A Quasi-Experimental Analysis of the Effectiveness of Credit Pooling for Infrastructure Financing on Economic Growth in Rural Southern Counties

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1. Background

Introduction

State credit pools are an innovation intended to spread the risks associated with infrastructure investment in any specific place. Since the 1987 amendments to the Federal Clean Water Act authorized federal monies to establish revolving funds for low-interest loans to construct new and expanded public wastewater facilities, six southeastern states (Alabama, Georgia, Kentucky, North Carolina, South Carolina, and Tennessee) have established various forms of state credit pools. In principle, these pools should make it easier for small rural places to access monies needed to develop infrastructure, and to acquire such funds at lower cost than would have otherwise been the case.

This report provides an evaluation of the impact of state credit pools on economic growth in places taking advantage of state credit pool loans. In an important sense, the evaluation is preliminary, since no detailed case studies have been done. The study makes uses of a quasi-experimental technique to match counties in the rural South that acquired such loan funds (treated counties) with similar counties (control counties) that did not access these funds. Differences in economic growth between the treated and control counties were then measured and evaluated.

This report is an abridgement of a dissertation by the senior author (Ray, 1999). That dissertation contained both more background material and the results of empirical analysis using several alternative models. In the interest of space, we report here only the essential materials necessary to understand the results of the most comprehensive form of the model tested. Readers and researchers interested in more detail are advised to consult the dissertation.

The report is organized as follows: We begin with a general background discussion of the relationship between infrastructure and economic growth, and the special problems of financing infrastructure in small rural places which credit pools are designed to address. We then explain the quasi-experimental methodology and its application in this particular study. Next, we report the results of empirical analysis that shows there is some preliminary evidence that state credit pools may have had positive impacts of economic growth in places accessing loans from such pools. Finally, we offer some tentative conclusions and some suggestions for future research.

The Problem: Infrastructure Finance and Rural Economic Growth

There is a dilemma in strategies to encourage economic growth in rural places. There seems to be a general agreement that places lacking surplus capacity in water and sewer infrastructure are non-players in competition for outside capital investments (Fox, 1988; Fox and Smith, 1990; Sears, Rowley, and Reid, 1996; Johnson, 1996). Without such investments, those places are destined to suffer economic stagnation and decline. On the other hand, it is also understood that having such infrastructure does not assure such investments. Not every place that has surplus water and sewer infrastructure will prosper.

These two facts make investment in rural infrastructure a risky undertaking. Some places that make such investments may still lose out on future economic development and be left with a long-term surplus capacity problem which strains the ability of the community to repay bonds used to pay for such capacity. The risk that they build it, but the investors will not come, can be borne by local property owners by issuing full faith and credit bonds, or it can be transferred to bondholders at some premium on interest rates. The amount of the premium will presumably depend upon the money market's assessment of the growth potential of the community selling the bonds.

The money market will have little difficulty in assessing the growth potential of major well-known urban centers and large geographic regions. Assessing the growth potential of specific places among the hundreds of villages, towns, and small cities in the hinterland, however, is a much more difficult and costly undertaking. Indeed, too little is known by regional economists with regard to forecasting growth in specific places to allow such an assessment to be made with any scientific reliability. Consequently, the risk premium that rural communities face is likely to be higher than that for major urban centers because bond repayment capacity depends upon factors that are not only unknown, but at present, unknowable.

The relatively high-risk premium for rural community bonds translates into a higher cost for infrastructure capital in rural areas. In the years after World War II, the federal and state government offset this disadvantage, at least partially, with grants and below-market loans to rural communities. But efforts to bring the federal budget into balance have substantially reduced federal subsidies, and rural communities have been forced to finance more and more infrastructure in the private money market. Other things constant, that relatively high-risk premium extracted in the private money market can be expected to cause rural communities to undertake fewer of the investments in infrastructure they must have to grow.

State Credit Pools

State credit pools are essentially institutional devices for spreading the risks of investing in place-specific infrastructure. Although the details of the organization of these pools vary somewhat from state to state, the differences are of secondary importance for this study. State credit pools buy the bonds issued by specific communities for water and wastewater facilities, bundle those bonds together in a single package, and use them as collateral for bonds which the credit pools sell in the money market. In most cases, the state credit pool has its own capital in the form of revolving loan money under the Federal Clean Water Act that is supplemented by a direct state appropriation of seed money. But in almost every case, the state credit pools also issue revenue bonds for sale in money markets to obtain additional loanable funds (see Petersen, et al., 1988).

