101	-0.00652***
	(0.00158)	(0.00120)	(0.00229)	(0.00120)
Treatment*2009	-0.0130***	-0.00932***	-0.00455**	-0.00932***
	(0.00158)	(0.00120)	(0.00229)	(0.00120)
Treatment*2010	-0.0105***	-0.00659***	-0.00196	-0.00659***
	(0.00158)	(0.00120)	(0.00230)	(0.00120)
Treatment*2011	-0.00701***	-0.00267**	0.00154	-0.00267**
	(0.00158)	(0.00120)	(0.00230)	(0.00120)
Treatment*2012	-0.00806***	-0.00401***	0.000400	-0.00401***
	(0.00158)	(0.00120)	(0.00229)	(0.00120)
Treatment*2013	-0.00971***	-0.00591***	-0.00118	-0.00591***
	(0.00158)	(0.00120)	(0.00230)	(0.00120)
Treatment*2014	-0.00661***	-0.00320***	0.00195	-0.00320***
	(0.00158)	(0.00120)	(0.00230)	(0.00120)
Treatment*2015	-0.000941	0.00148	0.00759***	0.00148
	(0.00158)	(0.00120)	(0.00230)	(0.00120)
Observations	9800	9800	9800	9800
R^2	0.694	0.692	0.686	0.831
Demographic Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

Table 14: Nonemployer

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Treatment	0.00514***		-0.00366	-0.0545*
	(0.00199)		(0.00649)	(0.0290)
Treatment*2002	-0.0242***	-0.0246***	-0.0169**	-0.0246***
	(0.00443)	(0.00444)	(0.00677)	(0.00444)
Treatment*2003	0.00657	0.00738*	0.0139**	0.00738*
	(0.00442)	(0.00442)	(0.00677)	(0.00442)
Treatment*2004	0.000246	-0.000325	0.00757	-0.000325
	(0.00441)	(0.00442)	(0.00678)	(0.00442)
Treatment*2005	0.00872**	0.00841*	0.0161**	0.00841*
	(0.00442)	(0.00444)	(0.00680)	(0.00444)
Treatment*2006	-0.00664	-0.00691	0.000705	-0.00691
	(0.00442)	(0.00444)	(0.00679)	(0.00444)
Treatment*2007	0.00242	0.00158	0.00981	0.00158
	(0.00443)	(0.00445)	(0.00682)	(0.00445)
Treatment*2008	-0.00456	-0.00535	0.00288	-0.00535
	(0.00444)	(0.00446)	(0.00686)	(0.00446)
Treatment*2009	0.00997**	0.00812*	0.0174**	0.00812*
	(0.00444)	(0.00446)	(0.00686)	(0.00446)
Treatment*2010	-0.0234***	-0.0242***	-0.0160**	-0.0242***
	(0.00444)	(0.00446)	(0.00688)	(0.00446)
Treatment*2011	-0.00203	-0.00352	0.00547	-0.00352
	(0.00444)	(0.00447)	(0.00689)	(0.00447)
Treatment*2012	-0.0330***	-0.0339***	-0.0256***	-0.0339***
	(0.00444)	(0.00446)	(0.00685)	(0.00446)
Treatment*2013	-0.0125***	-0.0131***	-0.00497	-0.0131***
	(0.00444)	(0.00446)	(0.00688)	(0.00446)
Treatment*2014	-0.0207***	-0.0208***	-0.0132*	-0.0208***
	(0.00444)	(0.00446)	(0.00689)	(0.00446)
Treatment*2015	0.00403	0.00391	0.0115*	0.00391
	(0.00444)	(0.00446)	(0.00687)	(0.00446)
Observations	9310	9310	9310	9310
R^2	0.203	0.165	0.201	0.236
Demographic Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.

Table 15: Net Migration

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Treatment	-0.000107		-0.00260***	0.00917***
	(0.000230)		(0.000867)	(0.00302)
Treatment*2002	-0.000501	-0.000632	0.00168**	-0.000632
	(0.000432)	(0.000385)	(0.000851)	(0.000385)
Treatment*2003	0.00183***	0.00145***	0.00401***	0.00145***
	(0.000431)	(0.000383)	(0.000851)	(0.000383)
Treatment*2004	-0.000127	-0.000181	0.00206**	-0.000181
	(0.000430)	(0.000383)	(0.000853)	(0.000383)
Treatment*2005	0.000793*	0.000411	0.00299***	0.000411
	(0.000431)	(0.000385)	(0.000855)	(0.000385)
Treatment*2006	0.000437	0.0000263	0.00263***	0.0000263
	(0.000431)	(0.000386)	(0.000854)	(0.000386)
Treatment*2007	0.000222	-0.00000946	0.00243***	-0.00000946
	(0.000432)	(0.000386)	(0.000857)	(0.000386)
Treatment*2008	0.000508	0.000501	0.00272***	0.000501
	(0.000433)	(0.000387)	(0.000861)	(0.000387)
Treatment*2009	-0.000149	0.000117	0.00206**	0.000117
	(0.000433)	(0.000387)	(0.000860)	(0.000387)
Treatment*2010	-0.00108**	-0.000910**	0.00114	-0.000910**
	(0.000432)	(0.000386)	(0.000862)	(0.000386)
Treatment*2011	0.000586	0.000857**	0.00281***	0.000857**
	(0.000433)	(0.000387)	(0.000863)	(0.000387)
Treatment*2012	0.000243	0.000415	0.00245***	0.000415
	(0.000432)	(0.000387)	(0.000860)	(0.000387)
Treatment*2013	-0.000231	0.0000368	0.00198**	0.0000368
	(0.000432)	(0.000386)	(0.000860)	(0.000386)
Treatment*2014	0.000382	0.000577	0.00260***	0.000577
	(0.000432)	(0.000386)	(0.000860)	(0.000386)
Treatment*2015	0.000550	0.000680*	0.00276***	0.000680*
	(0.000432)	(0.000386)	(0.000858)	(0.000386)
Observations	8330	8330	8330	8330
R^2	0.834	0.796	0.832	0.875
Demographic Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes

*** p<0.01, ** p<0.05, * p<0.1
Standard errors are in parentheses.
Demographic controls include variables for employment industry, race, and gender.