THE ECONOMIC IMPACTS OF THE PRISON DEVELOPMENT BOOM ON PERSISTENTLY POOR RURAL PLACES

by

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Abstract: Prison construction was a noticeable component of economic development initiatives in rural places during the 1980s and the 1990s. Yet, few comprehensive ex post-empirical studies have been conducted and therefore the literature remains inconclusive about the economic impacts of prisons. Following a temporal overview of the geography of prison development and associated characteristics, this research employs quasi-experimental control group methods to examine the effects of state-run prisons, constructed in rural places between 1985 and 1995, on county earnings by employment sector, population, poverty rate, and degree of economic health. Our analysis suggests that prisons have had no significant economic effect on rural places in general, but that they may have had a positive impact on poverty rates in persistently poor rural counties, while also associated with diminishing transfer payments and increasing state and local government earnings in places with relatively good economic health. However, we found little evidence to support the conclusion that prison impacts were significant enough to foster structural economic change.

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INTRODUCTION

Many rural areas have persistently high poverty rates and have continuously lagged behind the rest of the nation economically. Few economic development strategies have been shown to ameliorate this trend. Therefore, rural communities seeking alternatives to improve their situation have often turned to recruiting enterprises that are considered the least desirable elsewhere, such as prisons (Thies 2000). Rural prison facilities, although smaller than their urban counterparts, are generally large in comparison to base populations in rural places (Beale 1996). Hence, in most instances it is expected that prison construction and subsequent operations will create relatively significant direct impacts through prison employment and indirect impacts by way of regional multiplier effects. However, empirical evidence that demonstrates the economic effects of prison sitings in rural places is sorely lacking, and moreso is evidence in support of prisons' impacts on economically distressed places and their resident poverty populations.

The lack of analysis is due in part to the fact that measuring the economic effects of prison development is made difficult by varying pre-existing characteristics of places and the regions in which they are situated, such as the degree of economic diversity within the economy and the extent of inter-industry and inter-regional linkages. In conjunction, it is understood that the characteristics of the prison facility must be taken into consideration; the composition of facility expenditures, including number of employees and related skill levels and wages rates, all need to be evaluated. Yet, many argue that even if attempts to capture such factors are made, as when seeking to generalize impacts via an input-output analysis or conjoined econometric analysis, those efforts are fruitless because prisons are economic "islands" (Rephann 1996). That is, they are isolated from their host communities in both their hiring and expenditure practices, offering little beyond a physical presence that serves as a negative externality in attempting to attract other enterprises to the area (Carlson 1992; Shichor 1992).

Compositional characteristics of such investment often limit their economic development impact. For instance, prison employment policies and educational requirements often result in administrative and corrections positions being filled from within the corrections system by transfers of individuals from outside the community. These employees tend to commute to work rather than relocate (Beale 1993, 1996; Fitchen 1991). Job opportunities for area residents are typically comprised of clerical positions and skilled service work. These jobs tend to pay significantly lower wage rates than the more skilled, professional jobs. Thus, prisons are not thought to have much impact on welfare and poverty populations; in general, high-skilled jobs are taken by individuals originating outside the local community while the prison population itself provides ample low-skilled labor to fulfill menial tasks. Prisoners accumulate records of good behavior as part of their work details, earn nominal compensation for certain tasks, while receiving some level of training (Fitchen 1991). Additionally, most of the prisons constructed in rural places over the last several decades are owned and operated by the state or federal government or national private industry, therefore prison inputs and purchasing networks are at the national scale (Sechrest 1992). These factors suggest that not only do few jobs go to residents, but also the indirect benefit of the prison as an economic activity is limited, as substantial forms of leakage may go hand in hand with prison development.

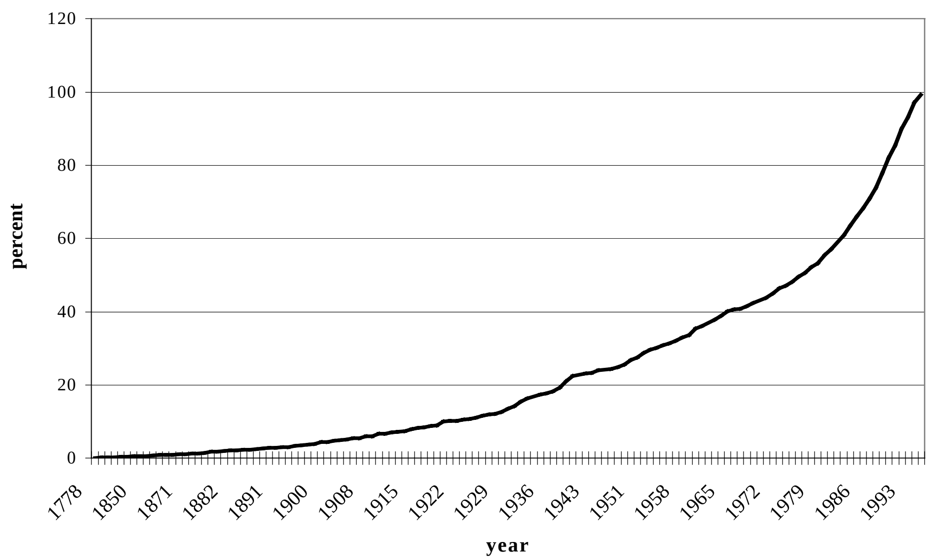
Yet, despite the potential limitations in the benefits derived locally from prisons, many rural places took advantage of the opportunity to bid for and acquire a prison during the prison-building boom that began in the 1980s and lasted through the late 1990s (Beale 1993, 1996; Beck and Harrison 2001). Laws such as “three strikes and you’re out” and longer and more severe prison terms for first time drug-related offenses led to an exponential increase in state level prison populations. Some states, and regions within them, made prison location into a business. Descriptive analysis of prison locations over the last two decades clearly show that in some regions of the country, prison siting became a target of development opportunity. The purpose of this analysis, therefore, is to provide a measure of impact of state prisons, developed at the height of the boom (1985–1995), on diverse rural places, particularly those with high levels of poverty and economic distress prior to prison construction.

This paper includes two parts. The first part of the paper describes the spatial and temporal distribution of prisons in the United States. A unique data set was created that allows for the tracking of prison construction in the U.S. to 1995 and the examination of the characteristics of those prisons in terms of the type of facility, its location, attributes of the prison population, and conjoint location of prison facilities. Thus, the first part provides a detailed snapshot of the spatial economic of incarceration in the United States. The second part of the paper presents an analysis of the effect of prison construction on county-level economic conditions. In this analysis we emphasize the effect such investments have on populations at risk as measured by changes in the poverty population and area-wide economic health in matching counties. In doing so, we first report on the theoretical models applied in order to make explicit the main assumptions of the research project and to relate the methods to the reader on a higher level of abstraction than is traditionally the case in the reporting of impact assessment (Becker and Porter 1986).

PART I: THE SPATIAL & TEMPORAL DISTRIBUTION OF PRISONS IN THE U.S.