Investors who purchase bonds issued by the state credit pools assume risks associated with the repayment abilities of all of the communities whose bonds are bundled together by the state credit pool. Hence, investors are not exposed directly to the risk of non-growth in a specific rural place, but to the more general risk of non-growth in the rural places of a specific state. Even if the borrowers from state credit pools are not representative of all the rural places in that state, the need for information on the specific growth prospects of particular places is reduced, and accordingly, the costs of funds for infrastructure development are lower than they otherwise might have been.

2. Quasi-Experimental Methodology

Vocabulary and Basic Concepts

The term “quasi-experimental” refers to research designs that have most aspects of an experiment (Cook and Campbell, 1979). They have a treatment, an outcome measure, and a control group whose experience serves as a baseline against which the effects of treatment can be measured. Missing, however, is the random assignment to treated and control group before treatment occurs. The control group is selected after the treatment has happened, but in a manner that permits isolating the treatment effect.

There exists a vast body of literature in regional science utilizing the quasi-experimental method for the empirical research. But none could be found to be using this method for studying an impact similar to the present research. It has been used to study the impact of highway constructions (Blum, 1982; Briggs, 1980; Isserman, 1987; Rephann, 1993), airport services (Wheat, 1970; Farnsworth, 1972), fiscal policies (Bender and Schwiff, 1982), energy booms (Isserman and Merrifield, 1987) population projections (Isard et al., 1960). Thus this study is a novel approach to identifying the impact of this relatively new technique of financing infrastructure investment on the economy of the local area. It is significant because its empirical insights may provide some hints to policy makers interested in rural areas about choices in the method of financing infrastructure investment.

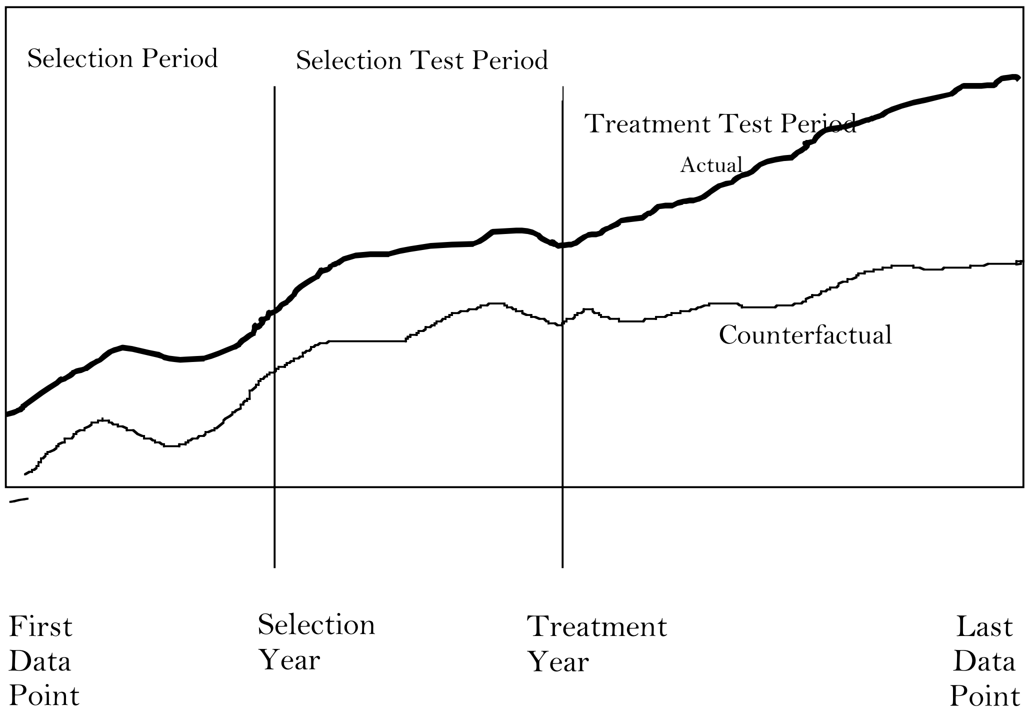
The question we seek to answer is whether rural communities in the South that have borrowed for water and sewer infrastructure from state credit pools have achieved faster rates of economic growth than similar counties that did not obtain such loans. The treatment is the existence of pooled loans for infrastructure; the control, communities that received no such funds but that, prior to the time of treatment, had a history of economic growth that closely matched a particular treated community. The critical problem is selecting the control communities so that they provide a reasonable indication of what is likely to have happened to the treated economies in the absence of the treatment.

