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**Do Agricultural Preservation Programs  
Affect Farmland Conversion? Evidence from a  
Propensity Score Matching Estimator**

*by*

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# **Do Agricultural Preservation Programs Affect Farmland Conversion?**

## **Evidence from a Propensity Score Matching Estimator**

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**Abstract:** More than 124 governmental entities concerned about suburban sprawl and farmland loss have implemented farmland preservation programs preserving 1.67 million acres at a cost of \$3.723 billion. Yet little analysis on these programs' effectiveness in slowing farmland loss has been conducted. Using a unique 50-year 269 county panel data set on preservation programs and farmland loss for six Mid-Atlantic States, we employ the propensity score matching method to find strong empirical evidence that these programs have had a statistically significant effect on the rate of farmland loss. Preservation programs on average decrease the rate of farmland loss by 3 percentage points; a 43% decrease from the average rate of 7.31%.

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## **Do Agricultural Preservation Programs Affect Farmland Conversion?**

Concerns about the loss of farmland and the increase in suburban sprawl led states and counties to institute programs to arrest or slow farmland conversion. Beginning in 1978, farmland preservation programs such as purchase of development rights/purchase of agricultural conservation easements (PDR/PACE) and transfer of development rights (TDR) have been established and funded to retain agricultural land. These programs usually attach an easement to the property that restricts the right to convert the land to residential, commercial and industrial uses in exchange for a cash payment and/or tax benefit. Farmland preservation programs are justified on various grounds including efficient development of urban and rural land, local and national food security, viability of the local agricultural economy, and the protection of rural and environmental amenities [16, 23].

More than 124 governmental entities<sup>1</sup> have implemented farmland preservation programs [3, 6, 7] and over 1.67 million acres are now in preserved status. Spending in both state and local programs to purchase these rights was \$3.723 billion [6, 7]. Citizens continue to pass ballot initiatives generating funds for these types of programs: in 2002, \$5.7 billion in conservation funding was authorized; in 2001, \$1.7 billion; and in 2000, \$7.5 billion, and most recently in 2006, \$5.73 billion [25]. And in the last decade, the federal government has provided financial assistance for state and local purchase of development rights programs to preserve agricultural land. While some evidence exists that these programs provide net benefits to society [15,14], little evaluation has been conducted on their effectiveness in retaining farmland. Several studies have evaluated the impact of (non-permanent) use-value or preferential taxation programs [9,17,27,33,22] on farmland conversion, yet few have studied the impact of the permanent easements conferred by the PDR/PACE and TDR programs, even though several studies have

suggested that the more expensive PDR/PACE programs have preserved too little land and that the TDR programs have preserved too little or the wrong “type” of farmland [30,29, 28, 1].

Assessing the impact of permanent preservation through PDR/PACE and TDR programs on the rate of farmland loss can be challenging. One cannot construct the proper counterfactual, i.e. one would like to know what would have happened to the rate of farmland loss in county A if it had not implemented a program. However, county A can not be in two states simultaneously, nor can a researcher randomly assign who has a preservation program and who does not. Given this, one issue in assessing the treatment impact is whether the program’s existence is endogenous. For example, Lynch and Carpenter [27] found no impact of PDR/PACE and TDR on the farmland loss rate assuming that the programs’ existence was exogenous. However, farmland preservation programs may be established in those counties with the highest rates of farmland loss and/or lower levels of farmland thus the very existence of the program may be predicated on the rate of farmland loss. Another concern is that knowing the acres preserved by a program, however permanent, is insufficient to assessing its impact on farmland loss. McConnell, Kopits, and Walls [31] find that preserving a large amount of farmland through a TDR program does not guarantee a decreased rate of farmland loss if the new housing developed with the TDRs occurs in rural areas on farmland. Similarly, recent evidence suggests that the positive amenities generated by these preservation programs may increase the demand for housing near the preserved parcels. This demand then can create more conversion pressure and higher housing prices. For example, Roe, Irwin, and Morrow-Jones [37] find that preservation efforts could induce further residential growth in areas with short commutes to employment centers and small amounts of remaining farmland. Geoghegan, Lynch and Bucholtz [18] and

Irwin [24] found that housing prices adjacent to preserved parcels can increase due to the permanency of adjacent open space.

We suggest we can overcome some of the empirical difficulties by using a propensity score method to estimate the treatment effect. This method has several benefits – first, the matching protocol ensures that the counties with farmland preservation programs will be matched to the counties without programs that are most similar to them in terms of characteristics and therefore dissimilar counties and outliers will have no/little influence on the treatment impact. Second, it does not assume that the treatment status is exogenous, i.e. not all counties are equally likely to have farmland preservation programs and some of the pretreatment covariates may influence the existence of such a program. Third, we can look at effectiveness by examining rates of farmland loss rather than by examining rates of preservation.

Using a unique 50-year 269 county panel data set on the existence of PDR/PACE and TDR programs and farmland loss for six Mid-Atlantic States, we find strong empirical evidence that these programs have had a statistically significant effect on the rate of farmland loss. Our results suggest that such programs can have an impact on farmland retention.

### **Model**

In a competitive land market, risk-neutral landowners seek to maximize the economic return from their land given the stream of net returns. Ricardian theory states that the profitability of agricultural land is based on fertility or soil characteristics and this fertility determines the land rent an agricultural producer would pay. Von Thunen, Mills and others proposed that the stream of benefits of living/farming at a particular location relative to the central business district determines the rent a person would pay. Hardie et al. [20] combine the Ricardian and Von Thunen models and find that the market values of parcels in suburban

counties are the sum of the Ricardian rent and the location or accessibility rent. Following along the line of Capozza and Helsley [10], Plantinga and Miller [35] and Plantinga, Lubowski, and Stavins [34], we model land values at the urban fringe as having an expected net agricultural value and a potential development value. This expected stream of rents and potential gains determines the market price per acre ( $P_i$ ) of the parcel  $i$ . The market value is thus the expected sum of agricultural rents,  $A_i$ , from time  $t=0$  up to an optimal conversion date  $t^*$ , at which time the land is converted into a residential use with expected sum of net returns of  $R_i$  as shown in equation (1).<sup>2</sup> Agricultural and residential rents are a function of  $X_i$ , the characteristics and location of the land, and  $s$ , time. The discount rate is  $r$ .

$$(1) \quad P_i(t) = E_t \left[ \int_{t=0}^{t^*} A_i(X_i, t) e^{-r(t)} dt + \int_{t^*}^{\infty} R_i(X_i, t) e^{-r(t)} dt \right]$$

Assuming the land is in an agricultural use at time  $t$ , expected net agricultural rents are greater than expected net residential rents. However, agricultural rents are expected to grow more slowly than net residential rents. Thus to maximize the return from the land, a landowner will set the optimal conversion date  $t^*(X_i)$  such that the expected net returns to agriculture and net returns to residential uses are equal;  $E_t \{A_i(X_i, t) - R_i(X_i, t)\} = 0$ . Say  $L(t^*(X))$  is the number of acres having characteristics  $X$  with optimal time to convert  $t^*$ . The amount of land leaving an agricultural use at time  $t$  is  $L(t^*(X))$ .