The growth in the number of federal, state, private, and joint local authority prison facilities in operation in the United States as of 1995 is described with reference to three distinct periods of spatial development: pre-1945, 1945–1984, and 1985–1995. All are illustrated by year of original construction in Figure 1, where the acceleration in prison sitings over time and periodic shifts are made evident by the change in the slope of the line. This is particularly clear for the latter period, during what has become known as “the prison development boom”, which is represented at the tail end of the graph, from about the 60th percentile on.

Figure 1. Cumulative Percent of Prisons by Year of Construction: 1778–1995



Prison Facility Location. Nearly 36 percent of the prisons in operation in 1995 were constructed from 1985–1995, with an average of 48 new prisons per year for the 11-year period. As in the prior two periods, the majority of those prisons were constructed in the South, mainly in South Atlantic states, such as North Carolina prior to 1945 and in Florida from 1945–1995. However, the pattern of growth from 1985 to 1995 shifted substantially from the South Atlantic Division of the South to the West South Central, concentrating in Texas with 14.4 percent of the growth for that period (more than double that of any other state). These and other dominant patterns of prison construction, including a boost in prison sitings in California from 1945–1984 and in Michigan from 1985–1995, can be seen by comparing Figures 2–4

Among the nearly 1,500 prisons included in our data set, 39 percent or 576 facilities are located in rural places as currently defined,[1] mainly in North Carolina, New York, and California (see Figure 5). Of that 39 percent, 36 percent were constructed from 1985–1995 alone and are somewhat concentrated in Michigan (9%), New York (7%), Texas (6%), and Connecticut (6%). Rural prisons constructed in earlier periods are more highly concentrated and located in southern places. For instance, the highest percentages go to Virginia (10.5%), Florida, and California (10.3% each) for 1945–1984 and to North Carolina (21.5%) for pre-1945 construction. Therefore, the older the rural prison, the greater the tendency for it to be located in the Southeast, while newly constructed rural prisons are more widely dispersed across the United States. Facilities in urban places have a similar geography for the earlier periods, but the more newly constructed the urban prison the more likely it is to be located in Texas. Texas went from housing 3.8 percent of all new urban prison locations for 1945–1984 to 19.6 percent of those constructed during the prison development boom, while no other state showed similarly significant growth in urban prison sitings from 1985–1995.

Prison Facility and Population Characteristics. Beyond the patterns of change in the location of newly constructed prison facilities in the United States, there has been little change in their character. For instance, prisons in the U.S. have historically been and are currently dominated by state-operated, minimum-security facilities that are authorized to house male inmates only. These characteristics were jointly held by 29 percent of the total number of prisons in operation in 1995, which were predominately located in North Carolina (10.4%). However, it should be noted that although new prison sitings during the boom were almost entirely state-run (80%), there were a slight shift with respect to past periods toward the construction of federal

[1] Here we use 2000 Census Bureau definitions of urban areas and urban centers to identify rural places (in 2000 the Census Bureau changed the urban designation from a place-based definition to one using these concepts). In Part II,

maximum- and minimum-security prisons. There was also a greater degree of segregation by gender authorization from 1985–1995 than in the earlier periods of prison growth and a significant reduction in the percent of prisons under court order limiting the maximum number of inmates with an associated reduction in the absolute number allowed. In reference to the latter, based on dates of original construction, that number went from an average of 167 inmates per restricted facility, which was 25 percent of all prisons constructed pre-1945, to 116 inmates for the 15 percent restricted for 1945–1984 constructions, to 54 inmates for the 7 percent restricted for 1985–1995.

Overall, at the time of the count used in this study (June 30, 1995), the United States prison population was characterized by an average total of 690 inmates per facility (668 for the average annual daily population). Among them, the percentage of black/African American inmates (47%) was slightly higher than that for white inmates (41%), with very few inmates of other racial categories besides Hispanic, Puerto Rican, Cuban, or others of Spanish decent (11%). Additionally, inmates under 18 made up less than one percent of the total and male inmates outweighed female nine to one. However, there was a distinct geography of gender and race between rural and urban prison facilities, with rural prisons holding higher percentages of male and black/African American inmates than their urban counterparts in 1995. Likewise, urban prison facilities held higher percentages of female and white inmates than rural prisons. This pattern was similar regardless of regional location or age of the facility, except in the case of inmates of Hispanic, Puerto Rican, Cuban, or others of Spanish decent, whom were equally represented in urban and rural prisons, but tended to be located more so in newer urban prisons. That pattern was found to be correlated with the growth in prison construction in Texas from 1985–1995 in places currently designated as urban.

Prison Employment Characteristics. On average, prison facilities in 1995 supported 226 full- and part-time payroll staff and the highest-paying, most skill-dependent occupations (professional, correctional, and administrative) made up nearly 87 percent of those positions. There was little difference between those numbers based on urban and rural facilities, but the number of full- and part-time payroll staff did vary when combined with regional location. For instance, the average for rural facilities in the Northeast was the largest (311 staff) followed by

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for the county-based analysis, rural is defined by a subset of non-metro ERS County Typology Codes (1993 Beale urban facilities in the Northeast (288 staff). Further, the count for urban/Midwest was slightly larger than that for rural/Midwest and the opposite was true for urban and rural facilities in the South, while the average number of staff for urban facilities in the West was nearly double that of rural prisons in the West. This suggests that when average employment is used as the measure of facility size, regional location is necessary to give meaning to urban and rural distinctions.[2] Adding the dimension of time further complicates generalizations, but lends support to prior findings with regard to urban and rural differences that suggest that urban prisons tend to be larger in size than rural (e.g. Beale 1996). For instance, when looking at prisons constructed during the 1985–1995 period alone, urban facilities emerge as those with the highest average number of staff overall and in all regions except the Midwest.

Considering location and both the gender and race of prison facility staff in 1995, the greatest percentage by far was white males, as has historically been the case, but with a greater degree of gender and racial diversity in urban prisons than in rural locations. In urban prisons the highest percentage of non-male staff members were found in facilities in Nebraska and New Hampshire (28% each), while the highest percentage of non-white staff were reported for urban facilities in Colorado, California, and Texas (approximately 67% each). Once again, a correlation was found with those of Hispanic, Puerto Rican, Cuban, or others of Spanish descent, who tended to be staffed moreso in new urban prisons in Texas than elsewhere.

Looking further at occupational characteristics, first by gender and then in conjunction with location, we found that in 1995 both males and females predominately worked in corrections occupations (an average of 71% and 41%, respectively), followed by professional (9%) and maintenance (7%) for males and clerical (25%) and professional for females (20%).[3] Although grave disparities existed between male and female staff for corrections occupations when considering the percentage of total full- and part-time staff (approximately 4:1 in urban facilities and 5:1 in rural facilities), women were almost equally represented in professional

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codes 7, 8, and 9).

[2] In past prison studies, the number of employees and inmates has commonly been used together as the measure of facility size and the result has been that rural prisons were found to be smaller than urban prisons. The failure of this study to support that finding may be due to the reliance on employment as the sole measure, but it is more likely due to the fact that prior studies have also tended to rely on county-level designations of urban and rural (i.e. based on metro and non-metro county classifications) rather than place-level designations (i.e. the community rather than the county in which the facility is located) as we have here.

