Do Agricultural Preservation Programs Affect Farmland Conversion?

Evidence from a Propensity Score Matching Estimator

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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. We ask how effective these programs are in slowing the rate of farmland loss. 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 2.2 to 3.1 percentage points; a 30-42% decrease from the average 5-year rate of 7.31%.

Do Agricultural Preservation Programs Affect Farmland Conversion?

Concerns about the loss of farmland and the increase in suburban sprawl led states and counties to instituted 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 (Gardner, 1977; Hellerstein et al., 2002).

More than 124 governmental entities[1] have implemented farmland preservation programs (American Farmland Trust (AFT), 2001; AFT, 2005; AFT, 2006) 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 (AFT, 2005; AFT, 2006). 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 (Land Trust Alliance and Trust for Public Lands, 2006). 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 (Feather and Barnard, 2003; Duke and Ilvento, 2004), 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 (Blewitt and Lane 1988; Gardner 1994; Lynch and Carpenter, 2003; Parks and Quimio, 1996; Heimlich and Anderson, 2001) on farmland conversion, yet few have studied the impact of the permanent easements conferred by the PDR/PACE and TDR programs. 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 (MALPF Task Force, 2001; Lynch and Lovell, 2003; Lynch and Musser, 2001; Adelaja and Schilling, 1999). 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 (MALPF Task Force, 2001). Only half as much agricultural land was preserved compared to agricultural land converted. Are the programs preserving land that would not have been converted to date thus having little to no impact on rate of loss? Therefore, we ask the question: do PDR/PACE and TDR programs affect the rate of farmland loss? 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.

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. Lynch and Carpenter (2003) find 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 itself may be predicated on the rate of farmland loss. Acres preserved may not be sufficient to assessing a program's impact on farmland loss. McConnell, Kopits, and Walls (2005) 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 (2004) 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 (2003) and Irwin (2002) find that housing prices adjacent to preserved parcels can increase due to the permanency of adjacent open space. Furthermore, if the programs are enrolling those parcels least likely to be converted, their impact on the rate of farmland loss may be insignificant.

We suggest we can overcome some of the empirical difficulties by using a propensity score matching (PSM) 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 observable characteristics. This provides a more transparent mean to decrease the influence of outliers and dissimilar counties. Second, because not all counties are equally likely to have farmland preservation programs, PSM incorporates pretreatment covariates that may influence the existence of such a program as well as farmland loss into the propensity score calculation. Third, a specific functional form is not assumed for outcome equation, the decision process or the unobservable terms. Therefore, propensity score matching may be a more appropriate approach because it requires fewer assumptions than an instrumental variable approach.

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 propose 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. (2001) 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. In the simplest form, one can think of the market price per acre P_i of the parcel i as determined by the stream of rents. The market value is thus the sum of agricultural rents given the land and locational characteristics of parcel i ($X_{i,}$), $A_i(X_i, t)$ from time $t=0$ up to an optimal conversion date $t^*(X_i)$, at which time the land is converted into a residential use with the sum of net returns of $R_i(X_i, t)$ as shown in equation (1).[2] The discount rate is r.

$$(1)\ P_i = \int_0^{t^*(X_i)} A_i(X_i, t)e^{-rt}dt + \int_{t^*(X_i)}^{\infty} R_i(X_i, t)e^{-rt}dt$$

Assuming the land is in an agricultural use at time t, agricultural rents are greater than net residential rents. However, agricultural rents are expected to grow more slowly than net residential rents $\left(\frac{\partial A_i}{\partial t} < \frac{\partial R_i}{\partial t}\right)$. Thus to maximize the return from the land, a landowner will set the optimal conversion date $t^*(X_i)$ such that the net returns to agriculture and net returns to residential uses are equal: $A_i(X_i, t^*) - R_i(X_i, t^*) = 0$. Let there be a density function across the land and locational characteristics that reflects potential development likelihood that we define as $F(X)$. We define $L(X)$ as the acres of land with characteristic X. Then the land in a county that would be converted from agricultural to another use at time t, $L_C(t)$ is equal to:

$$L_C(t) = \int_{\{X:\ t^*(X) \le t\}} L(X)\,dF(X)$$

Or all land with characteristics X such that the optimal conversion time $t^*(X_i)$ is less than the current time. Similarly, the land in a county that remain in agricultural production $(L_A(t))$ is equal to:

$$L_A(t) = \int_{\{X:\ t^*(X) > t\}} L(X)\,dF(X)$$

In some counties, landowners are offered the option of enrolling in a preservation program which permanently removes their option to convert their land for development. Upon enrolment, landowners receive a payment equal to the easement value, $EV_i(X_i)$, but retain ownership of the parcel and the stream of agricultural rent in perpetuity. If the agricultural landowner can extract the value of the 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).[3]

$$(2)\quad P_i^R = \left[\int_0^\infty A_i(X_i, t)e^{-rt}\,dt\right]$$

The enrollment decision depends on the land characteristic X_i and easement payment $EV(X_i)$, i.e. $\beta(X_i, EV(X_i))$. Landowners chose ($\beta(X_i, EV(X_i)) = 0,\ 1$) to maximize their economic returns according to (3)

$$
\text{(3)}\quad
\begin{aligned}
V_i = &\left(1-\beta(X_i, EV(X_i))\right) \left[\int_0^{t^*(X_i)} A_i(X_i, t)e^{-rt}dt + \int_{t^*(X_i)}^{\infty} R_i(X_i, t)e^{-rt}dt \right] \\
&+ \beta(X_i, EV(X_i)) \left[\int_0^{t^*(X_i)} A_i(X_i, t)e^{-rt}dt + EV(X_i) \right]
\end{aligned}
$$