In some counties, landowners are offered the option of enrolling their land into a preservation program which permanently removes their option to convert their land for development. Upon enrollment, landowners receive a payment equal to the easement value,  $EV_i(X_i, t)$ , but retain ownership of the parcel and the stream of agricultural rent in perpetuity. If

the agricultural landowner can extract the value of these development rights by selling them to a preservation program, the restricted market price will be the expect sum of agricultural rents forever as shown in equation (2).<sup>3</sup>

$$(2) P_i^R = E_t \left[ \int_{t=0}^{\infty} A_i(X_i, t) e^{-r(t)} dt \right]$$

Thus landowners chose  $\beta$  ( $\beta=0, 1$ ) to maximize their economic returns according to (3)

$$(3) \begin{aligned} V_i(t) = & (1-\beta) E_t \left[ \int_{t=0}^{t^*} A_i(X_i, t) e^{-r(t)} dt + \int_{t^*}^{\infty} R_i(X_i, t) e^{-r(t)} dt \right] \\ & + \beta \left[ E_t \left\{ \int_{t=0}^{\infty} A_i(X_i, t) e^{-r(t)} dt \right\} + EV(X_i, t) \right] \end{aligned}$$

If  $E_t \left\{ \int_{t^*}^{\infty} (R_i(X_i, t) - A_i(X_i, t)) dt - EV(X_i, t) \right\} < 0$ ,  $\beta=1$ . The enrollment decision depends on  $t^*(X)$

and  $EV$ , i.e.  $\beta(t^*, EV)$ . Land that is enrolled in the preservation program will not leave agriculture at its (previously) optimal time to develop,  $t^*$ . If there are  $N$  farms in the county, the number of acres of land that will be converted when a preservation program exists in the county

at time  $t^*$  is  $[1 - \frac{1}{N} \int_0^{t^*} \beta(t) dt] L(t^*(X))$ ; total acres with optimal time to convert  $t^*(X)$ ,  $L(t^*(X))$

minus the share of these farms that had enrolled in the program. If the preservation programs are having an impact on the rate of farmland loss, we would expected that

$$(4) \left[ 1 - \frac{1}{N} \int_0^{t^*} \beta(t) dt \right] L(t^*(X)) < L(t^*(X)).$$

Empirically, we would find this result if the preservation programs are enrolling farms that would have left agriculture. Alternatively, if the preservation programs are enrolling farms not threatened by conversion, we might find the right-side of equation (4) equal to the left-side.



Preservation programs also may not be enrolling many farms due to inadequate incentives ( $EV$  is too low), insufficient time in operation, and/or small budgets relative to the number of farmland acres in the county.

### **Propensity Score Matching (PSM) method**

To assess the impact of farmland preservation programs on farmland conversion rate, we employ the propensity score matching method developed by Rosenbaum and Rubin [38]. This method has been used in economic studies to evaluate the effect of job training programs [21, 12, 13, 40], labor market effects of college quality [8], the labor market effects of migration [19], the plant birth effects of environmental regulations [26] and the land market effects of zoning [32]. To the best of our knowledge, no one has used this methodology to identifying treatment effects of farmland preservation programs.

Assessing the impacts of preservation programs is difficult because of incomplete information. While one can identify whether a county has a preservation program (is treated) or not (not treated, or in our analysis, a control) and the outcome (rate of farmland loss) conditional on its treatment, one can not observe the counterfactual, i.e. what would have happened if no farmland preservation program had been established. Thus, the fundamental problem in identifying treatment effect is constructing the unobservable counterfactuals for treated observations.

Let  $Y_1$  denote the outcome in the group of observations if treatment has occurred ( $D = 1$ ), and  $Y_0$  denote the outcome for the group of control observations ( $D = 0$ ). If one could observe the treated and the control states, the average treatment effect,  $\tau$ , would equal  $\bar{Y}_1 - \bar{Y}_0$  where  $\bar{Y}_1$  equals the mean outcome of the treatment group and  $\bar{Y}_0$  of the control group. Unfortunately,

only  $\bar{Y}_1$  or  $\bar{Y}_0$  are observed for each observation. In a laboratory experiment, researchers solve this problem by randomly assigning subjects to be treated or not treated and then construct the unobserved counterfactual. In a natural setting, however,  $\tau \neq \bar{Y}_1 - \bar{Y}_0$  because the treatment condition is not randomly assigned. The propensity score matching (PSM) method proposed by Rosenbaum and Rubin [38] demonstrates that if data justify matching on some observable vector of covariates,  $X$ , then matching pairs on the estimated probability of selection into treatment or control groups based on  $X$  is also justified. To satisfy the Conditional Independence Assumption (CIA) and estimate an unbiased treatment effect, one must find a vector of covariates,  $X$ , such that  $\bar{Y}_1, \bar{Y}_0 \perp D | X$ ; or  $\bar{Y}_1, \bar{Y}_0 \perp D | P(D = 1 | X)$  where  $P(D = 1 | X) \in (0,1)$  is the propensity score that an individual self-selects into treatment groups, and  $\perp$  denotes independence. If CIA holds,  $Y_0$ , the outcome for the controls ( $D = 0$ ), can be assigned to the corresponding treated observations ( $D = 1$ ) as their unobserved counterfactuals using certain matching techniques. One can use a weaker CIA condition that  $E[Y_0 | D = 1, X] = E[Y_0 | D = 0, X] = E[Y_0 | X]$ ,  $P(D = 1 | X) \in (0,1)$  to estimate the average treatment effect.

The average treatment effect on the treated is thus the expected difference in outcome  $Y$  between the treated observations and their corresponding counterfactuals constructed from the matched controls:  $\Delta^{TT} = E(Y_1 | D = 1) - E(Y_0 | D = 1) = E(Y_1 | D = 1) - E(Y_0 | D = 0, P(X))$ .

For the weaker condition to hold, the conditioning set of  $X$  needs to include all of the variables that may affect the outcome and the existence of the programs except the treatment state. In our case, these might include changes in agricultural profitability, demand on land for non-agricultural purposes, and alternative employment opportunities for farmers. By assuming

the  $X$ s are equivalent for the matched treatment and control observations, we are controlling for the effect which these factors may have on the rate of farmland loss.