Application in Study

This study makes use of tools pioneered by Isserman and Merrifield (1982) to select control communities (see also Isserman, 1991; 1994). We will not present an exhaustive review of that literature here and refer those interested to Ray (1999). For our purposes, these data were first divided into two sets: 1) Data on non-metro counties of the states which had a credit pooling system operating by 1989 [Alabama (1987), Georgia (1986), Kentucky (1988), North Carolina (1989), South Carolina (1989), and Tennessee (1970)]; 2) Data on the non-metro counties of the states without a state credit pool (Florida and Mississippi). Within the former set, there were 36 counties that had obtained loans from state credit pools in 1989, the first year in which all six states had operating pools. Thus, 1989 was established as the treatment year, and the treatment period is 1989-1995.

For the purposes of selecting control groups and conducting statistical tests, several different time periods must be distinguished. The selection period or the pre-treatment period refers to the period before the policy is administered. The first portion of this period, known as the calibration period, is used to identify the control group. Variables that describe conditions and growth rates within that period are the basis of selecting the control counties. The latter part of the pre-treatment, known as the selection test period, is used to conduct a statistical pre-test. It permits an explicit evaluation of the validity of the control group or, more specifically, of its ability to trace out accurately the growth path of the treated county or counties. This period stretches from the end of the selection period to just before treatment begins. Since no treatment occurred during the selection test period, the counterfactual traced out by the control group during that period should be identical to the actual. A statistically significant difference between the two suggests a selection bias, misspecification, or other problem that must be solved, or at least recognized. The treatment test period starts the year before treatment occurs. A treatment effect is identified if the actual and the counterfactual diverge during this period, and their difference, is statistically significant.

The figure below shows a hypothetical, ideal case.[1] There is no significant difference between the growth paths of the treated counties and the control counties during the selection period, in part, because growth rates during that period are used to select the control group. Likewise, there is no difference in the selection test period. During the treatment test period they begin to diverge. The difference between the treated path (actual) and the control group path (counterfactual) is interpreted as the treatment effect. If the hypothesis holds true, then the statistical test should support the idea that the economic development took place at a much faster rate in the treated counties than in the control counties.



The selection period for determining control counties in this research was 1970-79. That is, control counties were matched to treated counties based on economic performance during the decade of the 1970s. The matches were then tested based on similarity of performance in the period 1980-88. Four sets of matches were generated in which 1) a single control county to be paired with a treated county was selected only from states (Florida and Mississippi) that did not have credit pools, 2) a single control county was selected only from the same state of the treated county with which its was paired, 3) a single control county was selected without regard to its state or the state of the treated county with which it was paired, and, 4) a composite control county was synthesized from the five counties in the entire pool that most closely matched a treated county.

Control counties were selected based on a Mahalanobis ranking.[2] In the case of single county controls, the treated county was paired with the county that had a Mahalanobis ranking of one (because its Mahalanobis distance was minimum). However, the process of optimal matching as it may be recalled, picks up the second ranked county as the best twin for a treated county, if it's first ranked county has already been matched to another treated county in the process. Consequently, in such situations a particular treated county tends to have a little less than 'best' fitted match although the process of optimal matching has a built-in mechanism of assigning matches so that the entire group of twins is as much like the entire group of treated counties as possible. Accordingly, it was thought that the best control was a synthesized composite county which averages the economic variables in the top five-ranked matches for each treated county. Hence, the empirical results reported here were obtained only using a composite control county.