If $\int_{t^*(X_i)}^{\infty} (R_i(X_i, t) - A_i(X_i, t))e^{-rt}dt - EV(X_i) < 0$, then $\beta(X_i, EV(X_i)) = 1$. Land i that is enrolled in the preservation program will not leave agriculture at its (previously) optimal time to develop, $t^*(X_i)$. Therefore, the number of acres converted from agriculture becomes

$$\int_{\{X:\ t^*(X) \le t\}} (1-\beta(X, EV(X)))L(X)dF(X);$$

the total acres with an optimal time to convert $t^*(X)$ earlier than t , minus that proportion of these acres chose to enroll in the preservation programs. If the preservation programs are having an impact on the rate of farmland loss, we would expect that the rate of conversion is lower as depicted in (4).

$$\text{(4)}\quad \int_{\{X:\ t^*(X) \le t\}} (1-\beta(X, EV(X)))L(X)dF(X) < \int_{\{X:\ t^*(X) \le t\}} L(X)dF(X)$$

The net effect of the agricultural land preservation programs is:

$$\int_{\{X:\ t^*(X) \le t\}} \beta(X, EV(X))L(X)dF(X) > 0$$

Empirically, we would find this result at any point of time if the preservation programs are enrolling farms that would have left agriculture by that point. Alternatively, if the preservation programs are enrolling farms not threatened by conversion at the time of evaluation $t^*(X) > t$, we might find the right-side of equation (4) equal to the left-side at that time. Alternatively, preservation programs may not be enrolling many farms due to inadequate incentives (*EV* is too low), insufficient time in operation (only began recently), 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 (1983). This method has been used in economic studies to evaluate the effect of job training programs (Heckman et. al., 1997; Dehejia and Wahba, 1999, 2002; Smith and Todd, 2005a), labor market effects of college quality (Black and Smith, 2004), the labor market effects of migration (Ham et. al., 2003) the plant birth effects of environmental regulations (List et. al, 2003) and the land market effects of zoning (McMillen and McDonald, 2002). 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, τ, would equal $\overline{Y}_1 - \overline{Y}_0$ where $\overline{Y}_1$ equals the mean outcome of the treatment group and $\overline{Y}_0$ of the control group. Unfortunately, only $\overline{Y}_1$ or $\overline{Y}_0$ 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, $\tau \neq \overline{Y}_1 - \overline{Y}_0$ because the treatment condition is not randomly assigned. The propensity score matching (PSM) method proposed by Rosenbaum and Rubin (1983) 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 $\overline{Y}_0 \perp D \mid X$ *; or* $\overline{Y}_0 \perp D \mid P(D = 1 \mid X)$ where $P(D = 1 \mid X) \in (0,1)$ 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. The CIA condition is stronger than required therefore we use the Conditional Mean Independence (CMI) assumption that

$E[Y_0 \mid D = 1, X] = E[Y_0 \mid D = 0, X] = E[Y_0 \mid X]$, $P(D = 1 \mid X) \in (0,1)$ to estimate the average treatment effect (Heckman, Ichimura, and Todd, 1998).

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: $\Delta^{TT} = E(Y_1 \mid D=1) - E(Y_0 \mid D=1) = E(Y_1 \mid D=1) - E(Y_0 \mid D=0, P(X))$.

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.

We first 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 locations 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 may have experienced development pressure at an earlier period than counties further away from a city, 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. We then ran balance tests for matches and calculated the average treatment effect on the treated over the matched groups.

Second, because there may have been some unobservable factors that vary by time period that impact farmland loss and are not captured by our estimated propensity scores, we also conduct matching within a time period. In this case, a treated county is restricted to match control counties within the same time period. The average treatment effect on the treated is then computed using these matched groups.[4]

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 restricting all future owners of the land parcels. The institutional structures of the programs vary by minimum criteria for enrolled farms (soil 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 (Danskin 2000) 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 (AFT 1997, 2001, 2002a, 2002b). 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, 1978, 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.

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 (USDA, 1997, 2001; US Department of Commerce, 1950-1992, 1950-2000).[5] The analysis uses data on 263 counties[6] and 10 time periods of 4-5 years each[7] corresponding to the years the *Census of Agriculture* were taken. This resulted in a total of 2609 observations during the 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 3.1 and 3.2 provides the names and descriptive statistics for the variables by the full sample, those counties with farmland preservation programs ("treated') and those without ("control") included in the analysis for 1949-1997 and 1978-1997 respectively.

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.[8] 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). We also consider the outcome variable, the change in farmland acres, calculated as $A_{t+1} - A_t$.

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), some 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 treatment counties 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.

Variables included in Propensity Score Computation

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.

Farmland loss is impacted by the non-agricultural net return for land, $R(X_i,t)$: 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 the farmland amenities. 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 for the programs. Percentage change in housing value is also an indicator for land prices and thus returns to conversion.

Agricultural returns, $A(X_i,t)$,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 local importance of agricultural sector. If the agricultural sector is strong, farmland owners 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.

The local economy may also impact the rate of farmland loss. 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.

We also include binary variables for the time periods: 1978-1982, 1982-1987, and 1987-1992 and 1992-1997. The period, 1992-1997, is the excluded category. Because no counties had a farmland preservation program before 1978, we cannot include time variables for the early years.

Propensity Score Estimation

As mentioned above, 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 choose a set of variables that satisfies the identification conditions (Smith and Todd, 2005b). Smith and Todd (2005b) summarize two types of specification tests motivated by Rosenbaum and Rubin (1983) 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 (Dehejia and Wahba, 1999). We estimate our propensity scores using a random effect logit model controlling for county effects (Table 4) using the variables outlined above. We use the second specification test as proposed by Dehejia and Wahba (1999, 2002).

The random logit model 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 follow a more even distribution although with slightly more observations having high probabilities of having a program. 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 0.00004. There are no treated observations below 0.00004. The common support ranges from [0.0004, 0.999].[9] 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:

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

where *j* is the index for control observations that are matched to the treated observation *i* based on estimated propensity scores (*j=1,2,…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.

The formula for calculation of treatment effect on treated thus is:

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

Nearest-neighbor matching has each observation paired with the control observation whose propensity score is closest in absolute value (Dehejia and Wahba 2002). 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 (2002) and Rosenbaum (2002) 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 on the treated. 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.