Given the fact that the data has both cross sectional and time series elements and that counties began land preservation programs in different time periods, heterogeneity may exist between states and over time. Therefore, matching between the control and treated observation is performed under three types of restrictions on the control counties. First, we match the treatment and control observations over the full sample (no restriction) and calculate the overall treatment effect. Using the full sample may provide the best matches since counties in different geographic states may reach the same development stage at the same time while counties within the same state may be at very different development stages at any given time. For example, counties close to metropolitan areas in different states may have experienced development pressure at an earlier period than counties further away from a city in the same state, all else the same. Matching over the full sample therefore has the advantage of providing better controls for treated counties than matching within state or within time period.

Second, because there may have been some unobservable that vary by time period that impact farmland loss, we only match treated counties to control counties within the same time period (time restriction) and estimate the treatment effect on these pairs. Third, again because some unobservable factors within a state may have impacted farmland loss, we only match treated counties with control counties within the same state (state restriction) but did not restrict the time period. A county could have been matched with itself from an earlier time period assuming similar characteristics between time periods. We then ran balance tests for matches under all three restrictions and calculated the treatment effect over the matched groups for each of the three protocols.

## **Background and Data**

Six Mid-Atlantic States (Delaware, Maryland, New Jersey, New York, Pennsylvania and Virginia) experienced a 47% decrease in farmland between 1949 and 1997. The Mid-Atlantic region was one of the first to implement farmland preservation programs. Southampton City and Suffolk County, New York created the first local purchase of development rights programs in the early 1970's. Maryland and Massachusetts each introduced state-level Purchase of Development Rights/Purchase of Agricultural Conservation Easement (PDR/PACE) programs in 1977. By 1997, 5 of the 6 states had a state-level agricultural preservation program under which farmland owners could enroll their land. Calvert County, Maryland was the first to introduce a Transfer of Development Rights (TDR) program with Montgomery County, Maryland following soon afterward.

These programs remove the right to convert the property to residential, commercial and industrial through negative easements in exchange for a monetary payment and/or income and estate tax benefits. The easements applied are perpetual applying to all future owners of the land parcels. The institutional structures of the programs vary by minimum criteria for enrolled farms (soils quality, acreage, proximity to preserved parcels), by payment mechanisms (auctions, installment, point-system), by the source of funding (taxes, bonds, developers), and by geographic specificity/designated zones. However, the easement restrictions are similar across the programs. Easement restrictions to date have been upheld by the courts [11] and thus these programs can be seen as permanently retaining farmland.

Three different types of preservation programs were considered: state PDR/PACE, local PDR/PACE, and local TDR. Data on which counties had farmland preservation programs was collected from American Farmland Trust [2, 3, 4, 5]. States and counties with farmland

preservation programs were contacted via email, snail mail and telephone to collect information on how many acres they had enrolled in 1974, 1979, 1982, 1987, 1992, and 1997. Counties were credited with having a program if any locality (township) within the county had a program that had preserved at least 1 acre. In 1974, no county had a preservation program in place. By 1997, 44% of the counties had some preservation activity through a state or local program.

Table 1 presents the date of implementation, the date of first easement purchase, the number of acres preserved as of January 2002, and the cost of governmentally purchased easements for the state-level programs. Table 2 presents the date of implementation, the date of first easement purchase, the number of acres preserved as of January 2002, and the costs of governmentally purchased easements for the 29 local programs. However, despite Maryland's successful state preservation program which has preserved 198,276 acres, 371,000 acres have been converted to a residential or commercial use simultaneously [30]. Only half as much agricultural land was preserved compared to agricultural land converted. This begs the question – do these agricultural land preservation programs have any effect on the rate of farmland loss? Are they preserving land that would not have been converted to date thus having little to no impact on rate of loss?

Other data were compiled from the *Census of Agriculture* and the *Census of Population and Housing* at the county level for the years 1949 through 2000 [44, 45, 42, 43].<sup>4</sup> The analysis uses data on 263 counties<sup>5</sup> and 10 time periods of 4-5 years each<sup>6</sup> corresponding to the years the *Census of Agriculture* were taken. This resulted in a total of 2609 observations during the almost 50-year period.

The data from the *Census of Population and Housing*, which are collected every 10 years, was adjusted to coincide with the years of the *Census of Agriculture*, which are collected every 4

to 5 years. We assumed that the variables changed at a constant rate between the population and housing census data years. This constant change assumption was used to interpolate the data to the year the agricultural census was collected. Table 3 provides the names and descriptive statistics for the variables by the full sample, those countries with farmland preservation programs (“treated”) and those without (“control”) included in the analysis

The outcome variable of interest is the rate of farmland loss for time period  $t$ . It is calculated as  $\frac{A_{t+1} - A_t}{A_t}$ , where  $A_t$  is the number of acres in the initial period. The rate of farmland loss averaged 7.31% for each 4-5 year time period.<sup>7</sup> The control counties had an average rate over the 50-year period of 7.61% while the treated had a rate of 4.23%. Other differences between the two groups include fewer acre of farmland in the treated counties (108,734 acres) compared to the control counties (144,199 acres).

Demographic variables calculated as a percentage change use the initial year of the time period as the ending year of the percent change calculation. Thus the percent change in housing median housing value for time period  $t$  was calculated as  $\frac{HU_t - HU_{t-1}}{HU_{t-1}}$ , where  $HU_t$  is the median housing value at time  $t$ .

While the census provides the most comprehensive data set over the longest period of time and largest geographic area, it does not report to what use farmland has been converted once it leaves agriculture. While we are fairly certain that much of the land was converted to residential or commercial uses (irreversible conversion for the most part), other farmland may have reverted to forest, tourism or recreational uses. Thus the loss of farmland cannot be automatically attributed to the loss of open space and in some cases this land could be returned to

farmland without excessive cost. Given the matching method however, we think we are most likely matching to control counties where the farmland loss is irreversible. In addition, because the unit of observation is a county, one can make no inferences about the spatial distribution or fragmentation of the remaining farmland which may have an impact on the long-run viability of the agricultural sector.

### ***Propensity Score Estimation and Common Support***

We estimate our propensity scores using a Logit model. CIA condition requires that we choose a set of variables that affects both the existence of farmland preservation programs and pretreatment (pre-program) farmland loss. No mechanical algorithm exists that can automatically chooses a set of variables that satisfies the identification conditions [40]. Moreover, the vector  $X$  that satisfies the balancing tests is not necessarily the most fully inclusive one.