[3] Taken as a percent of total male and total female full- and part-time staff independently. Data by race were not available.

occupations across both urban and rural facility locations overall. Additionally, females represented a higher share of professional occupations in urban facilities in the Northeast and South as well as in rural facilities in the South.

Finally, in relation to lower-skilled (and lower-wage) jobs and their availability to the local labor market considering potential competition from prison inmates, it is interesting to note that the majority of facilities support work assignments for inmates and do so in diverse occupations, although more generally in farming/agriculture. In 1995, 93 percent reported having inmates on work assignments, with an average of 206 per facility. Considering this and the number of jobs supplied by prisons on average (226) and the percent in higher-skilled occupations (87%) in 1995, the notion that prisons provide a sufficient supply of labor internally to fill the low-skilled positions created by the facility's existence, thereby eliminating access to those jobs for area residents, seems likely. This leads the way into the second part of the paper and our primary task, which is to construct a measure of impact of prison development from 1985–1995 with a focus on persistently poor rural places.

PART II: THE IMPACTS OF RURAL STATE-PRISON DEVELOPMENT

The techniques typically used for economic impact analysis vary considerably (Pleeter 1980). Yet all impact analyses are based on the assumption that a project or treatment, such as the development of a prison in a persistently poor rural place for the sake of economic growth, triggers organizational, population group, and project environment effects and side-effects that can be assessed to some extent (Becker and Porter 1986). In this case, it is assumed that there is a causal link between the prison's activities and economic impacts and that measurable effects occur by means of direct or indirect distributive processes. This simplistic idea of cause-and-effect forms the analytical basis of impact analysis, even though scientifically, the impact of a specific development like a prison can be measured only if it is known what the situation would have been if that prison had never been constructed (Isserman and Merrifield 1987). Therefore, it has to be demonstrated that changes in the dependent variables (prison impacts) are due to the independent variables (prison operation)—accordingly, a distinction must be made between the gross and net impacts of a prison (Neubert 2000).

Preliminary Considerations. Theoretically, net impacts can be arrived at by isolating the endogenous disturbance variables—those associated with the prison operation itself—from exogenous disturbances. The effects that are extraneous to the prison. Yet realistically, clear distinctions as such are made impossible by the embeddedness of the factors of interest to social scientists in complex, heterogeneous socioeconomic and spatial structures with implicit counterfactual (unobserved) qualities that lead to logical contradictions or misleading inferences when ignored (Kaufman and Cooper 1999). Therefore, scientific research as such can only be conducted in social laboratory experiments, where hypothetical approaches to causal inference that capture counterfactual contrasts are applied.

For instance, the social experiment method, which employs experimental data (e.g. Heckman et al. 1997; LaLonde 1986), allows for the construction of a control group from a randomized subset of a potential comparison population. Other advantages are discussed at length in Bassi (1984) and Hausman and Wise (1985), but in fact, social experiments are hardly feasible and often of questionable benefit; they are expensive to implement, are not amenable to extrapolation (i.e. not easily applicable to ex-ante analysis), and are typically limited by the ruling out of spillover, substitution, displacement, and equilibrium effects by requiring that the control group be completely unaffected by the treatment (Blundell and Costa-Dias 2002). As such, social experiments are rarely used in economics and geography.

Still, if a more realistic measure of impact is to be achieved, then comparison of situations with and without the treatment (whether cross-sectional, counterfactual, or longitudinal) is the most important and comprehensible element of evaluation (Neubert 2000). Yet, comparative examination necessitates reduction to a subset of relevant information (e.g. economic properties of the prison facility and/or the location) and explicit assessment objectives, thereby producing an *estimate* of effects given definitional "if… then" statements (Davis 1990). Thus, impact analysis is best understood not as a true measure of effect, but rather as a study designed to generate quantitative estimates based on a conditional model.

Quasi-Experimental Design. Both natural experiment and quasi-experimental approaches to impact analysis are based on comparison of treatment and non-treatment groups. The natural experiment design, commonly employing the "difference in difference" method, examines the differences between the average behavioral properties of a naturally occurring control (non-treatment) group and the treatment group. This measure can be used in the absence of a genuine randomized control group as well, in a quasi-experimental format whereby comparison group selection is determined with respect to the most important variables as defined by the researcher. In this situation the selection process is dependent on two critical assumptions that make comparison group selection extremely difficult (Blundell and Costa-Dias 2002, p. 3): (1) common time effects across groups and (2) no systematic composition changes within each group.

In other words, the matching of groups for comparison is done by selecting sufficient observable factors, where sufficiency is determined by hypothetical or statistical controls, and analysis is conducted on the basis that any two places with similar factor values will not display systematic differences in behavior when faced with the same treatment. The logic behind quasi-experimental analysis, therefore, is that a control group can be selected non-randomly to serve as the baseline from which inferences about change to treated places—in this case places in which a state prison was constructed, can be made. Although control group assignment is non-random, certain aspects of a true experimental design are reconstructed in the analytical process thereby approximating a randomized trial. As such, quasi-experimental designs offer an alternative in the absence of randomized controls, but they are disadvantaged by the potential loss of internal validity through the selection process.

Quasi-experimental design presupposes that qualitative comparability exists prior to treatment and that the impacts of treatment exposure would be the same for both groups when realistically the same outcome is rarely, if ever achieved, particularly due to heterogeneous external influences (Neubert 2000). Yet, quasi-experimental designs have been shown to be "sufficiently probing…well worth employing *where more efficient probes are unavailable*" (Campbell and Stanley 1963, p. 35, author's emphasis). As such, efforts to improve evaluations of the effectiveness of policies or projects intended to foster economic change in specific places have increasingly led to the application of quasi-experimental control group approaches to impact assessment (Cook and Campbell 1979; Isserman and Beaumont 1989; Isserman and Merrifield 1982; Reed and Rogers 2001). For example, quasi-experimental designs have been used in the evaluation of regional employment subsidies (Bohm and Lind 1993), for measuring the impacts of highways in rural areas (Broder et al. 1992; Rogers and Marshment 2000), and for determining the economic effects of the Appalachian Regional Commission (Isserman and Rephann 1995), among other economic development and infrastructure investment initiatives.

Mahalanobis Metric Matching. A number of acceptable analytical alternatives exist within the quasi-experimental framework; "no one method dominates… [t]he most appropriate choice of evaluation method has been shown to depend on a combination of the data available and the policy parameter of interest" (Blundell and Costa Dias 2002, p. 32).[4] Accordingly, a variety of matching techniques are used to pair treated observations with non-treated controls based on background covariates (e.g. population and income growth rates) that are defined by the researcher based on their study relevance. However, the most extensively documented of those techniques is Mahalanobis metric matching.