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 (2002) 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

Bandwidth and kernel type selection is an important issue when one selects matching method. Generally speaking, a large bandwidth leads to a larger bias but smaller variance of the estimated average treatment effect on the treated; a small bandwidth leads to a smaller bias but a larger variance. The difference among the kernel type is embedded in the weight they assign to the control observations that are farther away from the estimated propensity score of a treated observation to which controls are matched. A trade-off between bias and variance for the estimated effect could exist from the different weights assigned to those observations by different kernel types. As the selection of bandwidth and kernel type involves a trade-off between bias and variance, we need criteria that allow us to balance the two. The leave-one-out cross-validation mechanism proposed by Racine and Li (2004) and utilized by Black and Smith (2004) provide us such a criterion: to choose the method (a combination of matching method, kernel type, and bandwidth) that minimize Mean Squared Error (MSE) for the estimator given the distribution of the data. We employ the leave-one-out cross validation method taking into account balancing objectives to choose among matching methods.

We consider three alternative matching estimators: nearest neighbor estimator, kernel estimator and local linear estimator. We calculate the Mean Square Errors (MSE) for all the possible combinations of the three matching methods, five kernel types (epan kernel, biweight kernel, uniform kernel, tricube kernel, and Gaussian kernel), and bandwidth (bandwidth = 0.01, 0.02, …,0.1).

We find several interesting results for matching without restriction. 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 0.037, are much larger than those for the other matching methods, which range from 0.013 to 0.017. This result is consistent with other empirical exercises that found the nearest neighbor matching provided a worse result with asymmetrically distributed estimated propensity score for the control group. Second, while tricube local linear matching with bandwidth 0.04 and above (0.013) performs a bit better than kernel matching; however, the difference is very small, especially for epan kernel matching and uniform kernel matching with bandwidth 0.02 (0.015). This suggests that the two methods perform similarly. For matching within time period, we find again that the MSEs for nearest neighbor (0.037) are much larger than that for kernel and local linear matching (0.012 to 0.11). However, the local linear matching generally performs worse than kernel matching for all kernel types The MSEs for local linear matching (0.0123 to 0.11) are larger than that for kernel matching (0.0121 to 0.0126) for all kernel type except for kernel type of tricube. Third, the MSE for kernel matching across different bandwidth are very similar. Due to the similarity in performance for matching without restriction and that local linear matching performs worse for matching within time period, we rely on the uniform kernel matching with bandwidth 0.02 and epan kernel matching with bandwidth 0.02 to construct the matched treated and control counties for both matching scenarios. The two methods also provide us with better balance between the control and treatment covariates than other methods and bandwidths.

Balancing Test

We rely on two of the balancing tests that exist in the empirical literature: standardized difference test and a regression-based test.[10] The first method is a t-test for equality of the means for each covariate in the matched treated and control groups. The regression test estimates a coefficients for each covariate on polynomials of the estimated propensity scores, $[\hat{P}(X)]^{l}$ and the interaction of these polynomials interacted with the treatment binary variable, $D*[\hat{P}(X)]^{l}$ (*l,* here the order of the polynomial equals 3). If these estimated coefficients on the interacted terms are jointly equal to zero according to an F-test, the balancing condition is satisfied.

The two balancing tests give us similar results (Table 5.1 and 5.2). The balancing criteria are satisfied for most of our key covariates for matching without restriction using the regression test and the standardized difference test for both matching protocols (uniform kernel and epan kernel matching) except for two variables. These two are the percentage of operators who rent part but not all of the land they farm and the time dummy for 1978-1982. The percentage of operators who rent part of their land for matched treated and control group are 0.26 and 0.28 respectively. This suggests that counties with preservation programs have more operators that own all the land they farm. Interestingly, our random effect logit regression indicates a positive and significant impact of this covariate on the propensity of having a farmland preservation program; i.e. that the counties with a high percentage of operators that rent part of their land have a higher probability of having a program. A simple OLS, a random effect or a fixed effect regression of the rate of farmland loss on this covariate results in a negative or an insignificant coefficient, which implies that this covariate may bias our estimated average treatment effect on the treated downward. The bivariate variable for the time period of 1979 through 1982 is only balanced within the common support. The percent of treated counties in 1979-82 is 7% compared to control counties at 13%.

When limiting matches to within the same time period, we find balance on most covariates again but with more exceptions. Within common support, again, we find the percentage of operators who rent part but not all the land they farm is not balanced, as well as the percent of unemployment in the county (means of treated:control are 0.052:0.056). For the full sample, the number of farms in a county (treated: control=763:632) and whether the county was a metropolitan area since 1950 (treated:control=0.39:0.21) are added to the list of unbalanced variables off the common support. A simple OLS, random and fixed effect regressions of the rate of farmland loss on the percent unemployment returns a significant coefficient less than 0.22 or an insignificant coefficient. We therefore argue that the upward bias caused by the unbalanced percent unemployment may be negligible or nonexistent. Given number of farms and the metropolitan status are balanced on the common support, we expect the difference in these variables has little impact on bias in computing the average treatment impact.

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 did so at varying points in time. By 1978, all six states had some type of preferential taxation programs. The introduction of these preferential taxation programs could confound the results for the 1949-1978 timeframe. In addition, prior to 1978, no state had established and enrolled land in a farmland preservation program. Therefore, we think a more pure estimate could be derived from the post-1978 time period. Our estimates of the impact of existence of an agricultural preservation program on the rate of farmland loss appear in Table 6.1 for the 1978 to 1997 time period and Table 6.2 for the 1949 to 1997 time period. The bootstrap standard errors are reported in the second row of each matching protocol in Table 6.1 and Table 6.2.[11] All estimated treatment effects were corrected for bias and were statistically significant.