Smith and Todd [40] summarize two types of specification tests motivated by Rosenbaum and Rubin [38] that help choose the correct covariates to be included in the vector  $X$ . The first test examines whether there are differences in the means of the covariates in  $X$  between the treated ( $D=1$ ) and control ( $D=0$ ) groups after conditioning on  $P(X)$ . The second test requires dividing the observations into strata based on the estimated propensity score. These strata are chosen so that there is not a significant difference in the means between treatment and control groups within each stratum [12]. We specify the Logit model to provide the best prediction of the existence of an agricultural preservation program and use the second specification test as proposed by Dehejia and Wahba [12, 13].

Farmland loss is impacted by the non-agricultural net return for land: variables to proxy non-agricultural net return include whether a county has been in a metropolitan area since 1950,

the population level scaled by the size of the county, median family income, and the percentage change in median housing value.

Metropolitan counties may have difficulty retaining farmland due to shorter commuting distance to employment centers. Population increase will increase the net returns to residential and commercial uses and thus increase the rate of farmland loss. Metropolitan and growing counties may value the farmland as it become increasingly scarce and they see the loss of the environmental and scenic amenities farmland provided. These counties may be motivated to establish farmland preservation programs. Higher median incomes may have two impacts. One, higher median family income may increase the demand for larger houses. Large houses usually sit on larger parcels. Two, residents with higher income may be willing to pay more to preserve of the farmland amenities assuming these amenities are normal goods. Thus, an increase in the median family income could increase the demand for farmland accelerating the farmland loss rate and generate higher willingness to pay and better funding for the programs. Percentage change in housing value is also an indicator for land prices and thus returns to conversion.

Agricultural returns would impact the rate of farmland loss. As net returns decrease, the relative value of converting becomes higher. In addition, the expectation of the future may impact a farmland owner's decision to convert the land. The number of farmland acres, percentage of labor force in agricultural sectors, and number of farms proxy for the importance of agricultural sector. If the agricultural sector is strong, farmers may think they have a future in agricultural activities in the county. This confidence may decrease land conversion and increase enrollment in the preservation programs. A strong agricultural presence may also result in a higher level of governmental support for the agricultural land preservation programs.



Farmers may supplement their farm income and decrease their risk with off-farm employment allowing them to retain the farm. Their off-farm income opportunities will be better if they are better educated and the unemployment rate in a county is low. Off-farm employment benefits are proxied by the percent of the county level population that has at least a high school education and the unemployment rate. The percentage of operators with more than 100 days off-farm work and the percent of farms operated by someone who owns some farmland he/she farms are also included as factors that may impact the rate of farmland loss. These factors can positively or negatively affect the rate of farmland loss and enrollment in the preservation programs.

Our logit specification passes the specification test. Figure 1 is the distributions of treated and control groups for all 2609 observations. The X-axis indicates the estimated propensity score, and the Y-axis indicates the percent of observations in the treated and control groups that fall in each strata. The estimated propensity scores for the treatment group are quite evenly distributed, while the distribution of the estimated propensity scores for control group is asymmetric, with more than 60% of the observations falling in the interval between 0 and 0.0036. There are no treated observations below 0.0036. The common support ranges from [0.0036, 0.998].<sup>8</sup> Figure 2 shows the distribution of the estimated propensity score for observations that fall in this common support region. The definitions for the axis are the same as in Figure 1. The asymmetric distribution of the estimated propensity score for the control group requires a careful selection of the matching method to improve the efficiency of the estimated treatment effect.

### ***Matching Methods and Bandwidth Selection***

Several different matching methods are available. All matching estimators have the generic form for estimated counterfactuals:

$$(\hat{Y}_{io} | D_i = 1) = \left( \sum_{j \in \{D_j=0\}} w(i, j) Y_{jo} | D_j = 0 \right)$$

where  $j$  is the index for control observations that are matched to the treated observation  $i$  based on estimated propensity scores ( $j=1, 2, \dots, J$ ). The matrix,  $w(i, j)$ , contains the weights assigned to the  $j$ th control observation that is matched to the  $i$ th treated observation. Matching estimators construct an estimate of the expected unobserved counterfactual for each treated observation by taking a weighted average of the outcomes of the control observations. What differs among the various matching estimators is the specific form of the weights. The estimators are asymptotically the same among all matching methods. But in a finite sample, different method can provide quite different estimators.

Nearest-neighbor matching has each observation paired with the control observation whose propensity score is closest in absolute value [13]. This can be implemented with or without replacing the control and allowing it to be matched again. Replacement guarantees that the nearest match is used. Dehejia and Wahba [14] and Rosenbaum [39] both found that matching with replacement performs as well or better than matching without replacement (in part because it increases the number of possible matches and avoid the problem that the results are potentially sensitive to the order in which the treatment observations are matched). If a control is not the nearest neighbor to any treated observation, then it is not used to compute the average treatment effect. Therefore, the control observations used to compute the treatment effect are those most similar to the treated observations in terms of their observable characteristics.

Kernel matching and local linear techniques match each treated county with all control counties whose estimated propensity scores fall within a specified bandwidth. This bandwidth is centered on the estimated propensity score for the treated county. The matched controls are weighted according to the density function of the kernel type. More control counties are utilized under the kernel and local linear matching as compared to nearest neighbor matching. For kernel matching and local linear matching (LLM), there are five kernel types: Gaussian kernel, biweight kernel, epanechnikov (epan) kernel, uniform kernel and tricube kernel. The density functions for different kernel types are:

$$\text{Gaussian kernel: } w(z) = \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}z^2}$$

$$\text{Biweight kernel: } w(z) = \begin{cases} \frac{15}{16}(1-z^2)^2 & |z| \leq 1 \\ 0 & |z| > 1 \end{cases}$$

$$\text{Epanechnikov kernel: } w(z) = \begin{cases} \frac{3}{4}(1-z^2) & |z| \leq 1 \\ 0 & |z| > 1 \end{cases}$$

$$\text{Tricube kernel: } w(z) = \begin{cases} (1-z^3)^3 & |z| \leq 1 \\ 0 & |z| > 1 \end{cases}$$

$$\text{Uniform kernel: } w(z) = \begin{cases} 1/2 & |z| \leq 1 \\ 0 & |z| > 1 \end{cases}$$

where  $z = \frac{(\hat{p}_0 - \hat{p}_1)}{h}$ . The estimated propensity score for control and treated observations

are  $\hat{p}_0$  and  $\hat{p}_1$  respectively,  $h$  is the bandwidth centered on  $\hat{p}_1$ . Observations within  $h$  receive a weight of  $w(z)$ , while observations more than  $h$  away from the focal  $\hat{p}_1$  receive a weight of 0.

Uniform kernel gives equal weight to all of the observations that falls in the chosen bandwidth.