Table 1 lists the different variables that were used to run the matching procedure. Data on these variables for both the groups of counties were obtained from various secondary sources. The Previous Growth variables—"Total income growth rate" (INCGR) and "Total population growth rate" (POPGR) were calculated based on the data collected from the U. S. Census of Bureau publication—*USA Counties CD-ROM 1994* and *1996.* This was also the source of data for all the Economic Structure and Spatial Structure variables except for "Proximity to metropolitan area" (PMSA). Data for this variable were obtained from *City and County Data Book 1983* and *1994.* However, the Regional Economic Information System publications also had to be used to upgrade the data for the years 1994 and 1995.

The county matches are based on 1979 economic conditions and 1970-1979 growth rates, so that similarity is measured several years before the beginning of the treatment. The income data for the different sectors of the economy are usually reported in terms of millions of dollars. Since we are dealing with only certain key sectors of the economy, these income figures had to be converted into percentages of the total income so that parity is maintained when calculating differences in growth rates for each pair of counties for the different variables.

Residential adjustment is treated differently than the other categories because it can assume negative values for counties that have a net outflow of commuters' income.

This property creates a problem when doing the impact calculations because the growth rate of a negative number does not make intuitive sense. For instance, a residential adjustment of -$1 million in one year and +$2 million the next implies a rate of change of -300% although income actually increased. A decline from -$1million to -$2 million yields a rate of +100%.

Because of this problem, both the treatment effect on residential adjustment and the statistical significance tests of the effects are calculated using changes in residential adjustment's share of earned income. (Rephann, 1993) The procedure assumes that, had there been no treatment, the treatment county's residential adjustment share would have changed by the median change for the control counties or by the same amount as the matched county (Note however, that residential adjustment factor was not used in the model with synthesized composite counties serving as controls).

The Mahalanobis distance metric was used to combine the variables and identify the control county that is closest to being a "twin" of the treated county. The choice of metric is, in essence, the choice of the weight to assign to each variable. The Mahalanobis metric is preferred here as it has several desirable features. It creates a single summary index, it gives added weight to variables that have less variation in the data, it measures similarity as a continuous variable, and it has long been used in geographical research (King, 1969). Appendix Table 1 shows the pairing of treated counties with the counties used to synthesize a control match.

Evaluation of Control Set

The optimal matching procedure using the Mahalanobis metric suggests that the control set is the best counterfactual proxy that can be obtained from the reservoir of counties from which control matches were drawn. A more rigorous statistical evaluation of that proposition is possible, however. If the control group is a good proxy for hypothetical treated county growth in absence of loans from state credit pools after 1989, it should also closely track economic growth in the control counties in the period between 1979 and 1988. Consequently, we have tested the hypothesis that the mean difference of the rates of growth for all pairs of treated and composite control counties is zero prior to the treatment. The results of those tests are shown in Table 2.

Examination of Table 2 shows that personal and per capita income grew somewhat slower in the counties that later obtained credit pool loans than did their control composite in both the 1979-84 and 1984-88 periods, but that difference in growth rates was statistically significant at the five percent level only with regard to per capita income. Population growth was slightly faster in the counties that obtained such loans later, but like the differences in total personal income growth, the differences in population growth rates were not significantly different.

At a more disaggregated level, significantly slower growth rates in earnings from trade and services were observed in 1979-84 in the treated counties, and from transfer payments, dividends, interests, and rents throughout the 1979-88 period. However, as desired, the Hotelling T^2 test statistic for the global significance is not statistically significant, and hence, the control group made up of synthesized composite counties is deemed to be an acceptable counterfactual match for examining the impact of credit pool loans on counties.

3. Empirical Results

Performance Relative to Synthesized Composite Control County

Table 3 shows the results of statistical tests of differences in growth rates between treated and control counties in the post-test period after the treated counties obtained loans from state credit pools. These results show that there is statistically significant evidence that after 1990, total personal income grew faster in counties that obtained loans for water and sewer infrastructure than in the synthesized composite control counties with which each was paired. There is some evidence, albeit it not statistically significant, of a slightly faster rate of population growth in the treated counties in the post-test period. That population growth appears to have been sufficient to have negated any positive impact of the infrastructure investment on growth in per capita incomes in the treated counties.