For the outcome of rate of farmland loss, the average treatment effects on the treated of each matching protocol from 1978-1997 range from -0.022 to -0.031. We find 186 matches for the 187 treatment observations when matched over the full sample and 178 matches when matched over the common support only (dropping observations with propensity scores less than 0.0004 and greater than 0.999). The treated observations are matched with 844 control observations. Restricting matches to be from the same time period reduced the number of treated counties matched to 167 for the full sample and 150 for the common support. 815 control counties are used in these matches. The treatment impacts for matching without restriction over full sample range from -0.027 to -0.031. The range is from -0.027 to -0.030 for matching without restriction within common support. The within time period estimated impacts are -0.022 for all matching protocols.

We also look at the number of acres lost as a complement to the percent of acres lost. For that measure, the average treatment effects on the treated of each matching protocol from 1978-1997 range from -1701 to -2995. This suggests that counties with farmland preservation programs lost fewer acres per year 340 fewer acres on the low end and 600 fewer on the high end than similar counties without farmland preservation programs. The average treatment effects on the treated from matching without restriction over full sample range from -2452 to -2995, from -2327 to -2752 within common support. The within time period estimators range from -1711 to -1953 over full sample, and -1701 to -1804 within common support.

The average treatment effect on the treated from 1949-1997 are very similar to those above. We find 186 matches for the 187 treatment observations when matched over the full sample and 178 matches when matched over the common support with 2417 control observations used for the counterfactuals. The average reduction in the rate of farmland loss of each matching protocol from 1949 -1997 are the same as that from 1978-1997. The average reduction in the acres of farmland loss has a slightly smaller range. The range is from -2496 to -2928 for matching over full sample, and -2401 to -2656 for matching over common support. The matching results for restricting matches within time period for 1949-1997 are exactly the same as that from 1978-1997 since counties start to have active program after 1978.

The similarity of the average treatment effect from 1949-1997 to that from 1978-1997 suggests that unobservable factors varying across time period before 1982 do not have significant impact on farmland loss. Given no county had a preservation program with enrolled acreage before 1978, we had some concern about not controlling for these unobservable factors in computing the propensity scores. However, the estimated propensity scores for the observations before 1978 falls to the low end as one might expect and those observations are assigned very small weights in calculating counterfactuals.

Compared with matching without restriction, the average treatment effects on the treated from restricting matches to counties only within the same time period are smaller. The average impact estimators on the rate of farmland loss are -0.022 which are 0.005-0.008 points lower than above. The average impact on acres ranges from 1,701 to 1,953 with the difference from unrestricted matching ranging from -955 to -1284 for epan kernel matching, and from -499 to -597 for uniform kernel matching. There may be some unobservable factors within a time period that are impacting the results that restricting the matching to within time period addresses. Although the ATT are smaller for matching within time periods, they continue to be statistically significant.

The results suggest that the existence of a farmland preservation program in a county reduces farmland loss by 2.2 to 3.1 percentage points on average, 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 full sample, this is a 30-42% change in the rate. The change is an even larger percentage for the 1978-1997 sample, which has an average rate of farmland loss of 3.44%. Similarly, in an absolute sense, acres converted reduced from 1,701 to 2,995 acres from an average acres converted of 9,994 per period (17- 30%) Given that fact that the unbalanced covariates are thought to have either downward or negligible bias on our estimator of the average treatment effect, the estimated effect of farmland preservation programs actually may be conservative.

Sensitivity analysis

The propensity score matching method potentially provides more reliable results than a standard regression method by comparing control and treated observations that are similar to each other, explicitly excluding outliers, and estimating the treatment effect on the treated non-parametrically. However, if there are unobserved variables that affect either the treatment assignment or the outcome variable, the CIA conditions do not hold and the propensity score matching estimators are no longer consistent. While we controlled for many observables, we also conduct a sensitivity analysis by looking at Rosenbaum bounds and hidden bias equivalents (Rosenbaum, 2002; DiPrete and Gangl, 2004)[12].

Rosenbaum bounds is a signed rank test to assess the potential impact of hidden bias arising from confounding variables associated with both treatment and outcome variables. It assumes that the strength of the impacts from unobservable factors on treatment selection and outcome is the same. This approach is relatively conservative in the sense that it will find bias even if the strength of unobservable factors on outcome is not as strong the test assumes.

The estimated propensity score of a treated and control observation with identical characteristics (the same covariates) should be equal if all the relevant covariates that affect both the treatment assignment and outcomes are included in the propensity score model. The presence of unobserved covariates leads to discrepancies between the propensity scores of treated and control observations with identical characteristics. As a result, the odds ratio of a matched pair of treated and control observations based on these characteristics will no longer be equal to one. The larger the effect of an unobserved covariate on the treatment assignment, the larger the difference between the odds ratio and one will be.

Rosenbaum had shown that the odds ratio for matched pairs is bounded by the function of the strength of the effect. Therefore, a signed rank statistic of each strength level has its upper and lower bounds and their corresponding p-values. One can determine a critical level of the strength of effect for a 95% confidence interval. If the unobserved covariates affect the treatment assignment and/or the outcome at a strength level greater than the critical effect strength, the average treatment effects could include zero. (see Rosenbaum (2002) and DiPrete and Gangl(2004) for more information).

Beyond finding the upper and lower bounds, following DiPrete and Gangl (2004), we also calculate the hidden bias equivalents on some key covariates. Table 7 reports the upper and lower bounds for Kernel matching with Epan kernel type with bandwidth=0.02 for matching without restriction as well as the hidden bias equivalents.[13] The threshold gamma measures the effect strength of unobservable variables on treatment assignment and equals 1.55 for the rate of farmland loss. Thus the statistical significance of the ATT for the rate of farmland loss is called into question when the odds ratio of treatment assignment between the treated and control groups differs by more than 1.55. However, while questionable, the treatment effect can still be significant if the effect on the treatment assignment is greater than the effect on the outcome.