In our case, the estimated propensity scores for the control counties are asymmetrically distributed while the estimated propensity scores for the treatment counties are more evenly

distributed. Kernel matching operates well with asymmetric distributions because it uses the additional data where it exists but excludes bad matches. McMillen and McDonald [32] suggest that the local linear estimator is less sensitive to boundary effects. For example, when many observations have  $\hat{P}(X)$  near one or zero, it may operate more effectively than other standard kernel matching techniques.

We consider three alternative matching estimators in our empirical work: nearest neighbor estimator, kernel estimator and local linear estimator. We calculate the Mean Square Errors (MSE) for nearest neighbor matching, kernel matching with the five kernel types and local linear matching with the five kernel types for different bandwidth. We use the minimum MSE to pick the optimal bandwidth for each kernel type. Secondly, we pick the optimal kernel type based on the minimum MSE given their optimal bandwidth for each matching method. Finally, we choose the matching method with the minimum MSE given their optimal kernel type and bandwidth.

The leave-one-out validation mechanism proposed by Racine and Li [36] and utilized by Black and Smith [8] is employed to choose among the three matching methods. This mechanism yields several interesting results. First, the nearest neighbor estimator performs worse than the kernel matching and local linear matching for all kernel types. The MSEs for nearest neighbor matching, which are around 0.029, are much larger than those for the other matching methods, which range from 0.014 to 0.017. This result is consistent with other empirical exercises that found the nearest neighbor matching provided a worst result with asymmetrically distributed estimated propensity score for the control group. Second, while tricube local linear matching with bandwidth 0.1 performs a bit better than kernel matching; the difference is very small, especially for epan kernel matching and uniform kernel matching with bandwidth 0.01. This

suggests that the two methods perform similarly. Due to the similarity in performance and the relative difficulty in conducting a balancing test for the tricube local linear matching, we rely on the uniform kernel matching with bandwidth 0.01 and epan kernel matching with bandwidth 0.01 to construct the matching treated and control counties. The formula for calculation of treatment effect on treated thus is:

$$\Delta^{TT} = \frac{1}{N} \sum_{i=1}^N [Y_{i1} - (\hat{Y}_{io} | D_i = 1)] = \frac{1}{N} \sum_{i=1}^N [Y_{i1} - (\sum_{j \in \{D_j=0\}} w(i, j) Y_{jo} | D_j = 0)]$$

### ***Balancing Test***

Three types of balancing test methods exist in the empirical literature: standardized difference test, Hotelling  $T^2$  for joint equality test, and a regression-based test. We use the standardized difference test and the regression-based test.<sup>9</sup> The first method is a t-test for equality of the means for each covariate in the matched treated and control groups. The second test estimates a regression of each covariate on polynomials of the estimated propensity scores,  $[\hat{P}(X)]^l$  and the interaction of these polynomials of the estimated propensity score with the treatment binary variable,  $D * [\hat{P}(X)]^l$  ( $l$ , the order of the polynomial, equals 3). If these estimated coefficients are jointly equal to zero according to an F-test, the balancing condition is satisfied.

The two balancing tests give us similar results (Table 4).<sup>10</sup> The balancing criteria are satisfied for matching over the full sample using the regression test for both matching methods (uniform kernel and epan kernel matching). And in the standardized difference test, we accept the null hypothesis that there is no difference in the means of the covariates. However, when limiting matches to within the same time period or within the same state, neither balancing tests is completely satisfied for either matching method.<sup>11</sup> Therefore, it is possible that because the

control counties are not equally similar to the treatment counties on factors other than the existence of the preservation programs, the treatment effects computed using these matches could be due to these alternative factors.<sup>12</sup> While the “within time period” lack of balance were general attributes of the county (percent change in median housing value, high school education rate, unemployment rate), the “within state” lack of balance included several agriculturally related variables (rate of farm ownership and off-farm employment) which may have more direct influence on the rate of farmland loss.

## **Results**

We compute the estimated impacts of farmland preservation programs for two different time periods: the first is post-1978 through 1997 and second, the full period from 1949 to 1997. Between 1949 and 1978, states began to introduce preferential or use-value property taxation but at varying points in time which potentially could confound the results and no state had established a farmland preservation program and enrolled land. Therefore, we hypothesize that a more pure estimate could be derived from the more limited time period (1978 to 1997). Our estimates of the impact of existence of an agricultural preservation program on the rate of farmland loss appear in Table 5 for the 1978 to 1997 time period and Table 6 for the 1949 to 1997 time period. The bootstrap standard errors are reported in the second row of each matching protocol in Table 5 and Table 6.<sup>13</sup> All estimated treatment effects were corrected for bias and were statistically significant.

The average treatment effects of each matching protocol from 1978 -1997 range from -0.0293 to -0.0372. We find 162 matches for the 184 treatment observations when matched over the full sample and 160 matches when matched over the common support. This included 1101 control observations. Restricting matches to be from the same time period reduced the number

of treated counties matches to 114 for the full sample and 112 for the common support. Finally, matching each treatment county to another within the state (including itself in an earlier pre-program period), resulted in 93 matches for the full sample and 87 for the common support. The treatment impacts for the full sample range from -0.032 to -0.0365. The within time period estimates had a slightly larger range from -0.0293 to -0.0344. Matching within the same state had slightly higher estimates for the full sample (more matches) ranging from -0.0368 to -0.0372 and similar estimates for the common support, -0.0319 to -0.0320. Limiting it to the common support eliminates the treated counties with the highest propensity scores that have lower rates of farmland loss in addition to eliminating those control counties with low propensity scores.

The average treatment effects of each matching protocol from 1949 -1997 are very similar ranging from -0.0293 to -0.0375. We find 162 matches for the 184 treatment observations when matched over the full sample and 160 matches when matched over the common support with 2414 control counties. Restricting matches to the same time period again reduced the number of treated counties matches to 114 for the full sample and 112 for the common support with 435 control counties. Finally, matching each treatment county to another within the state resulted in 95 matches for the full sample and 89 for the common support with 406 control counties. The treatment impacts for the full sample range from -0.0322 to -0.0375, only slightly higher than the 1978 to 1997 period. The within time period estimates were almost identical to the early estimates with the same range from -0.0293 to -0.0344. Matching within the same state for the full sample again had more matches ranging from -0.0368 to -0.0369 and similar estimates for the common support, -0.0320 to -0.0325.

The results suggest that the existence of a farmland preservation program in a county reduces farmland loss by 0.032 on average (3 percentage points), i.e. we find that equation (4) is

satisfied. Given that the average rate of farmland loss per time period is 7.31% in the sample, this is an almost 43% change in the rate. Looking at the average numbers, a county with an average of 141,756 acres of farmland had an average loss of 9,922 acres each 4-5 years. For counties with farmland preservation programs, the farmland conversion decreases to 5,670 acres.