Mahalanobis metric matching is commonly done by randomly ordering the treatment observations, then calculating the distance between the first treated observation and all of the control or non-treated observations, where distance $d(i,j)$, between treated observation i and control subject j is defined by the Mahalanobis distance $(u - v)^T C^{-1}(u - v)$ (D'Agostino 1998). Here, u and v are the values of matching variables for the treated subject i, control subject j, and C is the sample covariance matrix of the matching variables from the full control group. The process that follows the calculation of the Mahalanobis distance is that the control observation with the minimum Mahalanobis distance is selected to match the first ordered treatment observation. Then, both are removed from their respective groups as a match and the process is repeated until all matches to treatment observations are made.

The major limitation of this technique is that as the number of covariates included in the specified model increases, it becomes more and more difficult to find relatively close matches. This is due to the fact that the calculated Mahalanobis distance expands with the number of dimensions in the analysis and therefore the average distance between observations tends to become larger as well. The reason is that significant differences often exist between covariates of

[4] Quasi-experimental design refers to a broad research approach, consisting of a variety and often combined techniques, generally selected on the basis of the situation being examined, the factors of interest, data restrictions, and the comfort of the researcher with the form of analysis, assumptions and associated issues of validity. For review: Blundell and Costa-Dias (2002) offer a summary of matching and instrumental variable quasi-experimental methods as well as a comparison to social and natural experiments. Campbell and Stanley (1963) provide a detailed survey of the strengths and weaknesses of a collection of heterogeneous quasi-experimental, true experiment, and correlational and ex post facto designs. Also see Heckman and Hotz (1989) and Reed and Rogers (2001) for choice among methods with respect to social and economic policy impact analysis in particular.

the treatment and non-treatment group in observational studies, perfect matches rarely if ever exist; as such, the more extensive the model, the greater the likely error of comparison. These differences or errors can lead to biased estimates; therefore, they must be adjusted for in order to reduce selection bias prior to determining the treatment effect.

Reducing Bias with Propensity Scores. A measure that can easily be added to the process in order to solve the bias problem is the propensity score. This metric allows for the simultaneous matching of covariates on a single variable and is defined as the conditional probability of receiving a treatment given specified observed covariates $e(X) = \mathrm{pr}(Z = 1 \mid X)$, implying that Z and X are conditionally independent given $e(X)$ (Rosenbaum and Rubin 1985). In other words, the propensity score is a value between zero and one that represents the predicted probability of the dependent variable. If the dependent variable is a treatment group, as with this study on rural places in which state prisons were opened during the prison development boom, then the propensity score would be interpreted as the predicted probability of acquiring a prison for each case based on the combined pre-treatment characteristics of the county, as defined by the covariates selected for the study.

The propensity score can be estimated using logistic regression or discriminant analysis. The latter requires the assumption that the covariates have a multivariate normal distribution while the former does not, but the results in both cases can be used similarly to adjust the measure of the treatment effect, thereby increasing the confidence level that approximately unbiased estimates are obtained (D'Agostino 1998). However, a number of approaches can be employed to reduce bias and increase precision using propensity scores. One is a case-control matched analysis performed in conjunction with the propensity score, another is nearest available matching on the estimated propensity score, and a number of other variations exist, many of which include the Mahalanobis metric (Parsons 2000; Rosenbaum and Rubin 1984; Rubin 1979; Rubin and Thomas 1996).

Having examined a number of techniques, Rosenbaum and Rubin (1985) concluded that the nearest available Mahalanobis metric matching within calipers defined by the propensity score was the best choice. Their decision was based on the technique's balance between the covariates for the treated and control or non-treated groups in addition to a balance between the squared covariates and the cross products between the two groups. This is achieved by combining the Mahalanobis metric matching and propensity score methods, constraining the control group to a preset amount of the treated observation's estimated propensity score (D'Agostino 1998). This is explained further in the next section with reference to the current study.

Study Methodology. The major requirement in statistical matching is to preserve the marginal distributions of the variables in their original form. To satisfy this condition, constrained matching is the method most commonly used (Rodgers 1984). In this analysis selection of both treatment and control or non-treatment groups were constrained initially by sequential calipers that serve as proxies for spatial independence (Isserman and Merrifield 1987). In the case of the treatment group, rural places in either completely rural counties or counties with an urban population of less than 20,000 not adjacent to a metro area were identified. Within that pool of potential treatment counties, those with one state-run prison constructed between 1985 and 1995 were selected for analysis (*see* Table 1). The pre-treatment control group was similarly chosen, with non-metropolitan county designations meeting the same urban/rural population criteria and the lack of, or presence of "0," state-run prisons within the county boundaries.

Since economic impacts on the treatment areas' economies as a whole as well as on their poverty population were of interest, the model covariates were specified as measures of growth, industrial structure, and population and demand (Isserman and Rephann 1995; Rephann et al. 1997; Rephann and Isserman 1994). Income shares by major industries (excluding mining where data suppression is a serious problem) serve as measures of industrial structure; change rates in economic health,[5] poverty, population, and total personal income as measures of growth; and, proportions of residential adjustment and transfer income, and state and local earnings as measures of population and demand (*see* Table 2).

[5] Based on an index developed by Glasmeier and Fuellhart (1999) that incorporates measures of unemployment, labor force participation, and dependency rates.

Table 1. Prison Locations and Year of Construction in Treatment Counties

City/Town	County	State	Year	City/Town	County	State	Year
FREESOIL	Mason	MI	1985	LIMON	Lincoln	CO	1991
WINNEMUCCA	Humboldt	NV	1986	MANISTIQUE	Schoolcraft	MI	1991
WOODVILLE	Wilkinson	MS	1986	ONTARIO	Malheur	OR	1991
JASPER	Hamilton	FL	1987	PAINESDALE	Houghton	MI	1991
MCCORMICK	McCormick	SC	1987	ALLENTOWN	Lehigh	PA	1992
PINEVILLE	Bell	KY	1987	DAVISBORO	Washington	GA	1992
WESTOVER	Howard	MD	1987	DILLEY	Frio	TX	1992
BLOUNTSTOWN	Calhoun	FL	1988	DUQUOIN	Perry	IL	1992
BRISTOL	Liberty	FL	1988	INA	Jefferson	IL	1992
ELLSWORTH	Ellsworth	KS	1988	PAMPA	Gray	TX	1992
LAMBERT	Quitman	MS	1988	SAN SABA	San Saba	TX	1992
MADISON	Madison	FL	1988	SHELDON	O'Brien	IA	1992
BAKER	Baker	OR	1989	BURGAW	Pender	NC	1993
FAIRFAX	Allendale	SC	1989	HOMERVILLE	Clinch	GA	1993
LEAKESVILLE	Greene	MS	1989	ABBEVILLE	Wilcox	GA	1994
SNYDER	Scurry	TX	1989	BRECKENRIDGE	Stephens	TX	1994
WAURIKA	Jefferson	OK	1989	BROWNWOOD	Brown	TX	1994
IDABEL	McCurtain	OK	1990	CENTRAL CITY	Muhlenberg	KY	1994
JARRATT	Sussex	VA	1990	COLORADO CITY	Mitchell	TX	1994
NEW CASTLE	Weston	WY	1990	HAYNESVILLE	Richmond	VA	1994
OAKWOOD	Buchanan	VA	1990	HOLDENVILLE	Hughes	OK	1994
ROBINSON	Crawford	IL	1990	MORGAN	Calhoun	GA	1994
STANDISH	Arenac	MI	1990	ALVA	Woods	OK	1995
TRION	Chattooga	GA	1990	DALHART	Dallam	TX	1995
WEST LIBERTY	Morgan	KY	1990	RIVERTON	Fremont	WY	1995
BARAGA	Baraga	MI	1991	SAN DIEGO	Duval	TX	1995
IRON RIVER	Iron	MI	1991	TAMMS	Alexander	IL	1995
LARNED	Pawnee	KS	1991				