We calculate the hidden bias equivalents on three key variables. Two variables are perfectly balanced and one is not. The total acres of farmland in a county and net profit per acre, both these variables were balanced between the control and the treatment groups. To check the impact of an unbalanced variable, we also calculate the hidden bias equivalents for the variable unemployment rate. At the critical level of gamma for the rate of farmland loss, in terms of affecting the ATT results, the possible unobserved variables would have to have the same impact as changing these 3 key variables by 14,902 acres (11%) for total acres of farmland, by $42.5 (19%) for net profit per acres, and by 0.17% (3.1%) for unemployment rate. For farmland acres loss, the critical threshold gamma is 1.62. The hidden bias equivalents are a change of 17,224 acres (12%) in total farmland, $48.6 (22%) in net profit per acre, and 0.20% (3.6%) in unemployment rate. These hidden bias equivalents suggest our ATT results are not largely sensitive to changes in key variables or potential unobserved variables.

Regression estimation

To further check the robustness, we estimate the effect of PDR and TDR programs in a regression framework following Wooldridge (2002). We specify a random effect model controlling for treatment, estimated propensity score, and a set of control variables that impact the rate of farmland loss. The control factors in the random effect model include: acres of farmland and its square (possible non-linear impacts), metropolitan status, percentage change in total housing units, median housing value, population per acre, net agricultural profit per acre, and percent of operator with any off-farm work, median family income, and percentage of the population with high school education. Controls for time effects include time dummies indicating the time periods after 1978. We estimate the random effect regression for both the full sample and a post-1978 sub-sample. We do not removed outliers or those not on the common support in this exercise.

For the rate of farmland loss, the estimated coefficient for the treatment indictor is -0.024 for the full sample compared to the ATTs of -0.022 to -0.031. The coefficient is -0.018 for the regression over the post-1978 sub-sample compared to the ATTs of -0.022 to -0.031. The estimated coefficient on the treatment indicator variable for acres of farmland lost is insignificant for full sample (ATTs range from -1,701 to -2,928) but significant over the post-1978 sub-sample. The estimated coefficient is -1,481 for post-1978 sub-sample compared to -1,701 to -2,995.

While on a whole, the results under both approaches are significant and similar, the differences can be explained in several ways. First, Woodridge’s approach (2002) estimates the Average Treatment Effect instead of the Average Treatment Effect *on the Treated*. Secondly, Wooldridge's approach assumes a linear relationship between the treatment indicator and the outcome variable, while PSM does not impose such a restriction.

Conclusions

Few studies have found that farmland preservation programs are having an impact on the rate of 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 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 on most variables of interest. One notable exception is the tenure variable since the percent of operators that rent part of the land they farm is consistently statistically different (although quite small 0.02) between the counties with preservation programs and those without. A high percent of operators who rent appears to increase the likelihood of a program – these could be cash grain farmers as compared to livestock or vegetable producers. A high degree of these farmers also tends to decrease the rate of loss. In previous work, both Lynch (2006) and Gardner (1994) had found that farmland preservation programs and preferential or use-value taxation had decreased the rate of loss of farms. This difference then between control and treatment counties may be tied to people retaining ownership but renting to full-time farmers.

The conclusion appears robust that agricultural preservation programs reduce the rate of farmland loss by about 2-3 percentage points for each time period for the Mid-Atlantic area. We are hopeful that we have accounted for the key variables needed to explain the existence of farmland preservation programs and farmland loss. Sensitivity analysis suggests that key characteristics that affect farmland loss would have to change a great deal (3.6-49%) to call into question the results.

Our estimate is the average impact on the treated; i.e. the impact on counties with farmland preservation program. 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 farmland could have converted to forest, tourism or recreational uses rather than residential or commercial uses. However, we believe that most counties with preservation programs were losing farmland to residential and commercial uses, thus irreversibly. Unfortunately, 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 (TDR versus PDR) result in different strategies? Similarly, one would be interested to know if the preserved land has remained in active farming and have the programs has any impact on agricultural viability?

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Figure 1: Distribution for estimated propensity scores for full sample

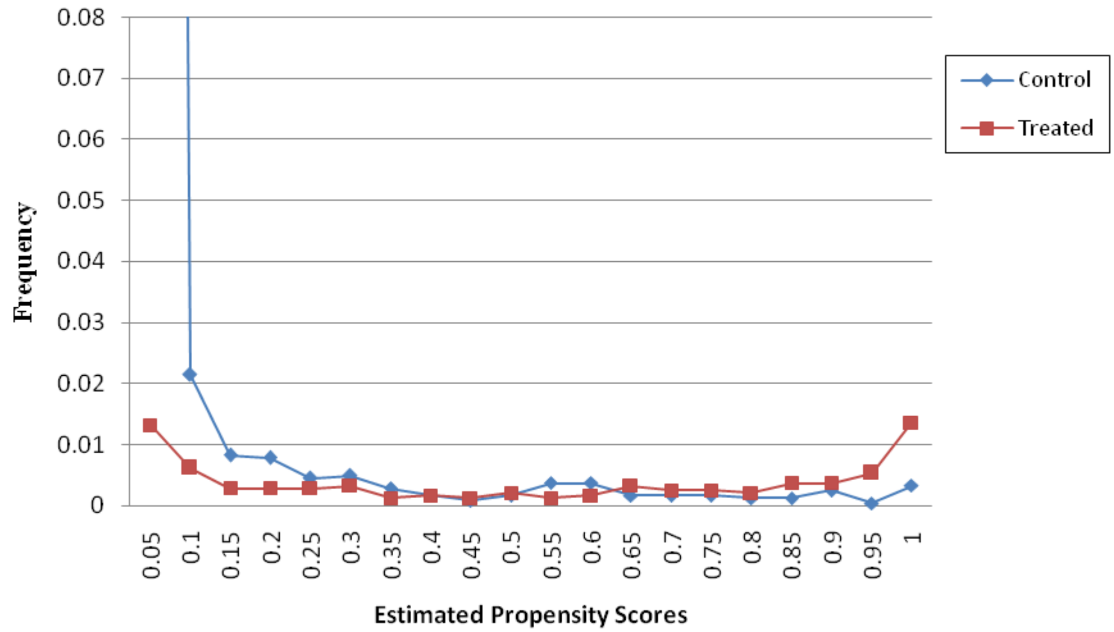

Table 1: State-level Agricultural Land Preservation Programs by 2002

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
New York	1996	1998	5,085	$ 10,886,317	$0.57
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