The estimated average treatment effect on the treated counties for matching within a time period is also significant for both matching methods. The average treatment effect in this case is estimated to be -0.034. The average treatment effect of having a preservation program when treatment counties are matched to control counties within the same state is also estimated to be -0.037. Because the balancing test for the within time period matched groups was not satisfied, we can not be certain if the change in the rate of farmland loss should be solely attributed to the existence of the farmland preservation programs. However, the constancy and robustness of the treatment impact across different subsamples and the fact the balance tests were satisfied for the matching in the full sample suggest the results have validity.

## **Conclusions**

While some studies have suggested that farmland preservation programs are not being effective, only a few have examined these programs' impact on the rate of farmland loss. These studies have not always found an impact on farmland loss. If a high rate of farmland loss is the reason that a county implements a program, one must take into account the identification problem that this simultaneity generates. Using the propensity score matching method to compare farmland loss among counties with and without farmland preservation programs having similar characteristics, this analysis finds that farmland preservation programs have reduced the rate of farmland loss.



Our specification includes variables that affect both farmland loss and the existence of farmland preservation program. The epan kernel matching with bandwidth 0.01 and uniform kernel matching with bandwidth 0.01 are employed because they provide low minimum mean square error among alternative methods. The standardized difference test and balancing in a regression framework suggest that the average treatment effects are estimated using treatment and control groups that have similar characteristics. The conclusion appears robust that agricultural preservation programs reduce the rate of farmland loss by about 3 percentage points for each time period for the Mid-Atlantic area. Matching procedures hinges on the specification of  $X$ . We are hopeful that we have accounted for the key variables needed to explain the existence of farmland preservation programs and farmland loss.

Given that counties may have different underlying causes for their farmland loss, for example, some counties in the analysis lost farmland because they lost population rather than because the land was being converted to housing, our results do not suggest that instituting a farmland preservation program may arrest farmland loss in all areas. Some of the farmland lost could have converted to forest, tourism or recreational uses rather than residential or commercial uses. However, we are fairly certain that most counties with preservation programs were losing farmland to residential and commercial uses, thus irreversibly. In addition, county-level data precludes us from knowing more about the spatial distribution or fragmentation of the remaining farmland which may have an impact on the pattern of suburban development, the open-space amenities, and the long-run viability of the agricultural sector.

Further research into the impact and the underlying reasons why these programs may impact farmland loss is important. For example, are farmland preservation programs shifting developers to convert forest land at an increased level, i.e. is the net loss of open space held

constant, or are they increasing the density of housing on the farmland they continue to convert. Have the programs had any impact on rejuvenating cities and local towns and/or stimulating in-fill development. Does this vary by states and could one determine if certain preservation programs result in different strategies. Similarly, has the preserved land remained in active farming and have the programs has any impact on agricultural viability.

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**Table 1. State-level Agricultural Land Preservation Programs by 2002**

State	Year of inception	Year of first easement purchase	Acres protected (1/2002)	Program funds spent	Funds spent per capita
Delaware	1991	1996	65,117	\$69,378,401	\$87.14
Maryland	1977	1980	198,276	\$335,001,530	\$48.01
New Jersey	1983	1985	86,986	\$375,180,691	\$29.34
Pennsylvania	1988	1989	209,338	\$560,621,620	\$34.12
Virginia	No program				

Source: American Farmland Trust. 2002.

**Table 2. Local PDR and TDR Programs begun by 1997 by State and County, 2000 acreage reported**

	Year of inception of first local program	Year of first easement purchase by PDR program	Acres protected (1/2002)	Program funds spent in PDR Programs
<b>Maryland</b>				
Anne Arundel	1991	1992	8,679	\$25,200,000
Baltimore	1979	1981	18,537	\$51,300,000
Calvert	1992	1992	8,000	
Carroll	1979	1980	37,190	\$54,210,903
Charles	1992		1,183	
Frederick	1991	1993	17,296	
Harford	1993	1994	26,800	\$48,900,000
Howard	1978	1984	18,176	\$187,560,000
Montgomery	1980	1989-pdr	50,931	\$28,079,376
Queen Anne's	1987		2,000	
Talbot	1989		500	
Washington	1991	1992	7,332	
<b>New Jersey</b>				
Morris	1992	1996	3,835	\$46,701,384
Burlington	1996		563	
New Jersey				
Pinelands	1981		5,722	
<b>New York</b>				
East Hampton	1982	1982	281	\$5,500,000
Eden	1977		31	
Perinton	1993		56	
Pittsford	1995	1996	962	\$8,199,917
Southampton	1980	1980		
Southold	1984	1986	1,318	\$11,512,250
Suffolk	1974	1976	8,120	\$60,142,788
<b>Pennsylvania</b>				
Bucks	1989	1990	9,550	\$50,104,299
Chester*	1989	1990	7,386	\$18,500,000
Lancaster	1980	1984	40,190	\$80,000,000
York	1990		240	
Plumstead				
Township	1996	1997	1,195	\$4,362,949
Solebury				
Township	1996	1998	1,285	\$11,500,000
<b>Virginia</b>				
Blackburg	1996		23	

Source: AFT 2002, 2001



**Table 3. Descriptive Statistics by the Full Sample, Control Counties, and Treated Counties, 1949-2000 for 6 Mid-Atlantic States**

Variable	Definition of Variables	Full Sample (N=2609)		Control (N=2425)		Treated (N=184)	
		Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
pcfland	Percent change in farmland	0.0731	0.1199	0.0761	0.1222	0.0423	0.0781
Explanatory Variables							
fland	total acres of farmland	141,756	106,982	144,199	108,740	1095,640	73,242
medfinc	median family income	29,929	11,105	28,705	10,127	46,058	10,842
met	=1 if county was a metro area in 1950	0.2227	0.4162	0.2124	0.4091	0.3587	0.4810
nprofper	net profit per acre (sales minus expenses)	219.4	1141.4	209.6	1180.4	348.1	300.5
numf	number of farms in county	979.5	894.7	993.9	906.4	789.0	696.9
pagffm	percent of residents employed in agriculture, forestry, fisheries and mining	0.0994	0.1061	0.1046	0.1081	0.0320	0.0263
pcmhval	percent change in median housing value	0.1081	0.0923	0.1105	0.0921	0.0759	0.0891
phighsch	percent of adults with a high school education	0.4778	0.1762	0.4602	0.1691	0.7098	0.0741
phoffw	percent of operators working 100+ days off the farm	0.4044	0.1041	0.4023	0.1056	0.4324	0.0760
poppera	population per acre	0.5727	1.7958	0.5594	1.8492	0.7488	0.7927
ppartn	percent of farms operated by people who own part of the agricultural land they farm	0.2389	0.0997	0.2367	0.1017	0.2677	0.0620
presprog	= 1, if a county has at least one acre of farmland enrolled in farmland preservation programs	0.0851	0.2791	0	0	1	0
punemp	percent unemployment	0.0549	0.0219	0.0552	0.0223	0.0518	0.0165
Source: US Census of Agriculture (1949-1997), US Census of Population and Housing (1950-2000), Personal Communication							