Earnings and population data were taken from the Bureau of Economic Analysis, Regional Economic Information System (1969–1999) and poverty rates were obtained from the Census Bureau 1970, 1980, and 1990 decennial Census and 1998 estimates. The year 1970 served as the base year for pre-test purposes, except for the growth covariates, for which the base was the absolute change rate from 1970–1980. Using this data for both treatment and potential control counties, propensity scores were estimated through logistic regression.[6] Then the distributions of the covariates were examined. Table 3 consists of descriptive statistics for the covariates and the logit of the estimated propensity score by group. The test statistic used to compare the groups was a two-sample *t*-statistic. These statistics reveal significant differences between the treatment and control groups on a number of the covariates: rate of change in total personal income (RTPI) and poverty (RPOV), proportion of farming (PFAR) and state and local government earnings (PSTL), and population (LPOP).

Table 2. County Selection Criteria and Variables Used in Empirical Analysis

TREATMENT GROUP*				
Description		**Measure**		**Year**
Rural places in 100% rural counties or counties with an urban population < 20,000 not adjacent to a metro area		spatial independence	ERS (Beale 7, 8, 9)	1995
and in counties with "1" state-run prison built 1985-1995		spatial independence	ICPSR 6953	1995
CONTROL GROUP*				
Description		**Measure**		**Year**
presence of "0" state prisons within county		spatial independence	ICPSR 6953	1995
Rural places in 100% rural counties or counties with an urban population < 20,000 not adjacent to a metro area		spatial independence	ERS (Beale 7, 8, 9)	1995
COVARIATES[a]				
RTPI	total personal income growth rate	growth	change rate	70-80
RPOP	population growth rate	growth	change rate	70-80
RPOV	poverty growth rate[b]	growth	change rate	70-80
RIND	economic health index growth rate[c]	growth	change reate	70-80
PFAR	proportion farm earnings	industrial structure	share total PI	1970
PAFF	proportion ag, fish, forestry svc earnings	industrial structure	share total PI	1970
PCON	proportion construction earnings	industrial structure	share total PI	1970
PFIR	proportion FIRE earnings	industrial structure	share total PI	1970
PMFG	proportion manufacturing earnings	industrial structure	share total PI	1970
PMIN	proportion mining earnings	industrial structure	share total PI	1970
PRTL	proportion retail trade earnings	industrial structure	share total PI	1970
PSER	proportion service earnings	industrial structure	share total PI	1970
PTPU	proportion TCPU earnings	industrial structure	share total PI	1970
PWSL	proportion wholesale trade earnings	industrial structure	share total PI	1970
PFED	proportion federal earnings	industrial structure	share total PI	1970
PMIL	proportion military earnings	industrial structure	share total PI	1970
PSTL	proportion state & local earnings	industrial structure	share total PI	1970
PRES	proportion residential adjustment	population & demand	share total PI	1970
RTFR	proportion transfer income	population & demand	share total PI	1970
LPOP	log of population	population & demand	base ten	1970
PCSL	state and local earnings per capita	population & demand	base pop	1970

*Additional counties excluded due to data issues
[a]Unless noted otherwise; data source: U.S. Dept. of Commerce, Bureau of Economic Analysis, REIS (1969-1999).
[b]Data source: U.S. Dept. of Commerce, Bureau of Census (1970 and 1980).
[c]Economic Health Index: Glasmeier, A. and K. Fuellhart. January 1999. Building on Past Experience: Creation of a new Future for Distressed Counties. Washington, DC: Appalachian Regional Commission

[6] The logistic regression was done in SAS. The SAS program for the propensity score is given in the Appendix.

The Mahalanobis metric was then calculated using a stepwise discriminant analysis. All of the significant covariates and the logit of the propensity score were included in the model. The next step was to calculate the Mahalanobis distances between the treatment and control counties. Matching was achieved by pairing a randomly selected treatment county to the closest control county, based on the Mahalanobis distance, from within a sub-set of control counties whose propensity scores fell no further than four standard deviations away from the treated county's propensity score. That match was then removed from the selection pool and the process continued until all treatment counties were matched with a control.

Table 3. Group Comparisons Prior to Matching

Variable	Treatment Group		Control Group		Comparisons	
	N = 55		N = 899		2-Sample	
	Mean	SD	Mean	SD	T-Stat	Sign
RTPI*	0.653	0.062	0.616	0.118	-2.331	0.020
RPOP	0.091	0.118	0.077	0.137	-0.694	0.488
RPOV*	-8.195	5.967	-5.929	6.131	2.664	0.008
RIND	-0.064	0.183	-0.043	0.227	0.679	0.497
PFAR*	0.100	0.080	0.160	0.126	3.496	0.000
PAFF	0.009	0.015	0.010	0.014	0.657	0.511
PCON	0.041	0.024	0.045	0.041	0.618	0.536
PFIR	0.020	0.009	0.019	0.009	-0.929	0.353
PMFG	0.125	0.108	0.101	0.106	-1.668	0.096
PRTL	0.091	0.024	0.089	0.025	-0.382	0.703
PSER	0.087	0.028	0.080	0.044	-1.087	0.277
PTPU	0.048	0.029	0.044	0.028	-1.157	0.248
PWSL	0.023	0.022	0.023	0.016	-0.204	0.838
PFED	0.020	0.023	0.022	0.021	0.607	0.544
PMIL	0.010	0.046	0.007	0.027	-0.941	0.347
PSTL*	0.098	0.030	0.088	0.035	-2.077	0.038
PRES	0.036	0.107	0.050	0.106	0.915	0.360
PTFR	0.137	0.047	0.131	0.046	-0.912	0.362
LPOP*	4.137	0.320	4.008	0.318	-2.937	0.003
PCSL	0.284	0.114	0.266	0.106	-1.198	0.231
Logit of Propensity Score*	-0.032	0.011	-0.026	0.013	3.411	0.001
Population 1970	20147	34076	13453	20427		
Population 1980	23170	38360	15001	23911		
Index Score 1970	137.34	37.68	130.83	38.61		
Index Score 1980	130.96	34.37	127.70	35.19		
Poverty Rate 1970	27.2	13.8	23.7	11.2		
Poverty Rate 1980	19.0	8.8	17.8	7.5		

* $p < .05$

Once matching was completed *t*-tests were run on the matched counties to examine whether or not the bias between the groups had been removed. Table 4 contains the descriptive statistics and *t*-tests for the after matching comparisons. As exhibited by the calculated means and lack of significance, the matched sample covariates were relatively evenly distributed. Further evidence of bias reduction is given in Table 5, which shows percent reduction in bias for the covariates with the largest initial biases, such as the poverty growth rate (RPOV) for which the bias was reduced by 61.5 percent. Essentially, no significant differences remain, therefore the matched control group was deemed satisfactory. Hence, the quasi-randomized experiment was successfully created and the covariates could be likened to the background variables in randomized experiments.