Maryland	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
Anne Arundel	1991	1992	8,679	$25,200,000
Baltimore	1979	1981	18,537	$51,300,000
Calvert	1978	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	
New Jersey				
Morris	1992	1996	3,835	$46,701,384
Burlington	1996		563	
New Jersey Pinelands	1981		5,722	
New York				
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
Pennsylvania				
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
Virginia				
Blackburg	1996		23	

Source: AFT 2002, 2001

Table 3.1: Descriptive Statistics by the Full Sample, Control Counties, and Treated Counties, 1949-2000 for 6 Mid-Atlantic States

		Full Sample (N=2609)		Control (N=2422)		Treated (N=187)	
Variable	Definition of Variables	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
pcfland	Percent change in farmland acres	0.0731	0.1199	0.0761	0.1222	0.0416	0.0777
cfland	Change in farmland acres	9,994	14,506	10,423	14,847	4,444	6,931
Explanatory Variables							
fland	total acres of farmland	141,756	106,982	144,169	108,803	110,501	73,023
medfinc	median family income	29,929	11,105	28,683	10,112	46,065	10,779
met	=1 if county was a metro area in 1950	0.2227	0.4162	0.2126	0.4093	0.3529	0.4792
nproffper	net profit per acre (sales minus expenses)	219. 4	1141.4	209.7	1181.2	345.1	298.97
numf	number of farms in county	979.5	894.7	994.6	906.8	783.6	692.5
pagffm	percent of residents employed in agriculture, forestry, fisheries and mining	0.0994	0.1061	0.1046	0.1081	0.0326	0.0265
pcmhval	percent change in median housing value	0.1081	0.0923	0.1105	0.0921	0.0768	0.0892
phighsch	percent of adults with a high school education	0.4778	0.1762	0.4599	0.1690	0.7092	0.074
phoffw	percent of operators working 100+ days off the farm	0.4044	0.1041	0.4023	0.1057	0.4313	0.0760
poppera	population per acre	0.5727	1.7958	0.5599	1.850	0.7389	0.7901
ppartn	percent of operators who own part but not all of the land they farm	0.2389	0.0997	0.2367	0.1017	0.268	0.062
presprog	= 1, if a county has at least one acre of farmland enrolled in farmland preservation programs	0.0717	0.258	0	0	1	0
punemp	percent unemployment	0.0549	0.0219	0.0552	0.0223	0.0516	0.0164

Source: US Census of Agriculture (1949-1997), US Census of Population and Housing (1950-2000), Personal Communication

Table 3.2: Descriptive Statistics by the Full Sample, Control Counties, and Treated Counties, 1978-2000 for 6 Mid-Atlantic States							
		Full Sample (N=1296)		Control (N=1109)		Treated (N=187)	
Variable	Definition of Variables	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
pcfland	Percent change in farmland acres	0.0344	0.1029	0.0331	0.1065	0.0416	0.0777
cfland	Change in farmland acres	4,070	8,692	4,007	8,956	4,444	6,931
Explanatory Variables							
fland	total acres of farmland	115,527	84,256	116,374	86,007	110,501	73,021
medfinc	median family income	36,983	9,138	35,452	7,863	46,065	10,779
met	=1 if county was a metro area in 1950	0.2207	0.4149	0.1984	0.399	0.3529	0.4792
nprofper	net profit per acre (sales minus expenses)	290.3	829	281	887.5	345.1	299
numf	number of farms in county	643.2	521	619.5	482.5	783.6	692.6
pagffm	percent of residents employed in agriculture, forestry, fisheries and mining	0.0545	0.0522	0.0582	0.0545	0.0326	0.0265
pcmhval	percent change in median housing value	0.099	0.0893	0.1028	0.0888	0.0768	0.0892
phighsch	percent of adults with a high school education	0.609	0.1235	0.5921	0.1222	0.7092	0.0740
phoffw	percent of operators working 100+ days off the farm	0.4345	0.0898	0.4350	0.0919	0.4313	0.076
poppera	population per acre	0.551	1.2995	0.5188	1.3645	0.739	0.7902
ppartn	percent of operators who own part but not all of the land they farm	0.291	0.0849	0.2954	0.0877	0.268	0.0616
presprog	= 1, if a county has at least one acre of farmland enrolled in farmland preservation programs	0.1443	0.3515	0	0	1	0
punemp	percent unemployment	0.0604	0.0209	0.0619	0.0212	0.0517	0.0165

Source: US Census of Agriculture (1978-1997), US Census of Population and Housing (1970-2000), Personal Communication

Table 4: Estimated Coefficients from a Random Effect Logit Model to Compute Propensity Scores

Dependent variable--presprog	Estimated Coeff.	Z-statistics
fland	0.0000624	(2.12)*
mefinc	0.001	(1.90)
pcmhval	0.212	(0.06)
phoffw	86.1	(2.15)*
phighsch	75.9	(2.14)*
ppartn	99.3	(2.36)*
punemp	786	(2.74)**
poppera	-1.00	(0.42)
numf	0.006	(2.37)*
pagffm	18.4	(0.34)
nprofper	0.006	(1.42)
met	-10.4	(1.82)
fland2	-1.44e-10	(2.11)*
medfinc2	-7.18e-9	(2.05)*
pcmhval2	-12.9	(0.94)
phoffw2	-75.9	(2.19)*
phighsch2	-35.0	(1.23)
ppartn2	-107.9	(2.33)*
punemp2	-3,945	(3.44)**
poppera2	-0.32	(0.75)
numf2	5.56e-7	(1.01)
pagffm2	-166.7	(1.13)
nprofper2	-1.35e-6	(0.67)
fland_punemp	-0.001	(3.34)**
medfinc_phoffw	0.00042	(0.85)
medfinc_punemp	-0.005	(1.34)
phoffw_phighsch	-27.5	(0.52)
phoffw_ppartn	-95.6	(1.66)
phighsch_met	13.7	(1.80)
punemp_pagffm	826.5	(1.29)
punemp_poppera	38.7	(1.24)
numf_pagffm	-0.046	(1.48)
pagffm_nprofper	0.057	(0.87)
nprofper_met	-0.004	(1.00)
tdummy7 (=1 if year=1979-1982)	-1.73	(2.76)**
tdummy8 (=1 if year=1983-1987)	-1.87	(3.04)**
tdummy9 (=1 if year=1988-1992)	-1.12	(2.24)*
Constant	-120.873	(4.85)**
Observations	2609	2609
Number of county fips code	263	263