**Table 4: Balancing Test for the Distribution of the Variables between Matched Treated ( $X_1$ ) and Control ( $X_0$ ) Groups for Observations after 1978**

	Epan Kernel Matching (bandwidth =0.01)		Uniform Kernel Matching (bandwidth =0.01)	
	Full sample	Common support	Full sample	Common support
Matching over full sample				
T-test*	0	0	0	0
Regression Test**	0	0	0	0
Matching within period				
T-test*	2	2	1	1
Regression Test**	2	0	2	1
Matching within state				
T-test*	2	2	2	2
Regression Test**	4	4	3	4

Notes: \*This is the number of pretreatment covariates where their P-value is below 0.05  
 \*\*The number of covariates for which the null hypothesis is rejected (p-value <0.05)

**Table 5: Comparison of Rate of Farmland Loss for the Matched Treated and Control Counties for Counties during 1978-1997 (N=93, 114, 162 using all observations, N=87, 112, 160 using observations within common support only)**

	Epan Kernel Matching (bandwidth =0.01)		Uniform Kernel Matching (bandwidth =0.01)	
	Full sample	Common support	Full sample	Common support
Matching over full sample				
ATT*	-0.0365	-0.0324	-0.0366	-0.0320
Standard error	0.0116	0.0117	0.0119	0.0120
Number of Matched Treated Counties	162	160	162	160
Number of Matched Control Counties	1101	1101	1101	1101
Matching within time period				
ATT*	-0.0344	-0.0293	-0.0339	-0.0295
Standard error	0.0113	0.0119	0.0113	0.0120
Number of Matched Treated Counties	114	112	114	112
Number of Matched Control Counties	435	434	435	434
Matching within state				
ATT*	-0.0368	-0.0320	-0.0372	-0.0319
Standard error	0.0115	0.0120	0.0113	0.0116
Number of Matched Treated Counties	93	87	93	87
Number of Matched Control Counties	253	250	253	250

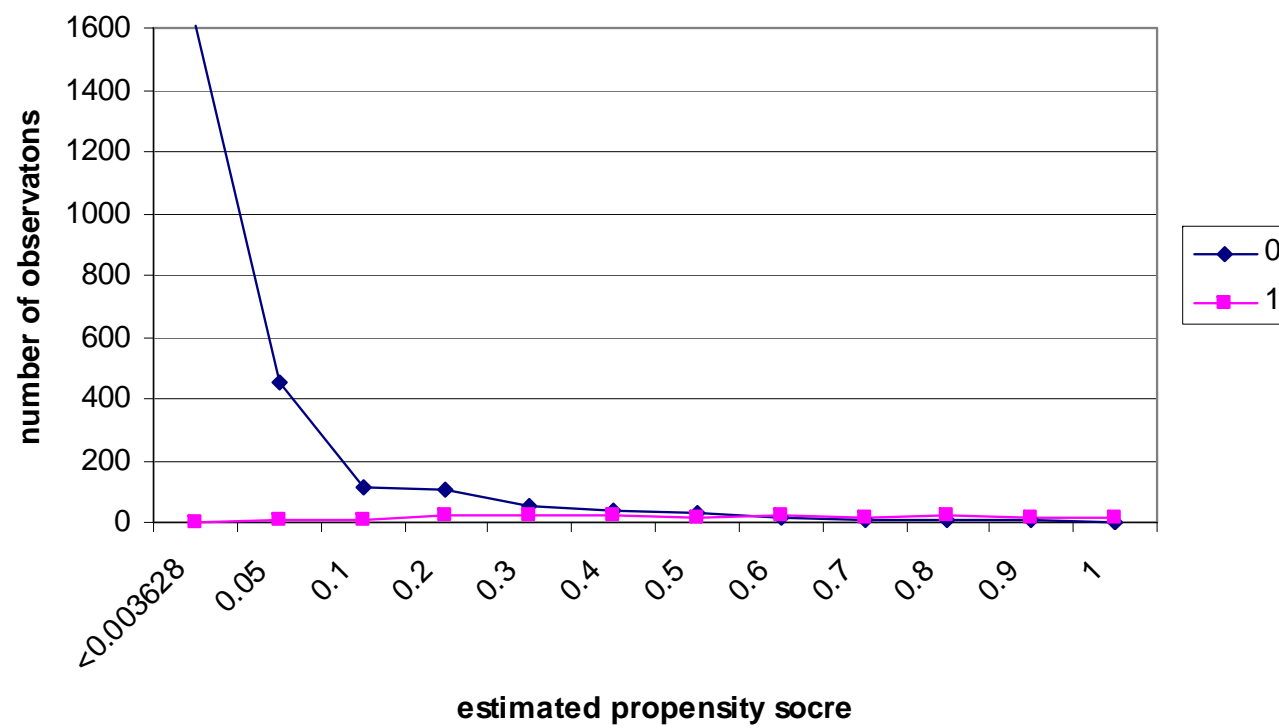
Note: \*We report the Bias Corrected Average Treatment Effect. For Epan kernel Matching using all observations, the biases for Matching over full sample, Matching within time period and Matching within state are 0.0061, 0.005 and 0.0064, respectively. For uniform kernel matching using all observations, they are 0.0062, 0.0045 and 0.0068. For Epan kernel Matching using observations within common support, the biases for Matching over full sample, Matching within time period and Matching within state are 0.0054, 0.0033 and 0.005, respectively. For uniform kernel matching using all observations, they are 0.005, 0.0035 and 0.0049.