Example Matched County. Before describing the results of our analysis, we present select characteristics of an example matched county that we will track as a way of making clearer both the procedure and the results that follow. Pender County, North Carolina was the site of the construction of an 808 inmate, medium-security prison in 1993. Prior to the construction of the prison, at the close of the pre-treatment period (1980), the county's population was 22,333 and the poverty rate was 21.3 percent. The per capita transfer payment level was $2,447 and the average earnings per job were $20,465 (both in 2000 dollars). The economic base of the county was in the farming sector, which held 20.8 percent of total full- and part-time employment in 1980. Since 1980, the following changes occurred statistically over time (absolute change from 1980 to 1999): 84 percent population increase, 7.7 poverty rate decrease, 61.4 percent per capita transfer payment increase, 2.6 percent average earnings per job increase, and 15.1 percent farming sector employment decrease. During the same period of time Pender's control county (Choctaw, OK) witnessed a 10.6 percent population decrease, a 1.7 percent decrease in the poverty rate, 36.5 percent growth in per capita transfer payments, and a 27.7 percent decrease in average earnings per job. There was also a minimal .4 percent decrease in farm employment, which was similarly the dominant employment sector in 1980.

Identification of Prison Impacts. The post-treatment period was estimated from 1980–1999 (except 1997 for index scores and 1998 for poverty rates),[7] thereby allowing for a

[7] These were the latest dates for which data were available for these measures at the time this study was completed.

Table 4. Group Comparisons After Matching

Variable	Treatment Group		Control Group		Comparisons	
	N = 55		N = 55		2-Sample	
	Mean	SD	Mean	SD	T-Stat	Sign
RTPI	0.653	0.062	0.645	0.105	-0.496	0.621
RPOP	0.091	0.118	0.116	0.128	1.098	0.275
RPOV	-8.195	5.967	-7.322	6.042	0.762	0.448
RIND	-0.064	0.183	-0.039	0.207	0.666	0.507
PFAR	0.100	0.080	0.112	0.102	0.699	0.486
PAFF	0.009	0.015	0.008	0.006	-0.377	0.707
PCON	0.041	0.024	0.042	0.030	0.181	0.857
PFIR	0.020	0.009	0.018	0.008	-1.227	0.222
PMFG	0.125	0.108	0.127	0.124	0.070	0.945
PRTL	0.091	0.024	0.087	0.026	-0.794	0.429
PSER	0.087	0.028	0.080	0.043	-1.055	0.294
PTPU	0.048	0.029	0.044	0.027	-0.693	0.490
PWSL	0.023	0.022	0.023	0.013	-0.214	0.831
PFED	0.020	0.023	0.021	0.020	0.424	0.672
PMIL	0.010	0.046	0.005	0.009	-0.816	0.416
PSTL	0.098	0.030	0.091	0.036	-1.072	0.286
PRES	0.036	0.107	0.068	0.123	1.435	0.154
PTFR	0.137	0.047	0.138	0.054	0.090	0.928
LPOP	4.137	0.320	4.058	0.286	-1.375	0.172
PCSL	0.284	0.114	0.263	0.108	-0.987	0.326
Logit of Propensity Score	-0.573	0.146	-0.608	0.153	-1.244	0.216
Population 1970	20147	34076	14192	9931		
Population 1980	23170	38360	16260	11298		
Index Score 1970	137.34	37.68	138.71	43.93		
Index Score 1980	130.96	34.37	135.16	35.91		
Poverty Rate 1970	27.2	13.8	25.6	12.1		
Poverty Rate 1980	19.0	8.8	18.3	7.6		

* *p < .05*

Table 5. Percent Reduction in Bias After Matching

	Initial Bias	Bias After Matching	Percent Reduction*
RTPI	0.037	0.008	78.2
RPOV	-2.266	-0.872	61.5
PFAR	-0.060	-0.012	79.6
PSTL	0.010	0.007	32.0
LPOP	0.130	0.080	38.6

*Percent Reduction = 100(1-(After Match Bias / Initial Bias)) maximum of five years for potential construction effects prior to the date of facility completion. A series of paired samples *t*-tests were run on the cumulative change rates for each of the covariates by group based on the following data select cases: all, economic health index rank 1 or 2 for 1980, economic health index rank 3 or 4 1980, poverty rate 1980 less than 20 percent, poverty rate 1980 greater than or equal to 20 percent, and poverty rate 1980 greater than 30 percent.[8] The "all" category test was meant to identify whether or not significant differences in the mean values of the 55 treated counties and their control counties developed during the post-treatment period. None of the factors examined were shown to be significantly different except for state and local government earnings (*see* Table 6). That is, on the average there was a noticeable difference in the positive growth in state and local government earnings between treatment counties and their controls, thereby suggesting that prison development was responsible for that difference.

The other tests were conducted in order to stratify the treatment county sample by economically distressed (index rank 3 or 4)/non-distressed counties (index rank 1 or 2) and persistently poor (poverty rate => 20%) / not persistently poor counties (poverty rate < 20%) at the start of the impact period. Significant differences were not found to exist between treatment and control counties in any of the categories, except for change in transfer income and state and local earnings for counties with an index rank of one or two in 1980; change in transfer payment income for treatment counties with a poverty rate less than 20 percent; and change in poverty rates for treatment counties with poverty rates equal to or above 20 percent in 1980. This suggests that prison impacts on economically distressed or persistently poor counties were limited to poverty rate effects for persistently poor counties. Further, counties impacted were likely not to be the most extremely poor given the lack of significant difference in change in poverty rates for counties in the 30 percent or greater range.

[8] The poverty rate category >=20% includes those counties that are in the >=30% range; otherwise all categories , both index rank and poverty rate, are mutually exclusive.