* significant at 5%; ** significant at 1%

Table 5.1: Balancing Test for the Distribution of the Variables between Matched Treated (X_1) and Control (X_0) Groups for Observations after 1978: covariates that are not balanced*.

	Epan Kernel Matching (bandwidth =0.02)		Uniform Kernel Matching (bandwidth =0.02)	
	Full sample	Common support	Full sample	Common support
Matching over full sample				
T-test**	ppartn (0.268:0.289)	ppartn (0.271:0.289)	ppartn (0.268:0.289) tdummy7 (0.070:0.133)	ppartn (0.271:0.289)
Regression Test	ppartn Medfinc tdummy7		ppartn tdummy7	
Matching within time period				
T-test**	ppartn (0.269:0.297) punemp (0.052: 0.056) numf (763:632) met(0.329:0.21)	ppartn (0.27:0.30) punemp (0.053:0.058)	ppartn (0.269:0.297) punemp (0.052:0.056) numf (763:637) met (0.329:0.21)	ppartn (0.27:0.30) punemp (0.053:0.057)
Regression Test	ppartn numf met	ppartn medfinc	ppartn met	ppartn medfinc

* We present the covariates for which we could reject the H_0: no difference in the mean at the 95% confidence level

** the means for the treated and control group are in parentheses

Tdummy7 indicate time period 1978-1982.

Table 5.2: Balancing Test for the Distribution of the Variables between Matched Treated (X_1) and Control (X_0) Groups for Observations 1949-1997: covariates that are not balanced*.

	Epan Kernel Matching (bandwidth =0.02)		Uniform Kernel Matching (bandwidth =0.02)	
	Full sample	Common support	Full sample	Common support
Matching over full sample				
T-test**	tdummy10 (0.47:0.35)	tdummy10 (0.47:0.35)		
Regression Test	ppartn	Ppartn	ppartn	ppartn
	medfinc	Phighsch	medfinc	phighsch
	phighsch	tdummy7	phighsch	tdummy7
	tdummy7		pagffm	
			tdummy7	

* We present the covariates for which we could reject the H_0: no difference in the mean at the 95% confidence level

** the means for the treated and control group are in parenthesis

Tdummy7 indicating time period 1978-1982, tdummy10- 1992-1997 respectively.

We do not present the balancing test result for restricting matching within time period as they are the same as that for sample in the time period 1978-1997: no county has a PDR or TDR program before 1978.

Table 6.1: Average Treatment Effect on the Treated for Rate of Farmland Loss and Farmland Acres Lost during 1978-1997:
Matched over Full sample and Restricted to within Same Time Period

	Epan Kernel Matching (bandwidth =0.02)		Uniform Kernel Matching (bandwidth =0.02)	
	Full sample	Common support	Full sample	Common support
Matching over full sample				
Rate of loss				
ATT*	-0.031	-0.030	-0.027	-0.027
	(0.01)	(0.01)	(0.01)	(0.01)
Acres lost				
ATT*	-2995	-2752	-2452	-2327
	(1105)	(1007)	(1069)	(960)
Number of Matched Treated Counties	186	178	186	178
Number of Matched Control Counties	844	844	844	844
Matching within time period				
Rate of loss				
ATT*	-0.022	-0.022	-0.022	-0.022
	(0.009)	(0.01)	(0.01)	(0.01)
Acres lost				
ATT*	-1711	-1701	-1953	-1804
	(791)	(842)	(798)	(846)
Number of Matched Treated Counties	167	150	167	150
Number of Matched Control Counties	815	815	815	815

Note: *We report the Bias Corrected Average Treatment Effect.
Bias for rate of loss outcome: Epan kernel Matching using all observations, the biases for Matching over full sample and Matching within time period 0.0022 and 0.00006 respectively. For uniform kernel matching using all observations, they are 0.0004 and 0.001. For Epan kernel Matching using observations within common support, the biases for Matching over full sample and Matching within time period are 0.0018 and 0.001 respectively. For uniform kernel matching within common support, they are 0.0008 and 0.001.
Bias for acres lost outcome: For Epan kernel Matching using all observations, the biases for Matching over full sample and Matching within time period 214 and 17 respectively. For uniform kernel matching using all observations, they are 18 and 73. For Epan kernel Matching using observations within common support, the biases for Matching over full sample and Matching within time period are 220 and 82 respectively. For uniform kernel matching within common support, they are 56 and 122.