There are only a few bits of statistical evidence of an impact of state credit pool loans on earnings by sector in treated counties. In the three years, 1993, 1994, and 1995, income from transfer payments, dividends, interests, and rents increased faster in the treated counties than in the composite county with which they were paired for control. This finding is of special interest because this source of income is unearned, and increased growth in income from this source may not be directly due to the investments in water and sewer infrastructure financed by loans from state credit pools. One possible reason could be that these counties were attracting population in the age group with a high proportion of retirees and/or real estate owners.

Comparison of the performance of the treated counties relative to their control counties in the pre-test period (Table 2) and in the post-test period (Table 3) may be of interest, even if the differences in growth rates are mostly not statistically significant. In the period, 1979-88, before receiving loans from state credit pools, the treated counties had slower rates of growth than their controls in earnings from manufacturing, trade, and services, and transfer payments, dividends, interests, and rents. After receiving the loans, the treated counties had slightly higher average rates of growth in earnings from all sectors, except transport. The changes are not statistically significant, but there is some weak evidence of a turn around in the treated counties in earnings growth after receipt of the state credit pool loans.

Performance Relative to Other Control Alternatives

In addition to the test reported above where the controls were synthesized composite county controls, differences in performance between the treated counties and alternative controls were also tested. We will only summarize those results here, appending the detailed statistical results for those who care to examine them.

When the control groups are drawn from states that do not have state credit pools (Florida and Mississippi), the evidence is counter to the hypothesis that credit pool loans have spurred economic growth. Compared to their controls, the counties receiving such infrastructure loans actually had a decline in per capita incomes after having the loans funded, mostly due to relatively high population growth.

Compared to controls drawn solely from within the state where the treated county is located, however, there is statistical significance that counties receiving such loan funds had faster growth in per capita incomes in 1993-95. Selecting controls from within the same state holds some state-peculiar institutional variables constant, and hence, the test results are interesting. But given the fact that decision-making regarding the approval of state credit pool loans includes political factors in every state, what appears to be a positive impact of credit pool loans may be nothing more than the result of a county, or groups of counties, having political clout in the state capital.

The third alternative tested post-treatment differences in economic performance when treated counties were matched with a control county drawn from a pool of all non-metro counties in all eight southeastern states. Since this control pool is a combination of the pools used in the two previous alternatives, one might expect the results to be something between what was obtained with those alternatives. There was statistically significant evidence of faster growth in total personal income for counties using pools in the single year, 1991, and of faster growth in per capita income between 1993-95.

4. Summary and Conclusions

Summary of Results

In this study, we have made use of the quasi-experimental technique to evaluate the impact of state credit pool loans for water and sewer infrastructure on the economic growth of rural southern counties. Widely used statistical techniques for choosing similar counties not obtaining such loans were used to construct alternative controls for each of the treated counties. The contemporary economic performance of those paired controls was used as the counterfactual quantitative benchmark for measuring the impact of credit pool loans as a quasi-experimental treatment.

The results of the tests are mixed. There appears to be some statistical association between the treatment and growth differences for the very important variable of per capita income in at least some, but not all, of the tests. Yet one cannot conclude with high certainty that the statistical association is a manifestation of a true effect of the treatment. Even if a treated county had perfectly tracked a control county in the past, there is no assurance that it would have continued to do so in the absence of the credit pool loan and infrastructure investment.

The research reported here, therefore, does not provide a definitive evaluation of the effectiveness of state credit pools as economic development tools. All conclusions must be tentative. The research does provide evidence, however, that there may be a causal relationship between state credit pool loan distributions and the geography of economic growth. It is at least an interesting possibility that needs further detailed investigation.

Implications for State Rural Development Policy

Given the tentative nature of the results, some caution is required in drawing implications from this research for state rural development policy. There is no assurance that state credit pools will generate increases in per capita income in rural counties over and above that which might have occurred in the absence of such credit pools.

Yet to the extent that state credit pools make investment capital available at lower costs than otherwise would have been the case, they foster greater investment in infrastructure at the margin. Hence, the empirical evidence uncovered here that credit pool financing is associated with relatively faster rates of growth in per capita incomes in rural counties is consistent with economic theory. The preliminary evidence, therefore, argues for the use of credit pools as part of a state rural economic development policy, particularly when such pools can be operated at relatively low administrative costs.