**Table 6: Comparison of Rate of Farmland Loss for the Matched Treated and Control Counties for Counties during 1949-1997 (N=95, 114, 162 using all observations, N=89,112,160 using observations within common support only)**

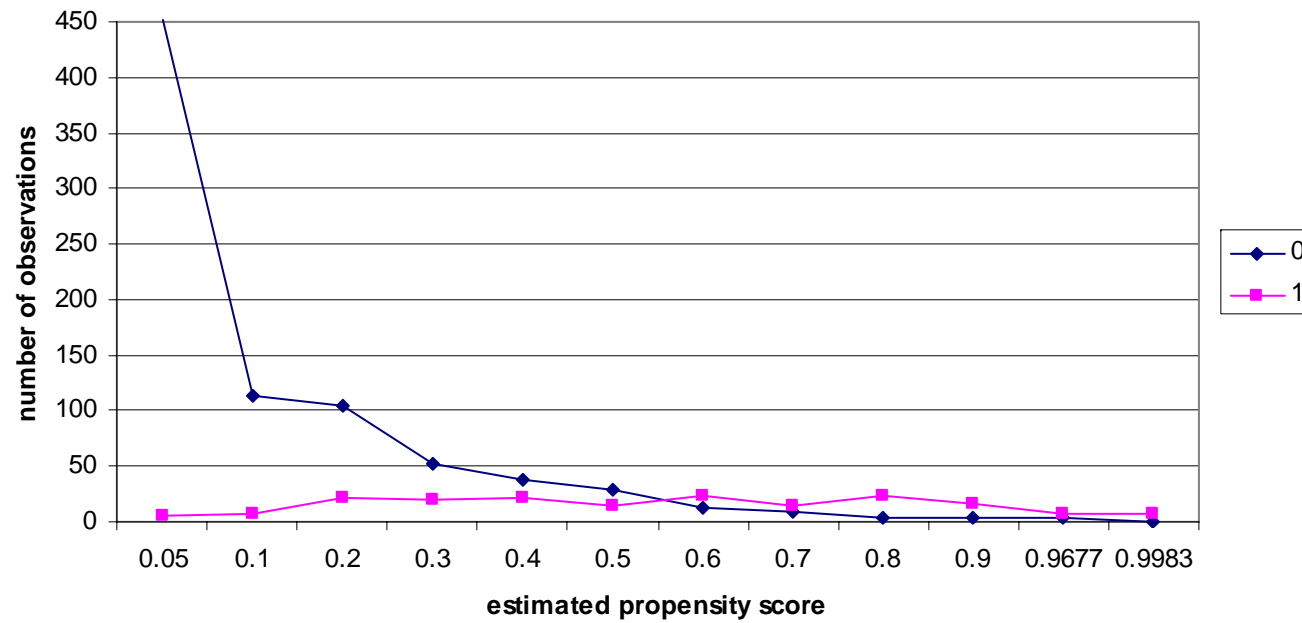
	Epan Kernel Matching (bandwidth =0.01)		Uniform Kernel Matching (bandwidth =0.01)	
	Full sample	Common support	Full sample	Common support
Matching over full sample				
ATT*	-0.0375	-0.0370	-0.0369	-0.0322
Standard error	0.0113	0.0115	0.0114	0.0114
Number of Matched Treated Counties	162	160	162	160
Number of Matched Control Counties	2414	2414	2414	2414
Matching within time period				
ATT*	-0.0344	-0.0293	-0.0339	-0.0325
Standard error	0.0113	0.0119	0.0113	0.0116
Number of Matched Treated Counties	114	112	114	112
Number of Matched Control Counties	435	434	435	434
Matching within state				
ATT*	-0.0368	-0.0320	-0.0369	-0.0325
Standard error	0.0117	0.0121	0.0115	0.0116
Number of Matched Treated Counties	95	89	95	89
Number of Matched Control Counties	406	403	406	403

Note: \*We report the Bias Corrected Average Treatment Effect. For Epan kernel Matching using all observations, the biases for Matching over full sample, Matching within time period and Matching within state are 0.0065, 0.005 and 0.0062, respectively. For uniform kernel matching using all observations, they are 0.0063, 0.0045 and 0.0063. For Epan kernel Matching using observations within common support, the biases for Matching over full sample, Matching within time period and Matching within state are 0.0048, 0.0033 and 0.0048, respectively. For uniform kernel matching using all observations, they are 0.005, 0.0053 and 0.0053.

**Figure 1: Distribution of Estimated Propensity Scores for Full Sample**



**Figure 2: Distribution of Estimated Propensity Score Within Common Support**



## Endnotes

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<sup>1</sup> Although there are 50 TDR programs, only 15 of them have protected farmland.

<sup>2</sup> To simplify the model only two land uses are used. However, in some cases the landowner will maximize his or her present value by shifting the land use to commercial, industrial or other alternative land uses.

<sup>3</sup> While not explicitly modeled, the landowner could sell the farmland in the future with the easement restrictions attached to the property. However, even with a new owner, no residential, commercial or industrial development would be permitted.

<sup>4</sup> We attempted to extend our data to the 2002 Census of Agriculture. However, due to the fact that the Census is now adjusting the data to deal with non-responses, the data in 2002 were not comparable to those in 1949-50 and beyond.

<sup>5</sup> Independent cities of Virginia are also included in the analysis. In several cases, due to either aggregation in data or actual boundary changes during the study period, counties and/or independent cities have been combined for this analysis.

<sup>6</sup> Counties with fewer than 5 farms in 1949 were excluded from the entire analysis. Six counties were excluded due to limited agricultural activity in 1949: Bronx, Queens, Richmond, Kings, and New York counties of New York state, and Arlington County of Virginia

<sup>7</sup> Farmland is defined by the U.S. Agricultural Census to consist of land used for crops, pasture, or grazing. Woodland and wasteland acres are included if they were part of the farm operator's total operation. Conservation Reserve and Wetlands Reserve Program acreage is also included in this count.

<sup>8</sup> The lower bound for common support is the maximum of the minimum of estimated propensity scores for treated and control; the upper bound is the minimum of the maximum of the estimated propensity scores for treated and control groups.

<sup>9</sup> The Hotelling  $T^2$  tests the joint null of equal means of all of the variables included in the matching between the treatment group and the matched control group. Smith and Todd [41] found that in some cases this test incorrectly treated matched weights as fixed rather than random. Therefore we rely on the other two balancing test.

<sup>10</sup> We report the balancing test for the 1978-1997 period. The balancing tests for 1949-1978 are almost identical.

<sup>11</sup> We hypothesize that the balance tests were not satisfied due to lack of similar control counties within each time period or within a state with which the treated observations could be match. For example, Delaware has only three counties and only three treated observations among 30 observations all in 1997. Virginia has only two treated counties for the entire study period.

<sup>12</sup> The "within the same time period" matches did not meet the balancing test for percent change in median housing value, percent of county population that had graduate from high school, and the percent of unemployment. The within the same state matches also did not balance on the percent high school variable. In addition, there was less agreement on the number of farmers who own part of the farm they operate, median family income, and percent of operators working 100 or more days off the farm.

<sup>13</sup> We use a simple bootstrap procedure to construct the standard errors for the average treatment effect. We make 2,000 independent draws from the treatment and control observations and form new estimates of the treatment effect for each draw. The bootstrap standard error estimate is the standard deviation of the 2000 new values for the estimated treatment effect.