Table 6. Significance of Cumulative Growth Rates

Variable	All	Index Rank 1 or 2	Index Rank 3 or 4	Poverty Rate < 20 %	Poverty Rate >= 20%	Poverty Rate >= 30 %
	N = 55	*N = 33*	*N = 22*	*N = 30*	*N = 25*	*N = 11*
Total Personal Income	0.757	0.408	0.476	0.616	0.907	0.267
Population	0.116	0.123	0.659	0.276	0.261	0.384
Poverty	0.568	0.374	0.057	0.347	0.020*	0.115
Index Score	0.124	0.417	0.189	0.178	0.471	0.439
Farm Earnings	0.707	0.335	0.353	0.909	0.527	0.143
Ag, Fish, Forest Earnings	0.413	0.341	0.801	0.524	0.165	0.788
Construction Earnings	0.824	0.865	0.885	0.688	0.501	0.968
FIRE Earnings	0.659	0.435	0.561	0.477	0.361	0.664
Manufacturing Earnings	0.635	0.833	0.334	0.641	0.303	0.454
Retail Trade Earnings	0.155	0.109	0.851	0.482	0.165	0.823
Service Earnings	0.704	0.636	0.988	0.904	0.694	0.699
TCPU Earnings	0.289	0.393	0.543	0.868	0.164	0.811
Wholesale Trade Earnings	0.742	0.739	0.969	0.448	0.448	0.807
Federal Civilian Earnings	0.950	0.791	0.893	0.486	0.361	0.664
Military Earnings	0.566	0.092	0.357	0.443	0.217	0.374
State and Local Earnings	0.026*	0.021*	0.140	0.128	0.100	0,229
Residential Adjustment	0.567	0.401	0.330	0.468	0,698	0.770
Transfer Income	0.041	0.008*	0.972	0.001*	0,800	0.848

** p < .05*

Growth Rate Differentials. The three factors found to be significant (state and local government earnings, transfer payment income, and poverty rate) served as the basis for impact measurement by way of growth rate differentials, defined as: $D_{jt} = R_{cjt} - R_{gjt}$ where D is the growth rate difference, *c* is a treatment county (*c* = 1, …, 55), *g* is a control county (*g* = 1, …, 55), R is the growth rate measured from the base year (*b*), *j* is one of the variables under investigation (*j* = 1, … ,*k*), and *t* is the test year (Rephann et al. 1997). A subset of growth rate differentials for base period 1980 and test date 1999 (1998 for poverty) for transfer income, state and local government earnings, and poverty rates are provided in Table 7.

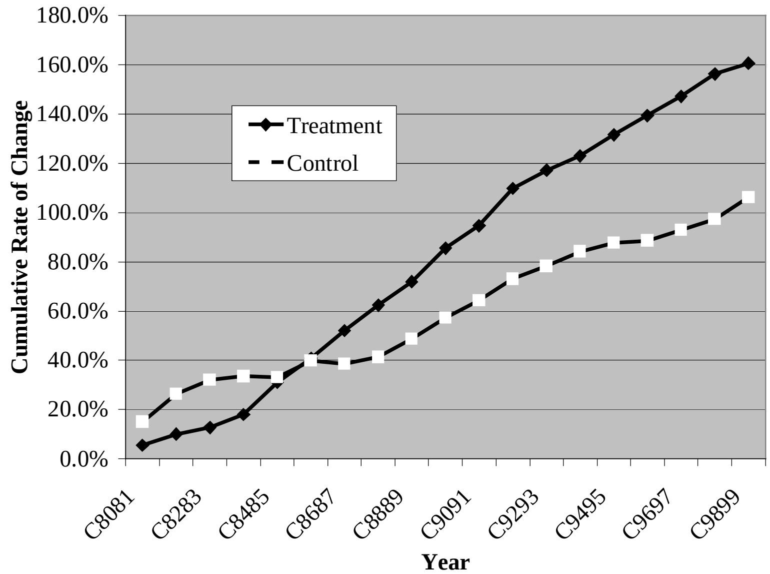
Table 7. Growth Rate Differentials; Treatment Counties; Index Rank 1 or 2 and Poverty Rate => 20; 1980–1999

	GROWTH RATE DIFFERENTIAL								
	State & Local Earnings			**Transfer Income**			**Poverty Rate**		
	Mean	**Cum**	**Point**	**Mean**	**Cum**	**Point**	**Mean**	**Cum**	**Point**
Duval, Texas [48131]	1.9%	36.6%	17.3%	1.8%	33.4%	10.2%	3.7	7.5	0.5
Washington, Georgia [13303]	1.4%	25.7%	6.5%	0.6%	12.0%	3.4%	0.2	0.5	0.0
Pender, North Carolina [37141]	2.9%	54.5%	14.5%	4.3%	82.4%	22.2%	-5.1	-10.2	-0.6
McCormick, South Carolina [45065]	1.5%	27.8%	11.9%	2.4%	46.2%	15.1%	-4.2	-8.5	-0.5
Hamilton, Florida [12047]	2.9%	55.3%	14.3%	1.7%	33.2%	8.4%	-0.4	-0.8	0.0
Madison, Florida [12079]	3.5%	66.3%	22.4%	0.8%	16.0%	4.4%	-1.4	-2.7	0.2
San Saba, Texas [48411]	2.3%	44.0%	16.3%	0.1%	1.5%	0.5%	2.0	3.9	0.2
Clinch, Georgia [13065]	0.0%	-0.7%	-0.2%	-0.4%	-7.3%	-1.8%	0.2	0.3	0.1
Mitchell, Texas [48335]	1.5%	28.2%	7.5%	-0.1%	-2.8%	-0.7%	2.5	5.0	0.2
Liberty, Florida [12077]	5.2%	99.3%	24.1%	1.1%	21.7%	5.7%	-1.0	-2.0	-0.1

*($1000)

Growth rate differentials are interpreted as the difference between treatment county growth and growth in its control county over the same period of time. Mean growth rate differentials are commonly used; here, mean (average), cumulative (additive), and point (absolute) estimates are given as alternative perspectives of impact. In reference to poverty rates, and transfer income for the most part, negative values represent a positive effect. For instance, the mean growth rate differential for poverty in Pender County, North Carolina was 5.1 percent below that of its matched county. The impact of this difference is evident in the reduction in the poverty rate in the county since 1980, from 21.3 percent to 15 percent in 1998 (model-based estimate) to 13.6 percent in 1999 (US Census 2000). However, the absolute change in poverty rate from 1980 to 1998 was comparatively minimal (-.6 percent differential). In relation, the mean growth rate differential in state and local government earnings for the same county was only 2.9 percent, but the cumulative value was relatively high at 54.5 percent. This suggests that growth may be substantial in the treatment county in comparison to its control (14.5% absolute), but potentially dispersed over time. Thus, there is a need to look more closely at the temporal factors of change, which is done graphically for this example in Figure 2.

Figure 2. Cumulative Change Rate in State & Local Government Earnings; Pender County, North Carolina; 1980–1999



The graph of cumulative growth rates in state and local government earnings for Pender County reveals that the treatment county began to outgrow its control county at the start of the prison boom, but since the date of construction for the prison was 1993, it would be misleading to attribute that growth to the prison. However, the line graph also illustrates that accelerated growth (increase in change in comparative slope) has taken place since the time of prison construction, thereby suggesting that some measure of the growth rate differential for state and local government earnings for the entire study period was likely due to the presence of the prison. If desired, a measure of that effect can be taken by multiplying the growth rate differential by the corresponding base level value. Expressed as $I_{jt} = (R_{cjt} - R_{gjt})V_{cjb}$ where I is the impact estimate, the term $(R_{cjt} - R_{gjt})$ is the growth rate differential as previously defined, and V is the value of the factor under examination, which in this case, continuing with the example, would be state and local government earnings.