State credit pools are not a panacea for the economic ills of the rural South. Other factors such as education, transportation access, and local leadership cannot safely be neglected. Moreover, maximizing the effectiveness of state credit pools requires that attention be given to minimizing political influence in approving loans, establishing reasonable standards of eligibility to borrow that screen out localities that have not addressed other factors likely to play a critical role in economic development, and providing appropriate training to local officials on how to use the credit pools.

Suggestions for Future Research

In some sense, it is perhaps not surprising that the statistical evidence is mixed. Recall that one reason for establishment of credit pools for infrastructure financing is the fact that infrastructure, in itself, is not sufficient to assure economic growth. Hence, it is to be expected that some fraction of the counties obtaining infrastructure loans from state credit pools would not respond to the "treatment." Moreover, some counties not obtaining loans from state credit pools may have financed infrastructure by other means.

While we must not rule out the possibilities that other researchers might find innovative ways to use the quasi-experiment technique to do more powerful tests of the linkage between state credit pool loans and subsequent local economic growth, we believe that further work with this technique is likely to yield diminishing returns. Rather, micro-analytical studies of individual cases now seems to be appropriate. One such approach might involve doing such detailed case studies on some subset of the county pairs used in this study.

Such a research agenda will be expensive because it will necessarily involve in-community study and interviews. Doing it well would require a significant commitment of time and personnel. If a well-designed research plan were in place, however, it would be the sort of project that could do double duty in training students, while also gathering valuable research data. The tentative conclusions will continue to tease rural development specialists until such studies are performed.

Endnotes

[1] The figure is reproduced from Isserman 1994.
[2] For a definition and discussion of the properties of the Mahalanobis metric, see Rubin (1979).

Table 1. Variables Used to Match Counties

Name	Variable Description	Year
Spatial Structure		
TOTPOP	Total population	1979
POPDENS	Population density	1979
PMSA	Proximity to metropolitan area	1979
Economic Structure		
PCI	Per Capita Income	1979
FARM	Farm earnings	1979
MANUF	Manufacturing earnings	1979
TRDNSRV	Combined earnings from the Wholesale, Retail and Service sectors	1979
TRANSP	Transportation and public utilities earnings	1979
TRNSFRINC	Combined earnings from Dividends, Interest & Rent (DIR) and Transfer Payments sectors.	1979
GOVTINC	Combined earnings from State & Local Govt. Federal Civilian and Federal Military sectors.	1979
RESADJ	Residence Adjustments (earnings from outcommuters residing in the county less incommuter earnings)	1979
Previous Growth		
INCGR	Total income growth rate	1970-79
POPGR	Total population growth rate	1970-79

Table 2: Pre-test Results—Mean Growth Rate Differences, Treated Counties and Their Composite Twins

	From 1979 to:	
	1984	1988
TOTAL PERSONAL INCOME	-1.95	-1.02
TOTAL POPULATION	0.38	0.40
PER CAPITA PERSONAL INCOME	**-2.54***	**-2.52***
Earnings by sector:		
FARMING	1.10	1.22
MANUFACTURING	-0.74	-1.47
TRDNSRV	**-2.73***	-0.45
TRANSPORT	0.29	0.07
TRNSFINC	**-3.57***	**-2.57***
GOVTINC	1.30	0.37
Global Significance - Overall fit of the Match:		
Hotelling T^2	2.25	
F-test statistic	2.37	

* indicates significance at the 5 percent level.

APPENDIX

Appendix Table 1: Treated County Matches Using Synthesized Composite Control Counties

Treated Counties	Composite Counties	Control Counties
Barbour_al	Composite county 1	Pike_al Colleton_sc Pickens-al Copiah_ms Attala_ms
Bullock_al	Composite county 2	Burke_ga Warren_ga Dooly_ga Greene_al Webster_ga
Cleburne_al	Composite county 3	Bibb_al Hickman-tn Coosa_al Meriwether_ga Marion_ga
Covington_al	Composite county 4	Escambia_al Monroe_ms Beaufort_nc Laurens_ga Marengo_al
Cullman_al	Composite county 5	Harnett_nc Greene_tn Wilkes_nc Rutherford_nc Putnam_fl
Marion_al	Composite county 6	Lauderdale_tn Tallapoosa_al Barnwell_sc Scott_ms Montegomery_nc
Randolph_al	Composite county 7	Jasper_ms Calhoun_ms Madison_fl Clay_al Newton_ms
Wilcox_al	Composite county 8	Amite_ms Chickasaw_ms Sumter_al Noxubee_ms Chocktaw_al