Table 6.2: Average Treatment Effect on the Treated for Rate of Farmland Loss and Farmland Acres Lost during 1949-1997:
Matched over Full sample and Restricted to within Same Time Period

	Epan Kernel Matching (bandwidth =0.02)		Uniform Kernel Matching (bandwidth =0.02)	
	Full sample	Common support	Full sample	Common support
Matching over full sample				
Rate of loss				
ATT*	-0.031	-0.030	-0.027	-0.027
	(0.01)	(0.01)	(0.01)	(0.009)
Acres lost				
ATT*	-2928	-2656	-2496	-2401
	(1077)	(992)	(1052)	(941)
Number of Matched Treated Counties	186	178	186	178
Number of Matched Control Counties	2417	2417	2417	2417
Matching within time period				
Rate of loss				
ATT*	-0.022	-0.022	-0.022	-0.022
	(0.009)	(0.01)	(0.01)	(0.01)
Acres lost				
ATT*	-1711	-1701	-1953	-1804
	(791)	(842)	(798)	(846)
Number of Matched Treated Counties	167	150	167	150
Number of Matched Control Counties	815	815	815	815

Note: *We report the Bias Corrected Average Treatment Effect in the parenthesis.
Bias for outcome as rate of loss: Epan kernel Matching using all observations, the biases for Matching over full sample and Matching within time period 0.002 and 0.00006 respectively. For uniform kernel matching using all observations, they are 0.0007 and 0.001. For Epan kernel Matching using observations within common support, the biases for Matching over full sample and Matching within time period are 0.0018 and 0.001 respectively. For uniform kernel matching within common support, they are 0.0008 and 0.001.
Bias for acres lost: For Epan kernel Matching using all observations, the biases for Matching over full sample and Matching within time period 147 and 17 respectively. For uniform kernel matching using all observations, they are 7 and 73. For Epan kernel Matching using observations within common support, the biases for Matching over full sample and Matching within time period are 123 and 82 respectively. For uniform kernel matching within common support, they are 58 and 122.

Table 7: Rosenbaum bounds and Hidden bias equivalents --Epan Kernel matching with bandwidth=0.02 and matching without restriction

	Critical P-values for Gammas				Hidden Bias Equivalents					
	Rate of farmland loss		Acres lost		Unemployment rate (%)		Total acres of farmland		Net profit per acre	
Gamma	sig+	sig-	sig+	sig-	equivalent	% to sample mean	equivalent	% to sample mean	equivalent	% to sample mean
1	0.000	0.000	0.000	0.000	0	0	0	0	0	0
1.05	0.000	0.000	0.000	0.000	0.02	0.3	1557	1.1	4.7	2.1
1.1	0.000	0.000	0.000	0.000	0.04	0.7	3063	2.2	9.2	4.2
1.15	0.000	0.000	0.000	0.000	0.05	1.0	4522	3.2	13.5	6.2
1.2	0.000	0.001	0.000	0.000	0.07	1.3	5939	4.2	17.6	8.0
1.25	0.000	0.002	0.000	0.001	0.09	1.6	7316	5.2	21.6	9.8
1.3	0.000	0.004	0.000	0.002	0.10	1.8	8656	6.1	25.4	11.6
1.35	0.000	0.008	0.000	0.003	0.12	2.1	9963	7.0	29.1	13.2
1.4	0.000	0.013	0.000	0.006	0.13	2.4	11239	7.9	32.6	14.9
1.45	0.000	0.021	0.000	0.010	0.14	2.6	12486	8.8	36.0	16.4
1.5	0.000	0.033	0.000	0.016	0.16	2.9	13707	9.7	39.3	17.9
1.55	0.000	$\boxed{0.049}$	0.000	0.025	*0.17*	*3.1*	*14902*	*10.5*	*42.5*	*19.4*
1.6	0.000	0.070	0.000	0.038	0.18	3.3	16074	11.3	45.6	20.8
1.62	0.000	0.080	0.000	$\boxed{0.044}$	*0.20*	*3.6*	*17223*	*12.2*	*48.6*	*22.2*
1.65	0.000	0.097	0.000	0.054	0.20	3.6	17223	12.2	48.6	22.2

* gamma - log odds of differential assignment due to unobserved factors
sig+ - upper bound significance level
sig- - lower bound significance level
*hidden bias equivalents are calculated at sample mean
We are not able to calcuate the hidden bias equivalence measure by percentage change in median housing value and percent of operators who own part but not all of the land they farm. The reason is that the maximum of marginal effect of the two variables on the participation and the outcome variable is bounded and lower than the effect indicated by most value of Gamma specified given our empirical specification (1.15 and 1.2 respectively).

Endnotes

[1] Although there are 50 TDR programs, only 15 of them have protected farmland.

[2] To simplify the model only two land uses are used. However, the landowner may maximize his or her present value by shifting the land use to commercial, industrial or other alternative land uses.

[3] 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.

[4] We also attempted to match within state in order to control for the heterogeneity across states. Our matching failed the balance tests for the covariates that change over time, eg pcmhval, due to small number of available control observations within some states that have state level programs. For example, all 3 out of the 3 counties in Delaware have farmland preserved by 1997, 20 out of 23 counties in Maryland have farmland preserved by 1987, 15 out of 20 counties in New Jersey have farmland preserved by 1992. The biased ATT (-0.039) from matching within state are more substantial than the estimated ATT from matching without restriction and matching within time period. It is possible after controlling for state and unbalanced covariates, we might still find a significant impact of farmland preservation on the rate of farmland loss; we just cannot definitely assign it to the farmland preservation programs.

[5] 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 a deal with non-responses, the data in 2002 were not comparable to those in 1949-50 through 1997.

[6] 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.

[7] Counties with fewer than 5 farms in 1949 were excluded from the entire analysis: Bronx, Queens, Richmond, Kings, and New York counties of New York state, and Arlington County of Virginia

[8] 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.

[9] 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.

[10] 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 (2005b) found that in some cases this test incorrectly treated matched weights as fixed rather than random. Therefore we do not use this balancing test.

[11] 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 on the treated.

[12] There are other strategies that assess the impact of hidden bias, the IV approach proposed by DiPrete and Gangl (2004) which is less conservative than Rosenbaum Bounds approach and an approach proposed by Antoji and Elder (2000) which uses the bias estimated from the observables to calculate bias from unobservable variables). We use Rosenbaum Bounds as the most appropriate for our problem.

[13] Given that fact the Rosenbaum bounds approach does not deal with stratified or cluster samples, we are unable to conduct a sensitivity analysis for our matching within time periods.