The difference of this measure, taken at two periods of time, say 1989 to allow for construction effects to be captured and then again in 1999, would separate out the impact on growth in state and local government earnings potentially attributable to the prison from that which is likely due to macro trends earlier in the study period. The results presented in this study are not disaggregated in such a way (for reasons of space), but Table 8 contains impact measures for comparison across counties that had poverty rates equal to or greater than 20 percent and an index rank of either one or two in 1980 (*N* = 10). According to these estimates, the greatest impact of prison development appears to be on poverty rates in Pender, NC and McCormick, SC, where a differential reduction in poverty by approximately 5.3 percent (mean) was achieved over the study period. That represents an absolute change of 6.3 percent and 8.9 percent, respectively, given the 1998 estimate based model (7.7% and 9.0% in 1999 using U.S. Census 2000 figures).

Table 8. Estimated Impacts; Treatment Counties; Index Rank 1 or 2 and Poverty Rate => 20; 1980–1999

	ESTIMATED IMPACTS*								
	State & Local Earnings			**Transfer Income**			**Poverty Rate**		
	Mean	**Cum**	**Point**	**Mean**	**Cum**	**Point**	**Mean**	**Cum**	**Point**
Duval, Texas [48131]	193	3676	1740	291	5533	1697	4.92	0.00	0.15
Washington, Georgia [13303]	208	3951	996	140	2655	753	-0.15	0.00	0.00
Pender, North Carolina [37141]	349	6623	1767	1036	19688	5295	-5.31	0.00	0.13
McCormick, South Carolina [45065]	97	1840	784	229	4358	1426	-5.29	0.00	0.08
Hamilton, Florida [12047]	223	4241	1093	184	3490	886	3.23	0.00	0.00
Madison, Florida [12079]	335	6362	2154	161	3067	852	3.47	0.00	-0.04
San Saba, Texas [48411]	97	1844	682	7	125	44	2.15	0.00	0.06
Clinch, Georgia [13065]	-2	-30	-7	-29	-545	-133	0.88	0.00	0.00
Mitchell, Texas [48335]	98	1868	494	-19	-361	-90	2.09	0.00	0.02
Liberty, Florida [12077]	127	2422	588	61	1165	307	-0.45	0.00	0.03

*($1000)

Summary and Conclusions. The significance of state and local government earnings and transfer payment income in counties with relatively good economic health was not surprising because the index is constructed from measures of transfers, income, and employment. Further, it serves as an alternative measure of poverty. In conjunction, based on the literature, it was expected that if prisons were to have any impact at all, it would be in the state and local government employment sector in counties with a sufficiently skilled labor pool. This was true in the case of Pender County, where prior to prison construction (1990) 32.3 percent of the population 25 years and older had at least some college education, which was slightly above average for all treatment counties in the study (mean 30.2%) and substantially above average for those counties with an index rank of one or two and a poverty rate greater than or equal to 20 percent (mean 23.9%).

Likewise, 40 percent of the sample with 20 percent or greater poverty in 1980 were counties ranked number two in the index (56% ranked 3, 4% ranked 4, and 0% ranked 1). This means that only 40 percent of the persistently poor counties were among the most economically distressed. Additionally, of that 40 percent all but 4 percent were located in the south and 64 percent were in counties with some urban population. This suggests that there may be a link to a prison's ability to have a positive economic effect based on the extent of the treatment county's pre-existing economic structure and relative location, which would also challenge the claim that the prison industry economy is an "island," such that a prison's impact may be more exogenously driven locally than the literature suggests. In other words, the characteristics of the place in which the facility is located have a greater influence on the measure of effect than do the characteristics of the facility itself; however, the current analysis does not support making further inferences on that topic.

Considering economy-wide impacts, based on a diversity measure for both earnings and employment by industry sector, it appears that prisons have very little sectoral impact on the county economy; therefore, prison development is not a good way to stimulate diverse economic growth.[9] For instance, in our example county (Pender, NC) there was a minor increase in employment diversity and a somewhat similar decrease in earnings diversity from the year prior to prison construction (1992) to 1999. At the same time, the percentage of total employment in state and local government jobs went almost unchanged (17.2% in 1992 and 17.1% in 1999), while the percentage of total earnings for that sector increased (21.9% in 1992 to 24.9% in 1999). This suggests that the prison impact on the Pender County economy may not have been the creation of new jobs per se, but rather the creation of jobs with higher pay than what existed previously in that employment sector.

The results of this analysis also suggest that prisons have had a positive effect on poverty rates (i.e. a decrease), particularly in places with persistently high poverty concentrations, although this seems to be tied to a reasonable degree of economic health in the county. That is, when moving to greater extremes of poverty and economic distress, prisons had virtually no effect on the study sample of rural places. It must be recognized, however, that only a limited number of potential covariates were analyzed and that other dimensions such as migration patterns and income distributions need to be considered. For instance, looking further at Pender County alone, the average number of annual in-migrants from the year of original prison construction (1993) to 1999 was nearly one and a half times that of out-migrants (3–2) and median income of in-migrants was 13 percent greater than that of out-migrants ($21,355 to $18, 928 in 2000 dollars).[10] This may explain at least some of the poverty impact for Pender County during that time period. Yet, interestingly, when compared to 2000 U.S. Census Bureau poverty thresholds, the median income of the out-migrating population for the impact period was within the poverty-income range for a family of four while that of the in-migrating population was in the poverty range for a family of five. Therefore, although the in-flow of a higher income population to the out-flow of a smaller number/lower-income population is clear, the representation of the poverty population in those movements is not altogether obvious. As such, few inferences can be made about the statistical change in poverty in relation to migration without further disaggregation of that population by income. However, these measures do lend some support to the notion that the majority of prison jobs are filled by transfers from outside the county.

In conclusion, the economic impacts of the prison development boom on persistently poor rural places, and rural places in general, appear to have been rather limited. Our analysis suggests that prisons may have had a positive impact on poverty rates in persistently poor rural counties as well as an association with diminishing transfer payments and increasing state and local government earnings in places with relatively good economic health. However, based on the number of significant covariates for the study sample and the size of the growth rates for individual counties in comparison to their matches, we are not convinced that the prison development boom resulted in structural economic change in persistently poor rural places. It may be more the case that the positive impacts found to exist are simply attributable to spatial structure, that is, due to the mere existence of a new prison operation in a rural place rather than the facility's ability to foster economy-wide change in terms of serving as an economic development initiative.

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[9] The diversity measure is based on the Herfindahl Index ($H=S_1^n(share_i)^2$), which is used here as one minus the sum of the squares of the employment shares of all the employment sectors of the economy and similarly for earnings.
[10] Derived from the Internal Revenue Service, Statistical Information Services, Office of the Statistics of Income Division, *County-to-County Migration Data* (1980–1981, 1983–1984, through 2000–2001 data series).

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APPENDIX

SAS Program for the Propensity Score

```
proc logistic data = pretreat nosimple;
model quasi = RTPI RPOP RPOV RIND PFAR PAFF PCON PFIR PMFG PRTL PSER
PTPU PWSL PFED PMIL PSTL PRES PTFR LPOP PCSL/selection = stepwise;
output out = preds pred = pr; run;
```