Appendix Table 1: Treated County Matches Using Synthesized Composite Control Counties (Continued)

Treated Counties	Composite Counties	Control Counties
Winston_al	Composite county 9	Lauderdale_tn Barnwell_sc Montegomery_nc Banks_ga Warren_tn
Bulloch_ga	Composite county 10	Pike_al Sumter_ga Yazoo_ms Macon_al Franklin_tn
Gordon_ga	Composite county 11	Warren_tn Hart_ga Person_nc Lauderdale_tn Macon_tn
Jefferson_ga	Composite county 12	Anson_nc Meriwether_ga Greene_ga Pickens-al Warren_nc
Lamar_ga	Composite county 13	Jackson_ga Hart_ga Yancey_nc Chatooga_ga Mercer_ky
Murray_ga	Composite county 14	Henderson_tn Pontotoc-ms Lewis_tn Gilmer_ga Jeffdavis_ga
Polk_ga	Composite county 15	Stanly_nc Chatooga_ga Jefferson_tn Jackson_ga Person_nc
Pulaski_ga	Composite county 16	Caldwell_ky Morgan_ga Dooly_ga Nicholas_ky Bleckley_ga

Appendix Table 1: Treated County Matches Using Synthesized Composite Control Counties (Continued)

Treated Counties	Composite Counties	Control Counties
Taylor_ga	Composite county 17	Marion_ga Warren_nc Saluda_sc Talbot_ga Jones_nc
Anderson_ky	Composite county 18	Mercer_ky Garrad_ky Pike_ga Trimble_ky Houston_tn
Bell_ky	Composite county 19	Marion_ky Mitchell_nc Rockcastle_ky Toombs_ga Pike_ms
Boyle_ky	Composite county 20	Stephens_ga Lee_nc Clarke_al Tift_ga Pasquotank_nc
Breathitt_ky	Composite county 21	Wayne_ms Rockcastle_ky Fentress_tn Jefdavis_ms Franklin_fl
Casey_ky	Composite county 22	Wayne_ky Leake_ms Cumberland_ky Clinton_ky Ashe_nc
Clay_ky	Composite county 23	Rockcastle_ky Wayne_ms Cumberland_tn Fentress_tn Wayne_ky
Fleming_ky	Composite county 24	Metcalfe_ky Montegomery_ms Edmonson_ky Marion_ky Jefdavis_ms

Appendix Table 1: Treated County Matches Using Synthesized Composite Control Counties (Continued)

Treated Counties	Composite Counties	Control Counties
Graves_ky	Composite county 25	Lincoln_tn Giles_tn Weakley_tn Dyer_tn Beaufort_nc
Grayson_ky	Composite county 26	Overton_tn Prentiss_ms Tippah_ms Mcnairy_tn Wayne_ky
Henry_ky	Composite county 27	Garrard_ky Trimble_ky Bracken_ky Saluda_sc Bleckley_ga
Letcher_ky	Composite county 28	Lincoln_ky Rockcastle_ky Franklin_ga Knox_ky Benton_tn
Monroe_ky	Composite county 29	Wayne_ky Ashe_nc Truetlen_ga Clinton_ky Overton_tn
Simpson_ky	Composite county 30	Taylor_ky White_tn Evans_ga Jeffdavis_ga Aleghany_nc
Pender_nc	Composite county 31	Cheroke_al Chilton_al Worth_ga George_ms Pearlriver_ms
Jasper_sc	Composite county 32	Crenshaw_al Macintosh_ga Macon_nc Fannin_ga Breckinridge_ky

Appendix Table 1: Treated County Matches Using Synthesized Composite Control Counties (Continued)

Treated Counties	Composite Counties	Control Counties
Grundy_tn	Composite county 33	Morgan_tn Yancey_nc Bledsoe_tn Lewis_ky Jackson_tn
Lawrence_tn	Composite county 34	Henry_tn Lamar_al Hardeman_tn Hardin_tn Mcnairy_tn
Monroe_tn	Composite county 35	Clarendon_sc Morgan_tn Marshall_ms Cocke_tn Pickens-al

Appendix Table 2: Post-Test Results of Mean Growth Rate Differences, Treated Counties and their Twins, Controls from State Without State Credit Pools

	From 1989 to:					
Variables	1990	1991	1992	1993	1994	1995
TOTAL PERSONAL INCOME	1.37	**2.65***	1.02	1.85	1.33	1.39
TOTAL POPULATION	0.33	0.60	1.84	1.71	1.26	1.05
POPULATION DENSITY	-1.18	-0.79	0.40	1.11	0.78	0.29
PER CAPITA PERSONAL INCOME	0.70	1.67	-0.98	-0.89	-1.00	-0.99
RESIDENCE ADJUSTMENT	-0.01	1.02	1.94	0.89	1.04	0.61
Earnings by sector:						
FARMING	**2.22***	**2.59***	-1.10	-0.35	-1.05	-0.50
MANUFACTURING	-0.05	0.05	0.98	0.91	1.05	1.56
TRDNSRV	**1.96***	0.74	-0.01	-0.27	-0.77	-0.77
TRANSPORT	**-2.27***	**-2.36***	-1.13	-0.72	-1.06	-1.16
TRNSFINC	1.65	1.82	1.97	1.86	1.53	1.57
GOVTINC	**3.44***	**4.88***	**4.35***	**3.45***	**2.78***	**2.43***

*indicates significance at the 5 percent level.

Appendix Table 3: Post-Test Results of Mean Growth Rate Differences, Treated Counties and their Twins, Controls from Same State

	From 1989 to:					
Variables	1990	1991	1992	1993	1994	1995
TOTAL PERSONAL INCOME	0.29	0.91	0.65	1.84	1.87	1.83
TOTAL POPULATION	-0.04	-0.46	-0.18	0.31	0.67	0.67
POPULATION DENSITY	1.25	-0.48	-0.17	0.34	0.60	0.48
PER CAPITA PERSONAL INCOME	-0.13	0.69	0.13	**2.18***	**2.08***	**1.79***
RESIDENCE ADJUSTMENT	-0.49	-0.69	-0.91	-0.85	-0.85	-0.93
Earnings by sector:						
FARMING	-0.12	0.68	0.71	1.09	0.49	1.50
MANUFACTURING	0.12	0.35	1.75	1.70	1.55	1.72
TRDNSRV	0.21	0.82	-054	0.72	1.51	1.46
TRANSPORT	0.49	-1.38	-1.17	0.61	0.52	0.01
TRNSFINC	0.52	-0.69	-0.24	1.95	**2.06***	1.94
GOVTINC	-0.13	0.09	0.54	0.93	0.97	1.05

*indicates significance at the 5 percent level.

Appendix Table 4: Post-Test Results of Mean Growth Rate Differences, Treated Counties and their Twins, Controls from All States

	From 1989 to:					
Variables	1990	1991	1992	1993	1994	1995
TOTAL PERSONAL INCOME	0.61	**2.09***	1.46	1.84	1.88	1.63
TOTAL POPULATION	0.23	0.31	0.46	0.56	0.79	0.52
POPULATION DENSITY	-0.26	-0.19	-0.23	0.07	0.34	0.18
PER CAPITA PERSONAL INCOME	0.33	1.44	1.02	**2.85***	**2.86***	**2.27***
RESIDENCE ADJUSTMENT	-0.69	-0.61	-0.73	-0.21	-0.25	-0.40
Earnings by sector:						
FARMING	1.54	1.47	1.32	1.29	1.21	1.02
MANUFACTURING	-1.05	-0.79	-0.04	0.18	0.18	0.36
TRDNSRV	1.33	1.29	1.29	1.81	1.98	**2.29***
TRANSPORT	0.39	-0.55	-1.54	0.31	0.34	0.23
TRNSFINC	0.13	0.05	-0.54	1.26	1.51	1.33
GOVTINC	0.07	1.13	0.85	1.01	0.97	0.96

*indicates significance at the 5 percent level.

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